NOTICES

OF THE

PROCEEDINGS

AT THE

MEETINGS OF THE MEMBERS

OF THE

Royal Institution of Great Britain,

WITH

ABSTRACTS OF THE DISCOURSES

DELIVERED AT

THE EVENING MEETINGS.

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Royal Institution of Great Britain.

WEEKLY EVENING MEETING,

Friday, January 17, 1879.

Sir W. Frederick Pollock, Bart. M.A. Vice-President, in the Chair.

Professor Tyndall, D.C.L. LL.D. F.R.S.

The Electric Light.

The subject of this evening's discourse was proposed by our late Honorary Secretary.* That word "late" has for me its own connotations. It implies, among other things, the loss of a comrade by whose side I have worked for thirteen years. On the other hand, regret is not without its opposite in the feeling with which I have seen him rise by sheer intrinsic merit, moral and intellectual, to the highest official position which it is in the power of English science to bestow. Well, he, whose constant desire and practice were to promote the interests and extend the usefulness of this Institution, thought that at a time when the electric light occupied so much of public attention, a few sound notions regarding it, on the more purely scientific side, might, to use his own pithy expression, be "planted" in the public mind. I am here to-night with the view of trying, to the best of my ability, to realize the idea of our friend.

In the year 1800, Volta announced his immortal discovery of the pile. Whetted to eagerness by the previous conflict between him and Galvani, the scientific men of the age flung themselves with ardour upon the new discovery, repeating Volta's experiments, and extending them in many ways. The light and heat of the Voltaic circuit attracted marked attention, and in the innumerable tests and trials to which this question was subjected, the utility of platinum and charcoal, as means of exalting the light, was on all hands recognized. Mr. Children, with a battery surpassing in strength all its predecessors, fused platinum wires eighteen inches long, while "points of charcoal produced a light so vivid that the sunshine, compared with it, appeared feeble."† Such effects reached their culmination when, in 1808, through the liberality of a few members of the Royal Institution, Davy was enabled to construct a battery of 2000 pairs of plates, with which he afterwards obtained calorific and luminous effects far transcending anything previously observed. The arc of flame between

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* Mr. William Spottiswoode, now President of the Royal Society.

† Davy: 'Chemical Philosophy,' p. 110.

the carbon terminals was four inches long, and by its heat quartz, sapphire, magnesia, and lime, were melted like wax in a candle flame; while fragments of diamond and plumbago rapidly disappeared as if reduced to vapour.*

The first condition to be fulfilled in the development of heat and light by the electric current is that it shall encounter and overcome resistance. Flowing through a perfect conductor, no matter what the strength of the current might be, neither heat nor light could be developed. A rod of unresisting copper carries away uninjured and unwarmed an atmospheric discharge competent to shiver to splinters a resisting oak. I send the self-same current through a wire composed of alternate lengths of silver and platinum. The silver offers little resistance, the platinum offers much. The consequence is that the platinum is raised to a white heat, while the silver is not visibly warmed. The same holds good with regard to the carbon terminals employed for the production of the electric light. The interval between the terminals offers a powerful resistance to the passage of the current, and it is by the gathering up of the force necessary to burst across this interval that the voltaic current is able to throw the carbon into that state of violent intestine commotion which we call heat, and to which its effulgence is due.

The smallest interval of air usually suffices to stop the current. But when the carbon points are first brought together and then separated, there occurs between them a discharge of incandescent matter which carries, or may carry, the current over a considerable space. The vapours of the metals, for example, yield arcs of extraordinary length. When a pellet of silver is substituted for the positive carbon, an arc of incandescent silver vapour is obtained many times the length of that obtainable between the pure carbons. The part played by resistance is strikingly illustrated by the deportment of silver and thallium when mixed together and volatilized in the arc. The current first selects as its carrier the most volatile metal, which in this case is thallium. While it continues abundant, the passage of the current is so free—the resistance to it is so small—that the heat generated is incompetent to volatilize the silver.† As the thallium disappears the current is forced to concentrate its power; it presses the silver into its service, and finally fills the space between the carbons with a vapour which, as long as the necessary resistance is absent, it is incompetent to produce.

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* In the concluding lecture at the Royal Institution in June, 1810, Davy fused iridium, the alloy of iridium and osmium, and other refractory substances. See 'Philosophical Magazine,' vol. 35, p. 463. Quetelet assigns the first production of the spark between coal-points to Curtet in 1802. Davy, certainly, in that year showed the carbon light with a battery of 150 pairs of plates in the theatre of the Royal Institution. 'Jour. Roy. Inst.,' vol. i. p. 166.

† I have already drawn attention to a danger which besets the spectroscopist when operating upon a mixture of constituents volatile in different degrees. When, in 1872, I first observed the effect described in the text, had I not *known* that silver was present, I should have *inferred* its absence.

For seventy years, then, we have been in possession of this transcendent light without applying it to the illumination of our streets and houses. Such applications suggested themselves at the outset, but there were grave difficulties in their way. The first difficulty arose from the waste of the carbons, which are dissipated in part by ordinary combustion, and in part by the electric transfer of matter from the one carbon to the other. To keep the carbons at the proper distance asunder regulators were devised, the first of them, I believe, by Staite, and the most successful by Duboscq, Foucault, and Serrin, who have been succeeded by a multitude of other inventors, to some of whom I shall subsequently refer. By such arrangements the first difficulty was practically overcome; but the second is a graver one, being probably inseparable from the construction of the voltaic battery. It arises from the operation of that inexorable law which throughout the material universe demands an eye for an eye, and a tooth for a tooth, refusing to yield the faintest glow of heat, or glimmer of light, without the expenditure of an absolutely equal quantity of some other power. Hence, in practice, the desirability of any transformation must depend upon the value of the product in relation to that of the power expended. We could boil water by electricity, but it would not be an economical way of supplying our baths and washhouses. These considerations are now to be applied. The metal zinc can be burnt like paper; it might be ignited in a flame, but I will avoid the introduction of all foreign heat and burn the zinc in air of the temperature of this room. This is done by placing zinc foil at the focus of a concave mirror, which concentrates to a point the divergent electric beam, but which does not warm the air. The zinc burns at the focus with a violet flame, and we could readily determine the amount of heat generated by its combustion. But zinc can be burnt not only in air but in liquids. It is thus burnt when acidulated water is poured over it; it is also thus burnt in the voltaic battery. Here, however, to obtain the oxygen necessary for its combustion, the zinc has to dislodge the hydrogen with which the oxygen is combined. The consequence is that the heat due to the combustion of the metal in the liquid falls short of that developed by its combustion in air by the exact quantity necessary to separate the oxygen from the hydrogen. Fully four-fifths of the total heat are used up in this molecular work, only one-fifth remaining to warm the battery. It is upon this residue that we must now fix our attention, for it is solely out of it that we manufacture our electric light.

Before you are two small voltaic batteries of ten cells each. The two ends of one of them are united by a thick copper wire, while into the circuit of the other a thin platinum wire is introduced. The platinum glows with a white heat, while the copper wire is not sensibly warmed. Now an ounce of zinc, like an ounce of coal, produces by its complete combustion in air a constant quantity of heat. The total heat developed by an ounce of zinc through its union

with oxygen in the battery is also absolutely invariable. Let our two batteries, then, continue in action until an ounce of zinc in each of them is consumed. In the one case the heat generated is purely domestic, being liberated on the hearth where the fuel is burnt, that is to say in the cells of the battery itself. In the other case, the heat is in part domestic and in part foreign—in part within the battery and in part outside. One of the fundamental truths which our late Secretary would wish you to bear in mind is that the sum of the foreign and domestic—of the external and internal—heats is fixed and invariable. To have heat outside you must draw upon the heat within. These remarks apply to the electric light. By the intermediation of the electric current the moderate warmth of the battery is not only carried away but concentrated, so as to produce, at any distance from its origin, a heat next in order to that of the sun. The current might therefore be defined as the swift carrier of heat. Loading itself here with invisible power, by a process of transmutation which outstrips the dreams of the alchemist, it can discharge its load, in the fraction of a second, as light and heat at the opposite side of the world.

Thus, the light and heat produced outside the battery are derived from the metallic fuel burnt within the battery; and, as zinc happens to be an expensive fuel, though we have possessed the electric light for more than seventy years, it has been too costly to come into general use. But within these walls, in the autumn of 1831, Faraday discovered a new source of electricity, which we have now to investigate. On the table before me lies a coil of covered copper wire, with its ends disunited. I lift one side of the coil from the table, and in doing so exert the muscular effort necessary to overcome the simple weight of the coil. I unite its two ends and repeat the experiment. The effort now required, if accurately measured, would be found greater than before. In lifting the coil I cut the lines of the earth's magnetic force, such cutting, as proved by Faraday, being always accompanied, in a closed conductor, by the production of an "induced" electric current which, as long as the ends of the coil remained separate, had no circuit through which it could pass. The current here evoked subsides immediately as heat; this heat being the exact equivalent of the excess of effort just referred to as over and above that necessary to overcome the simple weight of the coil. When the coil is liberated it falls back to the table, and when its ends are united it encounters a resistance over and above that of the air. It generates an electric current opposed in direction to the first, and reaches the table with a diminished shock. The amount of the diminution is accurately represented by the warmth which the momentary current develops in the coil. Various devices have been employed to exalt these induced currents. Faraday, indeed, foresaw that such attempts were sure to be made; but he chose to leave them in the hands of the mechanician, while he himself pursued the deeper study of facts and principles. "I have rather," he writes in 1831, "been desirous of discovering new

facts and new relations dependent on magneto-electric induction than of exalting the force of those already obtained; being assured that the latter would find their full development hereafter."

This fuller development was aimed at by Pixii, Clark, Saxton, and others, who caused magnets to rotate near coils of wire, or coils of wire surrounding iron cores to rotate near the poles of powerful steel magnets. The presence of the iron cores, as shown by Faraday, greatly intensified the action, the play of which was this:—When the coils approached the poles of the permanent magnets, currents were excited in one direction: when they retreated from the poles, currents were excited in the opposite direction. On passing a pole, therefore, a reversal of the current always occurred in the coil. To gather up these opposing currents, and send them in a common direction, an arrangement called a commutator was associated with the magneto-electric machine. I have here a model of an old Saxton machine with which Faraday used to illustrate what were then considered the larger phenomena of induced currents. It was considered a great result when half an inch of exceedingly thin platinum wire, inclosed in a glass tube to protect it from air currents, was caused to glow. In 1853 I had the pleasure of witnessing, in company with M. Biot and Professor Magnus, the performance of the first Ruhmkorff's machine, which you know is a generator of Faraday's electricity, and I well remember the ecstasy and surprise of the grand old man, evoked by effects which we should now deem utterly insignificant. Thus science grows. Forgetting, as it were, the things which are behind, it reaches ever forward to the things which are before. In connection with the development of the Ruhmkorff coil, besides Ruhmkorff himself, Apps in this country, and Ritchie in America, are especially deserving of honourable mention.

For more than twenty years magneto-electricity had subserved its first and noblest purpose of augmenting our knowledge of the powers of nature. It had been discovered and applied to intellectual ends, its application to practical ends being still to be realized. The Drummond light had raised thoughts and hopes of vast improvements in public illumination. Many inventors tried to obtain it cheaply; and in 1853 an attempt was made to organize a company in Paris for the purpose of procuring, through the decomposition of water by a powerful magneto-electric machine constructed by M. Nollet, the oxygen and hydrogen necessary for the lime light. The experiment failed, but the apparatus by which it was attempted suggested to Mr. Holmes other and more hopeful applications. Abandoning the attempt to produce the lime light, with persevering skill Holmes continued to improve the apparatus and to augment its power, until it was finally able to yield a magneto-electric light comparable to that of the voltaic battery. Judged by later knowledge, this first machine would be considered cumbrous and defective in the extreme; but judged by the light of antecedent events, it marked a great step forward.

Faraday was profoundly interested in the growth of his own

discovery. The Elder Brethren of the Trinity House had had the wisdom to make him their "Scientific Adviser;" and it is interesting to notice in his reports regarding the light the mixture of enthusiasm and caution which characterized him. Enthusiasm was with him a motive power, guided and controlled by a disciplined judgment. He rode it as a charger, holding it in by a strong rein. While dealing with Holmes, he states the case of the light *pro* and *con.* He checks the ardour of the inventor, and, as regards cost, rejecting sanguine estimates, he insists over and over again on the necessity of continued experiment for the solution of this important question. His matured opinion was however strongly in favour of the light. With reference to an experiment made at the South Foreland on the 20th of April, 1859, he thus expresses himself:—"The beauty of the light was wonderful. At a mile off, the apparent streams of light issuing from the lantern were twice as long as those from the lower lighthouse, and apparently three or four times as bright. The horizontal plane in which they chiefly took their way made all above or below it black. The tops of the hills, the churches, and the houses illuminated by it were striking in their effect upon the eye." Further on in his report he expresses himself thus:—"In fulfilment of this part of my duty, I beg to state that, in my opinion, Professor Holmes has practically established the fitness and sufficiency of the magneto-electric light for lighthouse purposes, so far as its nature and management are concerned. The light produced is powerful beyond any other that I have yet seen so applied, and in principle may be accumulated to any degree; its regularity in the lantern is great; its management easy, and its care there may be confided to attentive keepers of the ordinary degree of intellect and knowledge." Finally, as regards the conduct of Professor Holmes during these memorable experiments, it is only fair to add the following remark with which Faraday closes the report submitted to the Elder Brethren of the Trinity House on the 29th of April, 1859:—"I must bear my testimony," he says, "to the perfect openness, candour, and honour of Professor Holmes. He has answered every question, concealed no weak point, explained every applied principle, given every reason for a change either in this or that direction, during several periods of close questioning, in a manner that was very agreeable to me, whose duty it was to search for real faults or possible objections, in respect both of the present time and the future."

Soon afterwards, the Elder Brethren of the Trinity House had the intelligent courage to establish the machines of Holmes permanently at Dungeness, where the magneto-electric light continued to shine for many years.

The magneto-electric machine of the Alliance Company soon succeeded to that of Holmes, being in various ways a very marked improvement on the latter. Its currents were stronger and its light was brighter than those of its predecessor. In it, moreover, the commutator, the flashing and destruction of which were sources of

irregularity and deterioration in the machine of Holmes, was, at the suggestion of M. Masson,* entirely abandoned; alternating currents instead of the direct current being employed. M. Serrin modified his excellent lamp with the express view of enabling it to cope with alternating currents. During the International Exhibition of 1862, where the machine was shown, M. Berlioz offered to dispose of the invention to the Elder Brethren of the Trinity House. They referred the matter to Faraday, and he replied as follows: "I am not aware that the Trinity House authorities have advanced so far as to be able to decide whether they will require more magneto-electric machines, or whether, if they should require them, they see reason to suppose the means of their supply in this country, from the source already open to them, would not be sufficient. Therefore I do not see that at present they want to purchase a machine." Faraday was obviously swayed by the desire to protect the interests of Holmes, who had borne the burden and heat which fall upon the pioneer. The Alliance machines were introduced with success at Cape la Hève, near Havre; and the Elder Brethren of the Trinity House, determined to have the best available apparatus, decided, in 1868, on the introduction of machines on the Alliance principle into the lighthouses at Souter Point and the South Foreland. These machines were constructed by Professor Holmes, and they still continue in operation.†

As their present scientific adviser, the Elder Brethren did me the honour of asking my opinion as to the course which they proposed to pursue with regard to the introduction of these new machines. That opinion is expressed in the following extract from a report dated May 16th, 1868: "There is no doubt that electricity places at the disposal of the Elder Brethren a source of light next to the sun itself in power, and far transcending any light obtainable from the combustion of oil. With regard to the practical application of the magneto-electric light, the question, in my opinion, has been solved by the performance of the machine at Dungeness. That machine was one of the first, if not the very first, constructed with a view to lighthouse illumination. Defects inherent in first constructions were associated with the machine. If, notwithstanding these defects, some of which were very grave, the interruptions have been so few, it may be safely inferred that with our augmented experience, and with the improved apparatus now within our reach, the performance of the magneto-electric machine may be rendered practically perfect. It is with the profound conviction that the decision is a wise one that I learn the intention of the Elder

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* Du Moncel, 'l'Électricité,' Aug. 1878, p. 150.

† The photometric values of the lights produced by these machines were determined by Mr. Douglass, whose measurements showed that they fell far below the specified power. A new and very powerful machine being imported from Paris, Mr. Holmes was required so to strengthen his magnets as to make his machines equal to those of the "Alliance."

Brethren to introduce this powerful source of illumination with all its recent improvements at certain prominent points on the coast of England." With regard to the application of electricity to lighthouse purposes, the course of events was this: The Dungeness light was introduced on January 31, 1862; the light at La Hève on December 26, 1863, or nearly two years later. But Faraday's experimental trial at the South Foreland preceded the lighting of Dungeness by more than two years. The electric light was afterwards established at Cape Grisnez. It was started at Souter Point on January 11, 1871; and at the South Foreland on January 1, 1872. At the Lizard, which probably enjoys the newest and most powerful development of the electric light, it began to shine on January 1, 1878.

I have now to revert to a point of apparently small moment, but which really constitutes an important step in the development of this subject. I refer to the form given to the rotating armature in 1857 by Dr. Werner Siemens, of Berlin. Instead of employing coils wound transversely round cores of iron, as in the machine of Saxton, Siemens, after giving a bar of iron the proper shape, wound his wire longitudinally round it, and obtained thereby greatly augmented effects between suitably placed magnetic poles. Such an armature is employed in the small magneto-electric machine which I now introduce to your notice, and for which the Institution is indebted to Mr. Henry Wilde, of Manchester. There are here sixteen permanent horse-shoe magnets placed parallel to each other, and between their poles a Siemens' armature. The two ends of the wire which surrounds the armature are now disconnected. In turning the handle and causing the armature to rotate, I simply overcome ordinary mechanical friction. But the two ends of the armature coil can be united in a moment, and when this is done, I immediately experience a greatly increased resistance to rotation. Something over and above the ordinary friction of the machine is now to be overcome, and by the expenditure of an additional amount of muscular force I am able to overcome it. The excess of labour thus thrown upon my arm has its exact equivalent in the electric currents generated, and the heat produced by their subsidence in the coil of the armature. A portion of this heat may be rendered visible by connecting the two ends of the coil with a thin platinum wire. When the handle of the machine is rapidly turned the wire glows, first with a red heat, then with a white heat, and finally with the heat of fusion. The moment the wire melts, the circuit round the armature is broken, an instant relief from the labour thrown upon the arm being the consequence. Clearly realize, I beg of you, the equivalent of the light here developed. During the period of turning the machine a certain amount of combustible substance was oxidized or burnt in the muscles of my arm. Had it done no external work, the matter consumed would have produced a definite amount of heat. Now, the muscular heat actually developed during the rotation of the machine

fell short of this definite amount, the missing heat being reproduced to the last fraction in the glowing platinum wire and the other parts of the machine. Here, then, the electric current intervenes between my muscles and the generated heat, exactly as it did a moment ago between the voltaic battery and its generated heat. The electric current is to all intents and purposes a vehicle which transports the heat both of muscle and battery to any distance from the hearth where the fuel is consumed. Not only is the current a messenger, but it is also an intensifier of magical power. The temperature of my arm is, in round numbers, 100° Fahr., and it is by the intensification of this heat that one of the most refractory of metals, which requires a heat of 3600° Fahr. to fuse it, has been reduced to the molten condition.

Zinc, as I have said, is a fuel far too expensive to permit of the electric light produced by its combustion being used for the common purposes of life, and you will readily perceive in reference to our last experiment that the human muscles, or even the muscles of a horse, would be also very expensive. Here, however, we can employ the force of burning coal to turn our machine, and it is this employment of our cheapest fuel, rendered possible by Faraday's discovery, which opens out to us the prospect of being able to apply the electric light to public use.

In 1866 a great step in the intensification of induced currents, and the consequent augmentation of the magneto-electric light, was taken by Mr. Henry Wilde. It fell to my lot to report upon them to the Royal Society, but before doing so I took the trouble of going to Manchester to witness Mr. Wilde's experiments. He operated in this way, starting from a small machine like that worked in your presence a moment ago, he employed its current to excite an electro-magnet of a peculiar shape, between whose poles rotated a Siemens' armature;* from this armature currents were obtained vastly stronger than those generated by the small magneto-electric machine. These currents might have been immediately employed to produce the electric light; but instead of this they were conducted round a second electro-magnet of vast size, between whose poles rotated a Siemens' armature of corresponding dimensions. Three armatures therefore were involved in this series of operations; first, the armature of the small magneto-electric machine; secondly, the armature of the first electro-magnet, which was of considerable size; and thirdly, the armature of the second electro-magnet, which was of vast dimensions. With the currents drawn from this third armature Mr. Wilde obtained effects, both as regards heat and light, enormously transcending those previously known.†

* Page and Moigno had previously shown that the magneto-electric current could produce powerful electro-magnets.

† Mr. Wilde's paper, communicated by Faraday, was received by the Royal Society, March 26th, and read April 26th, 1866. It is published in the 'Philosophical Transactions' for 1867, p. 89. My opinion regarding Wilde's machine

But the discovery which, above all others, brought the practical question to the front is now to be considered. On the 4th of February, 1867, a paper was received by the Royal Society from Dr. William Siemens bearing the title, "On the conversion of Dynamic into Electrical Force without the use of Permanent Magnetism." * On the 14th of February a paper from Sir Charles Wheatstone was received, bearing the title, "On the augmentation of the Power of a Magnet by the reaction thereon of Currents induced by the Magnet itself." Both papers, which dealt with the same discovery, and which were illustrated by experiments, were read upon the same night, viz. on the 14th of February. It would be difficult to find in the whole field of science a more beautiful example of the interaction of natural forces than that set forth in these two papers. You can hardly find a bit of iron—you can hardly pick up an old horse-shoe, for example—that does not possess a trace of permanent magnetism; and from such small beginnings Siemens and Wheatstone have taught us to rise by a series of interactions between magnet and armature to a magnetic intensity previously unapproached. Conceive the Siemens' armature placed between the poles of a suitable electro-magnet. Suppose this latter to possess at starting the faintest trace of magnetism; then when the armature rotates, currents of infinitesimal strength are generated in its coil. Let the ends of that coil be connected with the wire surrounding the electro-magnet. The infinitesimal current generated in the armature will then circulate round the magnet, augmenting its intensity by an infinitesimal amount. The strengthened magnet instantly reacts upon the coil which feeds it, producing a current of greater strength. This current again passes round the magnet, which immediately brings its enhanced power to bear upon the coil. By this play of mutual give and take between magnet and armature, the strength of the former is raised in a very brief interval from almost nothing to complete magnetic saturation. Such a magnet and armature are able to produce currents

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was briefly expressed in a report to the Elder Brethren of the Trinity House on the 17th of May, 1866: "It gives me pleasure to state that the machine is exceedingly effective, and that it far transcends in power all other apparatus of the kind."

* A paper on the same subject, by Dr. Werner Siemens, was read on the 17th of January, 1867, before the Academy of Sciences in Berlin. In a letter to 'Engineering,' No. 622, p. 45, Mr. Robert Sabine states that Professor Wheatstone's machines were constructed by Mr. Stroh in the months of July and August, 1866. I do not doubt Mr. Sabine's statement; still it would be dangerous in the highest degree to depart from the canon, in asserting which Faraday was specially strenuous, that the date of a discovery is the date of its publication. Towards the end of December, 1866, Mr. Alfred Varley also lodged a provisional specification (which, I believe, is a sealed document) embodying the principles of the dynamo-electric machine, but some years elapsed before he made anything public. His brother, Mr Cromwell Varley, when writing on this subject in 1867, does not mention him ('Proc. Roy. Soc.,' March 14, 1867). It probably marks a national trait that sealed communications, though allowed in France, have never been recognized by the scientific societies of England.

of extraordinary power, and if an electric lamp be introduced into the common circuit of magnet and armature, we can readily obtain a most powerful light.* By this discovery, then, we are enabled to avoid the trouble and expense involved in the employment of permanent magnets; we are also enabled to drop the exciting magneto-electric machine, and the duplication of the electro-magnets. By it, in short, the electric generator is so far simplified, and reduced in cost, as to enable electricity to enter the lists as the rival of our present means of illumination.

Soon after the announcement of their discovery by Siemens and Wheatstone, Mr. Holmes, at the instance of the Elder Brethren of the Trinity House, endeavoured to turn the discovery to account. Already, in the spring of 1869, he had constructed a machine which, though hampered with defects, exhibited extraordinary power. The light was developed in the focus of a dioptric apparatus placed on the Trinity Wharf at Blackwall, and witnessed by the Elder Brethren, their engineer, and myself, from an observatory at Charlton, on the opposite side of the Thames. Falling upon the suspended haze, the light illuminated the atmosphere for miles all round. Anything so sun-like in splendour had not, I imagine, previously been witnessed. The labour necessary to bring a machine of the kind to perfection was then strikingly illustrated. It required a year of work after its first successful performance to render the action of the machine secure. There were ten electro-magnets and twenty helices in operation, four of the latter being used to excite the electro-magnets, and the remaining sixteen to develop the currents used for the light. When thrown into action the strain produced by the mutual attraction of the poles was so great as to endanger the stability of the machine, and to lessen this defect it was many times taken asunder and constructed anew. The machine was subjected to very severe scrutiny at Blackwall, Mr. Ayres watching it constantly day and night during a considerable number of trials. .Defects were revealed and removed, the final result being expressed in the following brief extract from a long report which I had the honour of submitting to the Elder Brethren February 21, 1870: "I think the experiments prove that with a due and by no means excessive amount of care, the dynamo-electric engine of Mr. Holmes may be worked in a satisfactory manner. With regard to the stability of the internal portions of the machine, it is rather a question for your engineer (Mr. Douglass) than for me. The strains and pressures within the machine may be very great, and may require a corresponding strength of construction to cope with them. Indeed this is the reason why the machine has been so often taken asunder. Mr. Holmes seems to have spared no pains to render his work secure, and no sign of weakness has, to my knowledge, manifested itself during the late trials."

* In 1867 Mr. Ladd introduced the modification of dividing the armature into two separate coils, one of which fed the electro-magnets, while the other yielded the induced currents.

As regards lighthouse illumination, the next step forward was taken by the Elder Brethren of the Trinity House in 1876–77. Having previously decided on the establishment of the electric light at the Lizard in Cornwall, they instituted at the time referred to, an elaborate series of comparative experiments wherein the machines of Holmes, of the Alliance Company, of Siemens, and of Gramme, were pitted against each other. The Siemens and the Gramme machines delivered direct currents, while those of Holmes and the Alliance Company delivered alternating currents. The light of the latter was of the same intensity in all azimuths; that of the former was different in different azimuths, the discharge being so regulated as to yield a gush of light of special intensity in one direction. The following table gives in standard candles the performance of the respective machines:—

Names of Machines.	Maximum.	Minimum.
Holmes	1523	1523
Alliance	1953	1953
Gramme (No. 1)	6663	4016
Gramme (No. 2)	6663	4016
Siemens (Large)	14818	8932
Siemens (Small, No. 1)	5539	3339
Siemens (Small, No. 2)	6864	4138
Two Holmes's coupled	2811	2811
Two Gramme's (Nos. 1 and 2)	11396	6869
Two Siemens' (Nos. 1 and 2)	14134	8520

These determinations were made by Mr. Douglass the engineer-in-chief, and Mr. Ayres the assistant engineer of the Trinity House. It is practically impossible to compare photometrically and directly the flame of a candle with these sun-like lights. A light of intermediate intensity—that of the six-wick Trinity oil lamp—was therefore, in the first instance, compared with the electric light. The candle power of the oil lamp being afterwards determined, the intensity of the electric light became known. The numbers given in the table prove the superiority of the Alliance machine over that of Holmes. They prove, for the resistances involved, the great superiority both of the Gramme machine and of the small Siemens machine over the Alliance, while the large Siemens machine is shown to yield a light far exceeding all the others. The coupling of two Grammes, or of two Siemens together, which was first successfully accomplished at the South Foreland, was followed by a very great augmentation of the light, rising in the one case from 6663 candles to 11,396, and in the other case from 6864 candles to 14,134. After this contest, which was conducted throughout in the most amicable manner, Siemens machines of the smaller type were chosen for the Lizard.*

We have machines capable of sustaining a single light and also

* As the result of a recent trial by Mr. Schwendler, they have been also chosen for India.

machines capable of sustaining several lights. The Gramme machine, for example, which ignites the Jablochkoff candles on the Thames Embankment and at the Holborn Viaduct, delivers four currents, each flowing through its own circuit. In each circuit are five lamps through which the current belonging to the circuit passes in succession. The lights correspond to so many resisting spaces, over which, as already explained, the current has to leap; the force which accomplishes the leap being that which produces the light. Whether the current is to be competent to pass through five lamps in succession, or to sustain only a single lamp, depends entirely upon the will and skill of the maker of the machine. He has, to guide him, definite laws laid down half a century ago, by which he must abide.

Ohm has taught us how to arrange the elements of our battery so as to augment indefinitely its electro-motive force. We have only to link its cells together so that the current generated by each shall pass through all the others, and add its electro-motive force to that of all the others. We increase, it is true, at the same time the resistance of the battery, diminishing thereby the quantity of the current from each cell, but we augment the power of the integrated current to overcome external hindrances. The battery resistance may, indeed, be rendered so great that the external resistance shall vanish in comparison. What is here said regarding the voltaic battery is equally true of magneto-electric machines. If we wish our current to leap over five intervals, and produce five lights in succession, we must invoke a sufficient electro-motive force. This is done simply by multiplying, by the use of thin wire, the convolutions of the rotating armature as, a moment ago, we augmented the cells of our voltaic battery. Each additional convolution, like each additional cell, adds its electro-motive force to that of all the others; and though it also adds its resistance, thereby diminishing the quantity of current contributed by each convolution, the integrated current becomes endowed with the power of leaping across the successive spaces necessary for the production of a series of lights in its course. The machines, on the other hand, which produce only a single light have a small internal resistance associated with a small electro-motive force. In such machines the wire of the rotating armature is comparatively short and thick, copper riband instead of wire being sometimes employed. Such machines deliver a large quantity of electricity of low tension—in other words, of low leaping power. Hence, though competent, when their power is converged upon a single interval, to produce one splendid light, their currents are unable to force a passage when the number of intervals is increased. Thus, by augmenting the convolutions of our machines we sacrifice quantity and gain electro-motive force; while by lessening the number of the convolutions, we sacrifice electro-motive force and gain quantity. Whether we ought to choose the one form of machine or the other depends entirely upon the external work it has to perform. If the object be to obtain a single light of great splendour, machines of low

resistance and large quantity must be employed. If we want to obtain in the same circuit several lights of moderate intensity, machines of high internal resistance and of correspondingly high electro-motive power, must be invoked.

When a coil of covered wire surrounds a bar of iron, the two ends of the coil being connected together, every alteration of the magnetism of the bar is accompanied by the development of an induced current in the coil. The current is only excited during the period of magnetic change. No matter how strong or how weak the magnetism of the bar may be, as long as its condition remains permanent no current is developed. Conceive the pole of a magnet placed near one end of the bar to be moved along it to the other end. During the time of the pole's motion there will be an incessant change in the magnetism of the bar, and accompanying this change we shall have an induced current in the surrounding coil. If, instead of moving the magnet, we move the bar and its surrounding coil past the magnetic pole, a similar alteration of the magnetism of the bar will occur, and a similar current will be induced in the coil.

You have here the fundamental conception of M. Gramme which led to the construction of his beautiful machine.* He aimed at giving continuous motion to such a bar as we have here described, and for this purpose he bent it into a continuous ring. By a suitable mechanism he caused the various parts of the ring to pass in succession close to the poles of a horse-shoe magnet. The direction of the current varies with the motion, and with the character of the influencing pole, the result being that the currents in the two semicircles of the coil surrounding the ring flow in opposite directions. But it is easy by a suitable mechanical arrangement to conduct them away from the places where they meet, and to cause them to flow in the same direction. The first machines of Gramme, therefore, furnished *direct* currents, similar to those yielded by the voltaic pile. M. Gramme subsequently so modified his machine as to produce alternating currents. Such alternating machines are employed to produce the lights now exhibited on the Holborn Viaduct and the Thames Embankment.

Another machine of great alleged merit is that of M. Lontin. It resembles in shape a toothed iron wheel, the teeth being used as cores round which are wound coils of copper wire. The wheel is caused to rotate between the poles of powerful electro-magnets. On passing each pole the core or tooth is strongly magnetized, and instantly evokes in the surrounding coil an induced current of corresponding strength. The currents excited in approaching and retreating, and in passing different poles flow in opposite directions, but by means of a commutator these conflicting electric

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* 'Comptes Rendus,' 1871, p. 176. See also Gaugain on the Gramme machine, 'Ann. de Chem. et de Phys.,' vol. xxviii. p. 324.

streams are gathered up and caused to flow in a common bed. The bobbins in which the currents are induced can be so augmented in number as to augment indefinitely the power of the machine. A series of toothed wheels, for example, may be fixed on the same axle, each wheel and its bobbins rotating between their own pair of electro-magnetic poles. In the larger machines of M. Lontin, the ends of the iron teeth which constitute the cores face corresponding helices and cores fixed to an exterior iron ring. In this disposition the bobbins of the rotating wheel are the electro-magnets, while the bobbins attached to the ring are those from which the induced currents are drawn. By coupling the bobbins together in various ways they can be united so as to furnish a single current, or to furnish a number of distinct currents which may be regarded as fractions of the whole current. To excite his electro-magnets, M. Lontin applies the principle of Mr. Wilde. A small machine furnishes a direct current, which he carries round the electro-magnets of a second and larger machine. Wilde's principle, it may be added, is also applied on the Thames Embankment and the Holborn Viaduct; a small Gramme machine being used in each case to excite the electro-magnets of the large one.

The Farmer-Wallace machine is also an apparatus of great power. It consists of a combination of bobbins for induced currents, and of inducing electro-magnets. The latter are excited by the method discovered by Siemens and Wheatstone. In the machines intended for the production of the electric light, the electro-motive force is so great as to permit of the introduction of several lights in the same circuit. A peculiarly novel feature of the Farmer-Wallace system is the shape of the carbons. Instead of rods large plates of carbon, with bevelled edges, are placed one above the other. The electric discharge passes from edge to edge, and shifts its position according as the carbon is dissipated. The plates are kept at the proper distance apart by an automatic electro-magnetic arrangement. The duration of the light in this case far exceeds that obtainable with rods. I have myself seen four of these lights in the same circuit in Mr. Ladd's workshop in the City, and they are now, I believe, employed at the Liverpool Street Station of the Metropolitan Railway. The Farmer-Wallace "quantity machine" pours forth a flood of electricity of low tension. While unable to cross the interval necessary for the production of a single electric light, it can fuse thick copper wires. When the current is sent through a short bar of iridium, this refractory metal emits a light of extraordinary splendour.*

The machine of M. de Méritens, which he has generously brought over from Paris for our instruction, is the newest of all. In its construction he falls back upon the principle of the magneto-electric machine, employing permanent magnets as the exciters of the induced

* The iridium light was shown by Mr. Ladd. It brilliantly illuminated the theatre of the Royal Institution.

currents. Using the magnets of the Alliance Company, by a skilful disposition of his bobbins, M. de Méritens produces with eight magnets a light equal to that produced by forty magnets in the Alliance machines. While the space occupied is only one-fifth, the cost is little more than one-fourth that of the latter. In the de Méritens machine the commutator is abolished. The internal heat is hardly sensible, and the absorption of power, in relation to the effects produced, is small. With his larger machines M. de Méritens maintains a considerable number of lights in the same circuit.*

In relation to this subject inventors fall into two classes, the contrivers of regulators and the constructors of machines. To the mechanicians of the former class already mentioned may be added Browning, Siemens, Carré, Gramme, Lontin, Achereau, &c. M. Rapieff has hitherto belonged to the inventors of regulators, but I have reason to know that he is engaged on a machine which when complete will place him in the other class also. Instead of two single carbon rods, M. Rapieff employs two pairs of rods, each pair forming a V. The light is produced at the common junction of the four carbons. The device for regulating the light is of the simplest character. At the bottom of the stand which supports the carbons are two small electro-magnets. One of them, when the current passes, draws the carbons together, and in so doing throws itself out of circuit, leaving the control of the light to the other. The carbons are caused to approach each other by a descending weight which acts in conjunction with the electro-magnet. Through the liberality of the proprietors of the *Times* every facility has been given to M. Rapieff to develop his invention at Printing House Square. The illumination of the press-room, which I had the pleasure of witnessing, under the guidance of M. Rapieff himself, is extremely effectual and agreeable to the eye. There are, I believe, five lamps in the same circuit, and the regulators are so devised that the extinction of any lamp does not compromise the action of the others.

Many other inventors might here be named, and fresh ones are daily crowding in. Mr. Werdermann has been long known in connection with this subject. Employing as negative carbon a disc, and as positive carbon a rod, he has, I am assured, obtained very satisfactory results. The small resistances brought into play by his minute arcs enable Mr. Werdermann to introduce a number of lamps into a circuit traversed by a current of only moderate electro-motive power. M. Reynier is also the inventor of a very beautiful little lamp, in which the point of a thin carbon rod, properly adjusted, is caused to touch the circumference of a carbon wheel which rotates underneath the point. The light is developed at the place of contact of rod and

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* The small machine transforms one-and-a-quarter horse-power into heat and light, yielding about 1900 candles; the large machine transforms five horse-power, yielding about 9000 candles.

wheel. Again the positive carbon wastes more profusely than the negative, and this is alleged to be due to the greater heat of the former. It occurred to Mr. William Siemens to chill the negative artificially, with the view of diminishing or wholly preventing its waste. This he accomplishes by making the negative a hollow cone of copper, and by ingeniously discharging cold water against the interior of the cone. His negative copper is thus caused to remain fixed in space, for it is not dissipated, the positive carbon only needing control. I have seen this lamp in action and can bear witness to its success.

There is something bewildering in the recent rush of constructive talent into this domain of applied electricity. The question and its prospects are modified from day to day, a steady advance being made towards the improvement both of machines and regulators. With regard to our squares, quays, esplanades, public halls, and other similar places, I strongly lean to the opinion that the electric light will finally triumph over gas. I am not so sure that it will do so in our private homes. As, however, I am anxious to avoid dropping a word here that could influence the share market in the slightest degree, I limit myself to this general statement of opinion.

To one inventor, in particular, belongs the honour of the idea, and the realization of the idea, of causing the carbon rods to burn away like a candle. It is needless for me to say that I here refer to the young Russian officer, M. Jablochkoff. He sets two carbon rods upright at a small distance apart, and fills the space between them with an insulating substance like plaster of Paris. The carbon rods are fixed in metallic holders, by one of which the current arrives, and by the other of which it passes away. A momentary contact is established between the two carbons by a little cross-piece of the same substance placed horizontally from top to top. This cross-piece is immediately dissipated or removed by the current, the passage of which once established is afterwards maintained. The carbons gradually waste, while the substance between them melts like the wax of a candle. The comparison, however, only holds good for the act of melting; for, as regards the current, the insulating plaster is practically inert. Indeed, as proved by M. Rapieff and Mr. Wilde, the plaster may be dispensed with altogether, the current passing from point to point between the naked carbons. M. de Méritens has recently brought out a new candle, in which the plaster is abandoned, while between the two principal carbons is placed a third insulated rod of the same material. With the small de Méritens machine two of these candles can be lighted before you; they produce a very brilliant effect.* In the Jablochkoff candle it is necessary that the carbons should be consumed at the same rate. Hence the necessity

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* Both the machines of M. de Méritens and the Farmer-Wallace machine were worked by an excellent gas-engine, lent for the occasion by the Messrs. Crossley, of Manchester. The Siemens machine was worked by steam.

for alternating currents by which this equal consumption is secured. It will be seen that M. Jablochkoff has abolished regulators altogether, introducing the candle principle in their stead. In my judgment the performance of the Jablochkoff candle on the Thames Embankment and the Holborn Viaduct is highly creditable, notwithstanding a considerable waste of light towards the sky. The Jablochkoff lamps, it may be added, would be more effective in a street, where their light would be scattered abroad by the adjacent houses, than in the positions which they now occupy in London.

It was my custom some years ago, whenever I needed a new and complicated instrument, to sit down beside its proposed constructor, and to talk the matter over with him. The study of the inventor's mind which this habit opened out was always of the highest interest to me. I particularly well remember the impression made upon me on such occasions by the late Mr. Darker, a philosophical instrument maker in Lambeth. This man's life was a struggle, and the reason of it was not far to seek. No matter how commercially lucrative the work upon which he was engaged might be, he would instantly turn aside from it to seize and realize the ideas of a scientific man. He had an inventor's power, and an inventor's delight in its exercise. The late Mr. Becker possessed the same power in a very considerable degree. On the Continent, Froment, Breguet, Sauerwald, and others might be mentioned as eminent instances of ability of this kind. Such minds resemble a liquid on the point of crystallization. Stirred by a hint, crystals of constructive thought immediately shoot through them. That Mr. Edison possesses this intuitive power in no common measure is proved by what he has already accomplished. He has the penetration to seize the relationship of facts and principles, and the art to reduce them to novel and concrete combinations. Hence an adverse opinion as to his ability to solve the complicated problem on which he is now engaged would be unwarranted. It is purely a case, not for the discovery of new facts and principles, but for the exercise of mechanical ingenuity in turning to a special account facts and principles already familiar to the scientific man.

I will endeavour to illustrate in a simple manner Mr. Edison's alleged mode of electric illumination, taking advantage of what Ohm has taught us regarding the laws of the current, and what Joule has taught us regarding the relation of resistance to the development of light and heat. From one end of a voltaic battery runs a wire dividing at a certain point into two branches which re-unite in a single wire connected with the other end of the battery. From the positive end of the battery the current passes first through the single wire to the point of junction, where it divides itself between the branches according to a well-known law. If the branches be equally resistant the current divides itself equally between them. If one branch be less resistant than the other, more than half of the current will choose the freer path. The strict law is that the quantity of

current is inversely proportional to the resistance. A clear image of the process is derived from the deportment of water. When a river meets an island it divides, passing right and left of the obstacle and afterwards reuniting. If the two branch beds be equal in depth, width, and inclination, the water will divide itself equally between them. If they be unequal, the larger quantity of water will flow through the more open course. Detaching one of these branch wires, I send the whole current from our battery through the other, in which a spiral of platinum wire is introduced. The spiral glows brightly. I now connect the second branch, which also contains its spiral. The current divides, and the consequence is that the first spiral falls while the second rises in illumination. Augmenting the resistance of either branch, an additional portion of the current is thrown upon the other, increasing the light of its spiral. Introducing, instead of either spiral, a piece of thick copper wire, nearly the whole of the current passes through it, the glow of the spiral in the other branch falling to darkness. And as in the case of the water we may have an indefinite number of islands producing an indefinite subdivision of the trunk stream, so in the case of electricity we may have instead of two branches any number of branches, the current dividing itself among them in accordance with the law which fixes the relation of current to resistance.

Let us apply this knowledge. Suppose an insulated copper rod, which we may call an "electric main," to be laid down along one of our streets, say along the Strand. Let this rod be connected with one end of a powerful Voltaic battery, a good metallic connection being established between the other end of the battery and the gas-pipes under the street. As long as the electric main continues unconnected with the gas-pipes the circuit is incomplete and no current will flow; but if any part of the main, however distant from the battery, be connected with the adjacent gas-pipes, the circuit will be completed and the current will flow. Supposing our battery to be at Charing Cross, and our rod of copper to be tapped opposite Somerset House, a branch wire can be carried from the rod into the building, the current passing through which may be subdivided into any number of subordinate branches which reunite afterwards and return through the gas-pipes to the battery. The branch currents may be employed to raise to vivid incandescence a refractory metal like iridium or one of its alloys. Instead of being tapped at one point, our main may be tapped at one hundred points. The current will divide in strict accordance with law, its power to produce light being solely limited by its strength. The process of division closely resembles the circulation of the blood; the electric main carrying the outgoing current representing a great artery, the gas-pipes carrying the return current representing a great vein, while the intermediate branches represent the various vessels by which the blood is distributed through the system. To fix the matter in your minds, I will illustrate the arrangement on a small scale. Before you is a battery with a thick copper wire

attached to one of its ends, while the other end is connected with a gas-pipe of the Institution. From three different points of the copper wire branch wires pass into three little models of houses, where the branches are subdivided and furnished with spirals of platinum wire. The branches reunite afterwards in the gas-pipe. When the branch currents pass through the houses, they kindle the platinum lamps, which glow with a soft, white light. This, if I understand aright, is Mr. Edison's proposed mode of illumination. The electric force is at hand. Metals sufficiently refractory to bear being raised to vivid incandescence are also at hand. The principles which regulate the division of the current and the development of its light and heat are perfectly well known. There is no room for a "discovery," in the scientific sense of the term, but there is ample room for the exercise of that mechanical ingenuity which has given us the sewing machine and so many other useful inventions, and which engages a greater number of minds in the United States than in any other nation in the world.*

It is sometimes stated as a recommendation to the electric light, that it is light without heat; but to disprove this, it is only necessary to point to the experiments of Davy, which showed that the heat of the Voltaic arc transcends that of any other terrestrial source. The emission from the carbon points is capable of accurate analysis. To simplify the subject, we will take the case of a platinum wire at first slightly warmed by the current, and then, through the gradual augmentation of the latter, raised to a white heat. When first warmed, the wire sends forth rays which have no power on the optic nerve. They are what we call invisible rays; and not until the temperature of the wire has reached nearly 1000° Fahr. does it begin to glow with a faint, red light. The rays which it emits prior to redness are all invisible rays, which can warm the hand but which cannot excite vision. When the temperature of the wire is raised to whiteness these dark rays not only persist, but they are enormously augmented in intensity. They constitute about 95 per cent. of the total radiation from the dazzling platinum wire. They make up 90 per cent. of the emission from a brilliant electric light. You can, by no means, have the light of the carbons without this invisible emission as an accompaniment. The visible radiation is, as it were, built upon the invisible as its necessary foundation.

It is easy to illustrate the growth in intensity of these invisible rays as the visible ones enter the radiation and augment in power. The transparency of the simple gases and metalloids—of oxygen, hydrogen, nitrogen, chlorine, iodine, bromine, sulphur, phosphorus, and even carbon, for the invisible heat-rays is extraordinary. Dis-

* Knowing something of the intricacy of the problem, I should certainly prefer seeing it in Mr. Edison's hands than in mine. It may be added that more than thirty years ago the radiation from incandescent platinum was admirably investigated by Dr. Draper of New York.

solved in a proper vehicle iodine cuts the visible radiation sharply off, but allows the invisible free transmission. We have hitherto used it dissolved in bisulphide of carbon. By fusing together iodine and sulphur, Professor Dewar has recently added to the number of our effectual ray-filters. It may be made as black as pitch for the visible, while remaining transparent for the invisible rays. By such filters it is possible to detach the invisible rays from the total radiation, and to watch their augmentation as the temperature rises. Expressing the radiation from a platinum wire when it first feels warm to the touch—when, therefore, all its rays are invisible—by the number 1, the invisible radiation from the same wire raised to a white heat might be 500 or more. An actual table of measurements will clearly show the gradual growth of the invisible radiation as a spiral of platinum wire rises from darkness to an intense white heat.

State of Spiral.	Obscure Radiation.
Dark	1
Dark, but hotter	3
Dark, but still hotter	5
Dark, but still hotter	10
Feeble red	19
Dull red	25
Red	37
Full red	62
Orange	89
Bright orange	144
Yellow	202
White	276
Intense white	440

It is not then by the diminution or transformation of the non-luminous emission that we obtain the luminous; the heat rays maintain their ground as the necessary antecedents and companions of the light rays. When detached and concentrated these powerful heat rays can produce all the effects ascribed to the mirrors of Archimedes at the siege of Syracuse. While incompetent to produce the faintest glimmer of light, or to affect the most delicate air-thermometer, they will inflame paper, burn up wood, and even ignite combustible metals. When they impinge upon a metal refractory enough to bear their shock without fusion, they can raise it to a heat so white and luminous as to yield, when analyzed, all the colours of the spectrum. In this way the dark rays emitted by the incandescent carbons are converted into light rays of all colours. Still, so powerless are these invisible rays to excite vision that the eye has been placed at a focus competent to raise platinum foil to bright redness without experiencing any visual, or even thermal, impression. Light for light, no doubt, the amount of heat imparted by the incandescent carbons to the air is far less than that imparted by gas flames. It is less because of the smaller size of the carbons, and of the comparative smallness of the quantity of fuel consumed in a given time. It is also less because the air cannot penetrate to the interior of

the carbons as it does to the interior of a flame. The temperature of the flame is lowered by the admixture of a gas which constitutes four-fifths of our atmosphere, and which, while it appropriates and diffuses the heat, does not aid in the combustion. This lowering of the temperature by the inert atmospheric nitrogen renders necessary the combustion of a greater amount of gas to produce the necessary light. In fact, though the statement may appear paradoxical, it is entirely because of its enormous actual temperature that the electric light seems so cool. It is this temperature that renders the proportion of luminous to non-luminous heat greater in the electric light than in our brightest flames. The electric light, moreover, requires no air to sustain it. It glows in the most perfect air vacuum. Its light and heat are therefore not purchased at the expense of the vitalizing constituent of the atmosphere.

Two orders of minds have been implicated in the development of this subject; first, the investigator and discoverer, whose object is purely scientific, and who cares little for practical ends; secondly, the practical mechanician, whose object is mainly industrial. It would be easy, and probably in many cases true, to say that the one wants to gain knowledge, while the other wants to make money; but I am persuaded that the mechanician not unfrequently merges the hope of profit in the love of his work. Members of each of these classes are sometimes scornful towards those of the other. There is, for example, something superb in the disdain with which Cuvier hands over the discoveries of pure science to those who apply them: "Your grand practical achievements are only the easy application of truths not sought with a practical intent—truths which their discoverers pursued for their own sake, impelled solely by an ardour for knowledge. Those who turned them into practice could not have discovered them, while those who discovered them had neither the time nor the inclination to pursue them to a practical result. Your rising workshops, your peopled colonies, your vessels which furrow the seas; this abundance, this luxury, this tumult"—"this commotion," he would have added, were he now alive, "regarding the electric light"—"all come from discoverers in Science, though all remain strange to them. The day that a discovery enters the market they abandon it, it concerns them no more."

In writing thus, Cuvier probably did not sufficiently take into account the reaction of the applications of science upon science itself. The improvement of an old instrument or the invention of a new one is often tantamount to an enlargement and refinement of the senses of the scientific investigator. Beyond this, the amelioration of the community is also an object worthy of the best efforts of the human brain. Still assuredly it is well and wise for a nation to bear in mind that those practical applications which strike the public eye, and excite public admiration, are the outgrowth of long antecedent labours begun, continued, and ended under the operation of a purely

intellectual stimulus. "The ancients discovered the electricity of amber; and Gilbert, in the year 1600, extended the discovery to other bodies. Then followed Boyle, Von Guericke, Gray, Canton, Du Fay, Kleist, Cunæus, and Franklin. But their form of electricity, though tried, did not come into practical use. Then appeared the great Italian Volta, who discovered the source of electricity which bears his name, and applied to its development the most profound insight, and the most delicate experimental skill. Then arose the man who added to the powers of his intellect all the graces of the human heart, Michael Faraday, the discoverer of the great domain of magneto-electricity. Œrsted discovered the deflection of the magnetic needle, and Arago and Sturgeon the magnetization of iron by the electric current. The voltaic circuit finally found its theoretic Newton in Ohm; while Henry, of Princeton, who had the sagacity to recognize the merits of Ohm while they were still decried in his own country, was at that time in the van of experimental inquiry.

"In the works of these men you have all the materials employed at this hour in all the forms of the electric telegraph. Nay, more, Gauss the illustrious astronomer, and Weber the illustrious natural philosopher, both professors in the University of Göttingen, wishing to establish a rapid mode of communication between the observatory and the physical cabinet of the University, did this by means of an electric telegraph. Thus, before those whom the world calls practical men appeared upon the scene, the force had been discovered, its laws investigated and made sure, the most complete mastery of its phenomena had been attained — nay, its applicability to telegraphic purposes demonstrated—by men whose sole reward for their labours was the noble excitement of research, and the joy attendant on the discovery of natural truth." I ought to apologize for thus reproducing words uttered by myself in the United States six years ago. But they apply with particular emphasis to the recent developments of the electric light.

"Few," says Pasteur, "seem to comprehend the real origin of the marvels of industry and the wealth of nations. I need no other proof of this than the frequent employment in lectures, speeches, and official language, of the erroneous expression, 'applied science.' A statesman of the greatest talent, stated some time ago, that in our day the reign of theoretic science had rightly yielded place to that of applied science. Nothing, I venture to say, could be more dangerous, even to practical life, than the consequences which might flow from these words. They show the imperious necessity of a reform of our superior education. There exists no category of sciences to which the name of applied science could be given. We have science and the applications of science, which are united as the fruit is to the tree."

One word more upon what may be called the philosophic bearings of this question. We have amongst us a small cohort of social

regenerators—men of high thoughts and aspirations—who would place the operations of the scientific mind under the control of a hierarchy which should dictate to the man of science the course that he ought to pursue. How this hierarchy is to get its wisdom they do not explain. They decry and denounce scientific theories; they scorn all reference to æther, and atoms, and molecules, as subjects lying far apart from the world's needs; and yet such ultra-sensible conceptions are often the spur to the greatest discoveries. The source, in fact, from which the true natural philosopher derives inspiration and unifying power, is essentially ideal. Faraday lived in this ideal world. Nearly half a century ago, when he first obtained a spark from a magnet, an Oxford don expressed regret that such a discovery should have been made, as it placed a new and facile implement in the hands of the incendiary. To regret, a Comtist hierarchy would have probably added repression, sending Faraday back to his bookbinder's bench as a more dignified and practical sphere of action than peddling with a magnet. And yet it is Faraday's spark which now shines upon our coasts, and promises to illuminate our streets, halls, quays, squares, warehouses, and, perhaps at no distant day, our homes.

[J. T.]

WEEKLY EVENING MEETING,

Friday, January 24, 1879.

Sir W. Frederick Pollock, Bart. M.A. Vice-President, in the Chair.

Professor W. E. Ayrton.

The Mirror of Japan and its Magic Quality.

The lecturer commenced by referring to the vast differences between the Chinese and Japanese nations, of which the English people as a rule do not seem to be aware. He instanced various points of contrast; one of the most important being the intensely oriental secluded character of the private life of the Chinese on the one hand, and the Japanese dwelling in houses unfurnished and left wide open to public gaze on the other. But why, he asked, in this comparative absence of nearly all that we should call furniture, does one article pertaining to the ladies' toilette—the bronze mirror with its stand—hold so prominent a position?

This mirror of the Far East is usually circular, from three to twelve inches in diameter, made of bronze, and with a bronze handle covered with bamboo. The reflecting face is generally more or less convex, polished with a mercury amalgam; the back is gracefully ornamented with a well-executed raised design, representing birds, flowers, dragons, a geometrical pattern, or some scene in Japanese mythical history. Occasionally there are also one or more Chinese characters (signifying long life, happiness, or some similar idea) of polished metal in bold relief. The general appearance of the back of the mirror, therefore, is something like that seen in the figure in the next page.

It might at first sight be surmised that the elaborate head-dresses of the ladies in Japan, combined with the painting of their faces, furnished an explanation of the prominence given to the metal mirror. But that this is not the case is easily seen from the fact that it is in the Imperial Palace, where the court ladies, still preserving the simple fashion of ancient days, merely comb back their long black tresses, and so have least need of a looking-glass, that the Japanese mirror receives the highest respect. A foreigner meets the mirror in the temples, in the hands of the street conjuror, in pictures of the infernal regions, and in the regalia of the Japanese sovereigns, and for some time after his arrival in Japan feels as an Oriental, ignorant of Biblical history, might when unable to understand the constant repetition of the cross in Roman Catholic countries. But at length he hears that

the mirror is part of the Japanese religion, and is mixed up with the "Divine Right of Kings," that it is the most precious of the possessions of a Japanese woman, and constitutes the most important part of the trousseau of a bride, and that the "Two Great Divine Palaces" at Isé in which was deposited the first made mirror, have in the eyes of the Japanese the same importance as has the Holy Sepulchre for the Greeks and Armenians, and Mecca for the Mahommedans.

And to realize the reason of this the stranger must learn that there is a famous ancient myth in Japan, which was recounted by the lecturer, detailing how the sun-goddess in a rage shut herself up in a rocky cave, and how the other gods, to dispel the darkness thus caused,

used various artifices to entice her forth, the most successful ruse being the manufacture of the first historical mirror, in which seeing her face she was drawn forth by her curiosity and jealousy. He will also learn how in the supposed creation of the Japanese empire, the sun-goddess is reputed to have handed this mirror (with the two other "gods' treasures" which, together with a mirror, at present constitute the regalia of the emperor) to her grandson with these words: "Look upon this mirror as my spirit, keep it in the same house and on the same floor with yourself, and worship it as if you were worshipping my actual presence."

After describing many interesting points in connection with this strange mirror worship of the Japanese as seen in the palace and in

the cottage, the lecturer went on to say that to the majority of those present the investigation of the so-called magic properties of the Japanese mirror would probably prove of yet more interest.

This magic property, which is possessed by a few rare specimens coming from the East, is as follows:—If the polished surface is looked at directly it acts like that of an ordinary mirror, reflecting the objects in front of it, but giving, of course, no indication whatever of the raised patterns on the back; if, however, a bright light be reflected by the smooth face of the mirror on to a screen, there is seen on this screen an image, formed of bright lines on a dark ground, more or less perfectly representing the pattern on the back of the mirror, which is altogether hidden from the light.

When this appearance is seen for the first time it is perfectly startling, even to an educated mind; and if the source of light is sufficiently bright, as for instance a tropical sun, it is difficult for the observer to divest himself of the idea that the screen is not perforated with cuts corresponding with the pattern on the back of the mirror, and illuminated from behind.

This strange phenomenon was known to Sir David Brewster and to Sir Charles Wheatstone, both of whom were of opinion that it was produced by trickery on the part of the maker. Sir David Brewster, for example, says in the 'Philosophical Magazine' for December, 1832:—"Like all other conjurors, the artist has contrived to make the observer deceive himself. The stamped figures on the back (of the mirror) are used for this purpose. The spectrum in the luminous area is *not an image of the figures on the back.* The figures are a copy of the picture which the artist *has drawn on the face of the mirror*, and so concealed by polishing that it is invisible in ordinary lights, and can be brought out only in the sun's rays."

Professor Ayrton then related how he had been quite unable to find for sale in any of the shops of Japan one of these magic mirrors which was supposed in Europe to be a standard Japanese trick, and he explained how he had at length ascertained that with regard to this so-called magic mirror the Japanese were the people who know least about the subject.

But these magic mirrors were known to the Chinese from the earliest times, and one of their writers spoke about them in the ninth century of the Christian era. They call them *Theou-kouang-kién*, which means literally "mirrors that let the light pass through them," the name, of course, arising from a popular error on the subject. The Roman writer Aulus Gellius, who lived seventeen centuries ago, referred to mirrors that sometimes reflected their backs and sometimes did not. From the great antiquity of these Chinese magic mirrors the German writer Herr Sterne has concluded that it is probable that the mirrors with secret signs and figures of imps on the back, which formed a portion of the stock-in-trade of the witches of the middle ages, were of Eastern manufacture. The Italian historian Muratori gives an account of the magic mirror found under the pillow of the Bishop of

Verona, who was afterwards condemned to death by Martin della Scala, as well as of the one discovered in the house of Colla da Rienzi, and on the back of which was the word "Fiorone." But of these magic mirrors which have played so important a part, not only in the priestcraft of China, but also in the oracles of the Greeks and Etruscans, and in the witchcraft of the middle ages, inquiry has shown that Japanese literature makes absolutely no mention.

Is it, then, that such mirrors cannot be found in Japan? Undoubtedly they cannot be bought by inquiry at the shops, but Professor Ayrton's investigations have shown that if a careful examination with properly arranged light be made of a large number of the ordinary Japanese bronze mirrors, a few, perhaps two or three per cent., will be found showing the phenomenon clearly.

The lecturer then referred to the extracts he had made from a large portion of all that had been written in various languages regarding the explanation of the phenomenon. He mentioned that the earliest explanation was given by a Chinaman, Ou-tseu-hing, who lived between 1260 and 1341, and who also had the impression that the magic property of the mirror was produced by an artifice; for he wrote: "When we turn one of the mirrors with its face to the sun, and allow it to throw a reflection on a wall close by, we see the ornaments or the characters which exist in relief on the back appear clearly. Now the cause of this phenomenon arises from the employment of two kinds of copper of unequal density. If on the back of the mirror a dragon has been produced while casting it in the mould, then an exactly similar dragon is deeply engraved on the face of the disk. Afterwards the deep chisel-cuts are filled up with denser copper, which is incorporated with the body of the mirror, which ought to be of finer copper, by submitting the whole to the action of fire; then the face is planed and prepared, and a thin layer of lead or of tin spread over it.*

"When a beam of sunlight is allowed to fall on a polished mirror prepared in this way, and the image is reflected on a wall, bright and dark tints are distinctly seen, the former produced by the purer copper, and the latter by the parts in which the denser copper is inlaid."

Ou-tseu-hing adds that he has seen a mirror of this kind broken into pieces, and that he has thus ascertained for himself the truth of this explanation.

In a paper communicated some years ago to the French Academy of Sciences, the well-known French writer on China, M. Stanislas Julien, says: "Many famous philosophers have for a long time, but without success, endeavoured to find out the true cause of the phenomenon which has caused certain metallic mirrors constructed in China to have acquired the name of *magic mirrors.* Even in the country itself

* This probably refers to the mercury amalgam which is used in polishing, and which Ou-tseu-hing mistook for lead or tin.

where they are made no European has up to the present time been able to obtain, either from the manufacturers or from men of letters, the information, which is so full of interest to us, because the former keep it a secret when by chance they possess it, and the latter generally ignore the subject altogether. I had found many times in Chinese books details regarding this kind of mirrors, but it was not of a nature to satisfy the very proper curiosity of philosophers, because sometimes the author gave on his own responsibility an explanation that he had guessed at, and sometimes he confessed in good faith that this curious property is the result of an artifice in the manufacture, the monopoly of which certain skilled workmen reserve to themselves. One can easily understand this prudent reticence when we remember that the rare mirrors which show this phenomenon sell from ten to twenty times as dear as the rest."

The prevalent idea has been that the phenomenon of the magic mirror was caused by a difference of density in various parts of the surface, either produced intentionally or accidentally; and this the lecturer explained arose from two causes, first from the common belief that the patterns on Japanese and Chinese mirrors were, like those on ordinary coins, produced by stamping, the other because the distinguished European philosophers who had examined into the question had investigated with considerable success experimentally how such mirrors might be made, but they had not, the lecturer thought, directed their attention to the examination of the question—How was the phenomenon in these rare Eastern mirrors actually produced?—obviously a very different question.

Professor Ayrton mentioned that he and his colleague, Professor Perry, were led to take up the investigation from a very remarkable fact pointed out by Professor Atkinson of Japan, viz. that a scratch made with a blunt iron nail on the back of one of these magic mirrors, although it produced no mark on the face of the mirror which could be seen by direct vision, nevertheless became visible as a bright line on the screen when a beam of sunlight was reflected from the polished face of the mirror. The lecturer mentioned that after trying various experiments with polarised light, &c., Professor Perry and himself availed themselves of a very simple method of investigation, but one which had apparently not suggested itself to previous observers. On one occasion, when some of their students were using lenses to endeavour to make the exhibition of the phenomenon more striking, it occurred to them that the employment of beams of light of different degrees of convergence or divergence would furnish a test for deciding the cause of the whole action. For while, if the phenomenon were due to molecular differences in the surface—the commonly received opinion—the effect would be practically independent of the amount of convergence of the beam of light; on the other hand, if it, by any chance, were due to portions of the reflecting surface being less convex than the remainder, a complete *inversion* of the phenomenon might be expected to occur, if the experiments, instead of being tried in ordinary

sunlight, were made under certain conditions in a converging beam—that is, the thicker portions of the mirror might be expected to appear darker instead of brighter than the remainder.

[Experiments were then shown of the image cast on the screen: 1st, when a divergent beam of light fell on the mirror; 2nd, when the beam was parallel; 3rd, when the beam was convergent: and it was seen that, 1st, the pattern appeared as bright on a dark ground; 2nd, the pattern was invisible; 3rd, the pattern appeared as dark on a bright ground.]

Again, by allowing a parallel beam of light to fall on the Japanese mirror and interposing a double convex lens between the mirror and the screen, we can make the image show the pattern either as bright on a dark ground or as dark on a bright ground, or not at all, merely by causing the screen to be: 1st, nearer the lens than the conjugate focus of the mirror; second, farther than the conjugate focus; 3rd, at the conjugate focus. [This experiment was here shown.] Now it can easily be proved by simple geometrical optics that each of these effects would be produced if the thicker parts of the mirror were a little less convex than the remainder. [This was explained by various geometrical diagrams.] And lastly, if the phenomenon was, as the previous experiment would lead us to conclude, due not to unequal reflecting power of the different portions of the surface of the mirror, but to minute inequalities on the surface, in consequence of which there is more scattering of the rays of light falling on one portion than on another, then since rays of light making very small angles with one another do not separate perceptibly until they have gone some distance, it follows, that if the screen be held *very near* to the mirror, the apparent reflection of the back, the magical property in fact, ought to become invisible. And this, also, it was shown, was exactly what happened when the screen was made almost to touch the polished surface.

The lecturer then proceeded to explain why a *divergent* beam, emitted by a bright luminous *point* at some fifteen feet distance from the mirror, gave the best effects.

We have, therefore, strong reasons for favouring the "inequality of curvature" theory. In order, however, to make the explanation quite certain, the lecturer said he had made a small concavity and a small convexity on the face of one of the mirrors, by hammering with a blunt tool, carefully protected with a soft cushion to avoid scratching the polished surface, and he showed by experiment that the concavity reflected a bright image and the convexity a dark one when the pattern on the back appeared bright, but when the light was so arranged that the pattern appeared as dark on a bright ground, it was the convexity which appeared as the bright spot, and the concavity as the dark one.

Guided by all that precedes, we are led to the undoubted conclusion, that the whole action of the magic mirror arises from the thicker portions being flatter than the remaining convex surface, and

even being sometimes actually concave. But, in spite of this irresistible conclusion forced on us by the experiments previously mentioned, it must be admitted that it seems extraordinary how such small inequalities in the surface of the mirror—so small, in fact, that the eye quite fails to detect them—can, even with a proper arrangement of the light, produce on the screen an image of the pattern on the back as sharp and clear as is seen with a good specimen of a magic mirror.

The next question arises, Why is there this difference in the curvature of the different portions of the surface? The experience that Professor Ayrton had gained from an examination of a large number of Japanese mirrors supplied, in part at any rate, the answer to the question. No thick mirror reflects the pattern on the back, not one of the many beautiful mirrors exhibited at the National Exhibition of Japan in 1877, and which the lecturer was so fortunate as to be able to experiment with in a darkened room with a bright luminous point at some twelve feet distance, showed the phenomenon in the slightest degree; some good old mirrors in the museum of the Imperial College of Engineering, and which belonged to the family of the late Emperor, the Shogun, of Japan, failed to reflect any trace of a design, and some old round mirrors without handles, which he had also tried, were (with the exception of one which was immensely prized and brought to him wrapped in five distinct silk cases, and the heirloom of the family of a nobleman) equally unsuccessful.

Again, it is not that the pattern is less clearly executed on the backs of these choice mirrors, since the better the mirror the finer and bolder is the pattern, but what is especially noticeable is that every one of these mirrors is as a whole far thicker than an ordinary Japanese mirror, and its surface is much *less convex.* This naturally led him to inquire, How are Japanese mirrors made convex? Are they cast so, or do they acquire this shape from some subsequent process?

His search through all the literature at his disposal, European, Japanese, Chinese, on the subject of mirrors failed to elicit the slightest hint, he was therefore compelled to perform the somewhat difficult task of obtaining information from the Japanese workmen themselves. Eventually he ascertained that while practically all Japanese mirrors were convex, the surface of each half of the mould was quite flat, and that the curvature was given to the mirror after casting in the following way.

The rough mirror is first scraped approximately smooth with a hand-scraping tool, and as this would remove any small amount of convexity, had such been imparted to it in casting, it is useless to make the mould slightly convex. If, however, a convex or concave mirror of small radius is required, then the surface of the mould is made concave or convex. On the other hand, to produce the small amount of convexity which is possessed by ordinary Japanese mirrors the following method is employed, if the mirror is thin, and it is with thin mirrors we have especially to deal, since it is only in these

mirrors that the apparent reflection of the back is observed. The mirror is placed face uppermost flat on a wooden board, and then scraped or rather scratched with a rounded iron rod about half an inch in diameter and a foot long, called a *megebo*, " distorting rod," so that a series of parallel scratches is produced, which causes the face of the mirror to become convex in the direction at right angles to the scratches, but to remain straight parallel to the scratches; in fact it becomes very slightly cylindrical, the axis of the cylinder being parallel to the scratches. This effect is very clearly seen by applying a straight-edge in different ways to the face of an unpolished mirror which has received a single set of scratches only. A series of scratches is next made with the *megebo* in a direction of right angles to the former, a third set intermediate between the two former, and so on, the mirror each time becoming slightly cylindrical, the axis of the cylinder in each case being parallel to the line of scratches, so that eventually the mirror becomes generally convex. Some workmen prefer to make the scratches with the *megebo* in the form of small spirals, others in the form of large spirals; but the general principle of the method employed with their mirrors appears to be always the same,—the face of the mirror is scratched with a blunted piece of iron, and becomes slightly convex, the back, therefore, becoming concave.

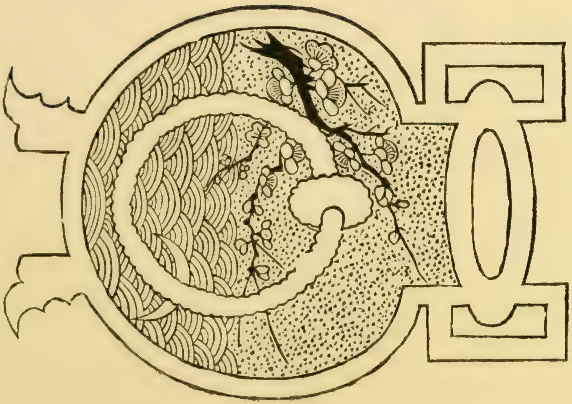
[Some mirrors were here exhibited: one with its surface flat. although somewhat rough, just as it came from the mould after casting; a second that had received one set of parallel scratches with the *megebo*, and which by means of a straight-edge was shown to be slightly cylindrical; and a third, on the face of which the operation of scratching had been completed, and which was, therefore, slightly convex.]

After the operation with the " distorting rod " the mirror is very slightly scraped with a hand scraping-tool to remove the scratches and to cause the face to present a smooth surface for the subsequent polishing.

In the case of thick mirrors the convexity is first made by cutting with a knife, and the " distorting rod " applied afterwards. But in connection with this cutting process of thick mirrors there is one very interesting point. If the maker finds on applying from time to time the face of the mirror to a hard clay concave pattern, and turning it round under a little pressure, that a portion of the surface has not been in contact with the pattern, in other words, that he has cut away this portion too much, then he rubs this spot round and round with the *megebo* until he has restored the required degree of convexity. Here again, then, scratching on the surface produces convexity.

Now, why does the scraping of the " distorting rod " across the face of the mirror leave it convex ? During the operation it is visibly concave. The metal must receive then a kind of " buckle," and spring back again so as to become convex when the pressure of the rod is removed. It might in such a case reasonably be expected that the

MIRROR AT KAMAKURA.



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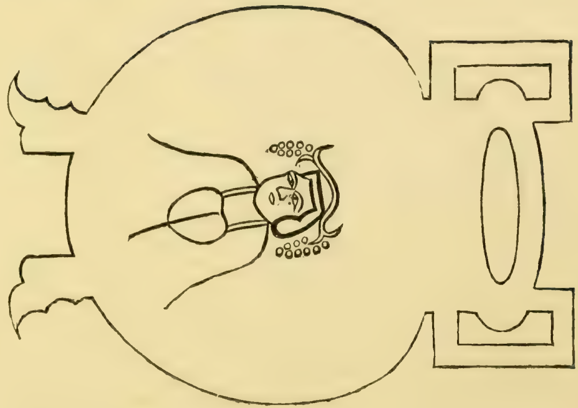

FRONT.

thicker parts of the mirror would yield less to the pressure of the rod than the thinner, and so would be made less convex, or even they might not spring back, on the removal of the rod, and so remain actually concave. Again, since we find that scraping the face of the mirror is the way in which it is made convex, and the back therefore concave, we might conclude that a deep scratch on the back would make the back convex and the face slightly concave. Such a concavity, as we have proved, would explain the phenomenon of the bright line appearing in the reflection of sunlight on the screen which was observed by Professor Atkinson to correspond with the scratch on the back.

After the scratches produced by the *megebo* are removed, the mirror is polished with whetstones and then with charcoal. The face now becomes fairly smooth, but it still generally contains some few cavities; these the maker fills up from a stock of copper balls of various sizes which he has at hand. (It was probably the presence of these bits of copper that led Ou-tseu-hing to believe that the explanation of the cause of the magic mirrors was the inlaying of different metals.) The face of the mirror is finally rubbed over with a mercury amalgam containing fifty per cent. of tin, by means of a small straw brush, or with the hand.

The lecturer then referred to the various metal mixtures employed by the Japanese in making their mirrors, the best being composed of 75 per cent. of copper, 23 of tin, and 2 per cent. of a natural sulphide of lead and antimony.

Although the Japanese have paid no attention to the magic mirror which has created such interest in Europe, they have in connection with their priestcraft employed mirrors, on the surface of which, if looked at very obliquely, could be seen the faces of saints, which were not in any way connected with the pattern on the back of the mirror. The accompanying figures represent the front and back of one of these religious mirrors, about four and one-fifth inches high and three and a half wide, and which exists at Kamakura, the old capital of the former Emperor of Japan, the Shogun, in a temple to which great reverence is paid on account of the supposed supernatural character of this mirror. In the polished surface, when looked at very obliquely, is seen the face of a Buddhist priest, and the back is ornamented with a moon rising from the sea, a rosary, and a plum tree.

The lecturer also exhibited two mirrors of this kind which he had had made in consequence of the belief expressed by one of the Japanese mirror makers, that the phenomenon of the so-called magic mirrors was produced by chemical action on the surface. But the result of the experiment had been, that if the face of a mirror which had been chemically acted on was polished until every trace of the marks disappeared for direct or oblique vision, then they also disappeared in the image produced by reflecting a beam of light on to a screen, and consequently that it did not seem possible, as far as his

experiments had gone, to produce by means of chemical action on the surface, a mirror fulfilling all the conditions of a magic mirror. He concluded by saying—"It appears, then, contrary to what is generally believed, that the magic of the Eastern mirror results from no subtle trick on the part of the maker, from no inlaying of other metals, or hardening of portions by stamping, but merely arises from the natural property possessed by certain thin bronze of buckling under a bending stress, so as to remain strained in the opposite direction after the stress is removed. And this stress is applied partly by the 'distorting rod,' and partly by the subsequent polishing, which, in an exactly similar way, tends to make the thinner parts more convex than the thicker."

WEEKLY EVENING MEETING,

Friday, January 31, 1879.

William Spottiswoode, Esq. D.C.L. President R.S. Vice-President, in the Chair.

H. Heathcote Statham, Esq.

The Logic of Architectural Design.

[Abstract deferred.]

GENERAL MONTHLY MEETING,

Monday, February 3, 1879.

GEORGE BUSK, Esq. F.R.S. Treasurer and Vice-President, in the Chair.

The Marquis of Blandford,
Mrs. L. Lawrence,
Joseph Mellor, Esq.
Mrs. Julius von Mumm,
Major-General Charles Sawyer,

were *elected* Members of the Royal Institution.

The decease of Mrs. SARAH FARADAY, widow of the late Professor, on January 6th, 1879, aged 79, was announced.

The Special Thanks of the Members were returned to Dr. C. W. SIEMENS for his liberal gift and arrangements in respect of the Boiler and the Dynamo-Electric Machine made by Mr. T. A. Edison; to M. RAPIEFF for his present of Electric Lamps; and to Mr. W. H. PREECE for his present of a Phonograph.

The Special Thanks of the Members were given to PROFESSOR TYNDALL for his kindness in repeating, on January 20th, his discourse on the Electric Light, given on January 17th, to meet the disappointment experienced by numerous Members and their friends.

WARREN DE LA RUE, Esq. M.A. D.C.L. F.R.S. was elected SECRETARY OF THE ROYAL INSTITUTION, and WILLIAM SPOTTISWOODE, Esq. D.C.L. LL.D. Pres. R.S. was elected MANAGER.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The India Office: Bengal Government—Report on Public Instruction in Bengal for 1877-8. fol. 1878.

M. Louis Schwendler's Précis of Report on Electric Light Experiments. fol. 1878.

Asiatic Society of Bengal—Journal, Vol. XLVII. Part I. Nos. 2, 3; Part II. No. 3. 8vo. 1878.

Proceedings, 1878, Nos. 7, 8. 8vo.

Astronomical Society, Royal—Monthly Notices, Vol. XXXI. Nos. 1, 2. 8vo. 1878.

Belgium: Royal Academy of Sciences—Mémoires, Tomes X. XII.-XLII. 4to. 1836-78.

Mémoires Couronnés, Tomes XI.-XXXVIII. Tome XXXIX. Partie 1. Tomes XL. XLI. 4to. 1837-78.

Mémoires Couronnés: Collection en 8vo. Tomes I.-XXVIII. 1852-78.

Bulletins, Tomes XLI.-XLIV. 8vo. 1876-8.

Almanach, 1877, 1878. 12mo.

A. Namur: Tables de Logarithmes à 12 Décimales jusqu'à 434 Milliards, avec Preuves. 8vo. 1877.

British Architects, Royal Institute of—1878-9: Proceedings, Nos. 3–5. 4to. Transactions, Nos. 2–5. 4to.
Chemical Society—Journal for Nov. Dec. 1878, Jan. 1879. 8vo.
Conway, Moncure D. Esq. (the Author)—Demonology and Devil-Lore. 2 vols. 8vo. 1879.
Coutts, J. Esq. (the Author)—The Philosophy of Science: Experience and Revelation. 16to. 1878.
Cox, Edward W. Esq. Serjeant-at-Law, &c.—The Claims of Psychology. (K 103) 8vo. 1878.
Editors—American Journal of Science for Dec. 1878, and Jan. 1879. 8vo.
Analyst for Dec. 1878, and Jan. 1879. 8vo.
Athenæum for Dec. 1878, and Jan. 1879. 4to.
Chemical News for Dec. 1878, and Jan. 1879. 4to.
Engineer for Dec. 1878, and Jan. 1879. fol.
Horological Journal for Dec. 1878, and Jan. 1879. 8vo.
Iron for Dec. 1878, and Jan. 1879. 4to.
Journal for Applied Science for Dec. 1878, and Jan. 1879. fol.
Nature for Dec. 1878, and Jan. 1879. 4to.
Telegraphic Journal for Dec. 1878, and Jan. 1879. 8vo.
Franklin Institute—Journal, Nos. 636, 637. 8vo. 1878.
Geographical Society, Royal—Proceedings, New Series. Vol. I. No. 1. 8vo. 1879.
Geological Institute, Imperial, Vienna—Verhandlungen, 1878. Nos. 11–13. New Series, No. 1. 8vo. 1879.
Jahrbuch: 1878. Band XXVIII. No. 3. 8vo.
Geological Survey of India—Records, Vol. XI. Part 4. 8vo. 1878.
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WEEKLY EVENING MEETING,

Friday, February, 7, 1879.

Sir W. Frederick Pollock, Bart. M.A. Vice-President, in the Chair.

Rev. H. R. Haweis, M.A.

Bells.

[Abstract deferred.]

WEEKLY EVENING MEETING,

Friday, February 14, 1879.

C. William Siemens, Esq. D.C.L. F.R.S. Vice-President, in the Chair.

G. Johnstone Stoney, Esq. M.A. F.R.S.

The Story of the November Meteors.

[As some readers may wish to consult the original investigations referred to in this lecture, a list of them is given in a postscript at the end.]

METEORS AS THEY APPEAR IN THE EARTH'S ATMOSPHERE.

When observers band together to watch every quarter of the sky, and to keep on the look-out through the whole night, the number of meteors that present themselves is very great. In this way it has been ascertained that upwards of thirty on the average, which are conspicuous enough to be seen without instruments, come within the view of the observers stationed at one locality. And it is computed that telescopic meteors must be about forty or fifty times as numerous as those visible to the naked eye.

These results may be obtained from observations made at one station; but when concerted observations are carried on at different stations, several other facts of interest come to light. By simultaneous observations at distant stations, it has been discovered that the height of meteors above the surface of the earth usually ranges from 120 down to twenty miles, the average height being about sixty miles; that the direction of their flight is towards the earth, either in a vertical or in a sloping direction; and that their speed in most cases lies between thirty and fifty miles a second.

We thus arrive at the conclusion that *visible* meteors are phenomena of our own atmosphere; and as the atmosphere reaches a height, at most, of 150 miles, and is, therefore, but a thin film over so vast a globe as the earth, it is obvious that the spectators at any one place can see only a very small portion of the meteors which dart about through all parts of this envelope. After making allowance for this, we are forced to conclude that no fewer than 300 millions of these bodies pass daily into the earth's atmosphere, of which about seven millions and a half are large enough to be seen with the naked eye on a clear night, and in the absence of the moon.

From the direction and swiftness of their flight, it is manifest that meteors are visitors from without. They plunge into our atmosphere,

and the resistance to which they become then suddenly exposed must raise them to a temperature which exceeds that of the most intense furnace. The heat is enough first to melt and then to dissipate in vapour the most refractory substances, and it only now and then happens that even a part of a meteor escapes this fate, and reaches the ground. They are for the most part lost in vapour ere they get within several miles of us. The difficulty, indeed, is not to account for their incandescence, but to see why they do not emit a greater flood of light where the heat must be so intense. And, in fact, they cannot be other than very small bodies, or they would be much brighter. The average weight of those visible to the unassisted eye appears to be under an ounce, and the telescopic ones, of course, are much lighter.

SPORADIC METEORS, AND METEORIC SHOWERS.

Meteors may be distributed into two very obvious classes—casual meteors, which dart irregularly through the sky, and meteoric showers, which stream into our atmosphere in one definite direction, and at stated intervals of time. We are concerned at present with the meteoric showers. Many such are known to exist, of which the principal are the August shower, through which the earth passes every year upon the 9th, 10th, and 11th of August; and the great November shower, which is discharged upon the earth three times in a century. The November meteors are those about which most is known, and it was of these, therefore, that the lecture chiefly treated.

THE REGIONS FROM WHICH METEORS COME.

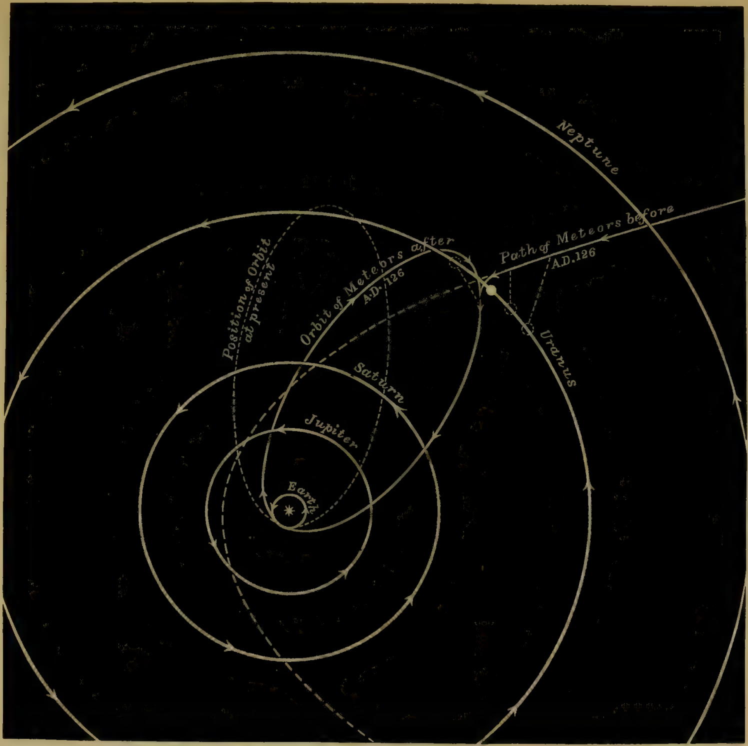
To make their history intelligible, it was necessary to explore, in some degree, the regions from which they come. For this purpose a great diagram was exhibited on a scale rather more than thirty times the scale of the accompanying woodcut. Yet, though the diagram was so large, every hundredth of an inch upon it represented a distance in nature equal to the interval between the earth and the moon. The distance from the earth to the sun on this diagram was a decimeter, that is, four inches; and, on the same scale, the nearest fixed star would have to be placed at a distance of twenty kilometers, or upwards of twelve miles.

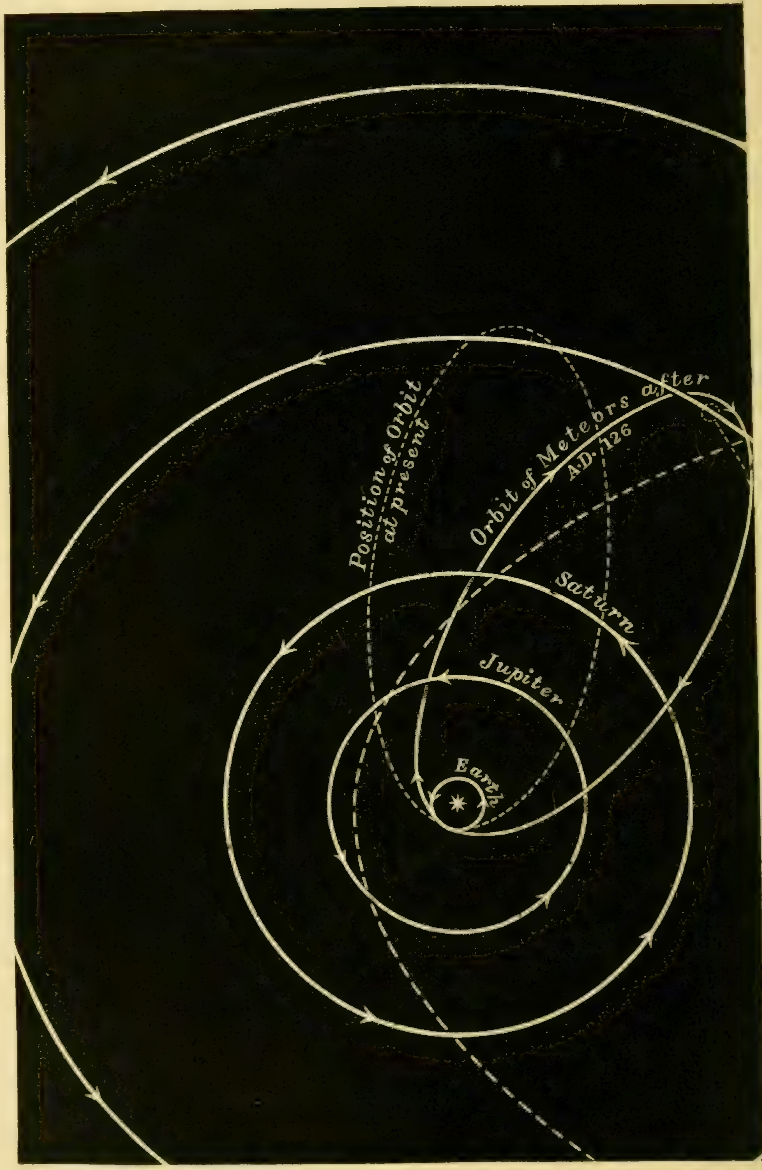
ORBIT OF THE GREAT NOVEMBER SWARM.

In these vast celestial spaces, there are no rails over the roughnesses of which the train must be made to rattle, if it is to move at all; there are no wheels to be worn out; there is no air in which a wind must be produced, or through which noise will be propagated. The music of the spheres is not a sound audible to the ear, and an impediment to motion: it is harmless, it is altogether good, it is the pleasure

of the human mind when it understands the great works of nature. There is no thundering along through the heavens. All is silence and peace round the planets as they swiftly glide. Bodies which sweep in this way without obstruction through the depths of space, are ready to yield at once the due amount of obedience to the attraction of the sun. Accordingly each meteor which traverses the elliptic orbit represented in the diagram, mends its pace so long as it is gliding along that half of its course in which it is approaching the sun, because here the sun is drawing it forwards as well as sideways; and the forward attraction increases its velocity, while the sideward attraction bends its path into the oval form. The meteor takes upwards of sixteen years to traverse this part of its orbit, and all this time its velocity is on the increase. It has attained its greatest speed when it reaches the point of its orbit which is closest to the sun, near to which is the place where it crosses the earth's path. As it passes this point its velocity is twenty-seven miles a second. The earth moves at the rate of nineteen miles a second in very nearly the opposite direction, so that if the meteor happen to strike the earth, the velocity of its approach is the sum of these two numbers, or forty-six miles a second; and it is at this enormous speed that it plunges into our atmosphere. But if it escape the earth, and continue its course along its orbit, it loses speed for the next sixteen years, until it passes the farthest part of its orbit at its slowest pace, which is about a mile and a third per second. In each revolution its velocity oscillates between these extremes. Its orbit is so vast that it takes thirty-three years and a quarter to get round it.

Such is a good picture of the course pursued by each member of the great November swarm. There are countless myriads of meteors in this mighty group, each one moving independently of the rest, each one fulfilling its own destiny. They form, together, an enormous stream of meteors, the dense part of which appears to be about 100,000 miles in width, and of immense length. The orbit along which they travel was represented on the diagram by an ellipse of 207 centimeters, or close upon seven feet, long—i. e. by an oval about as long and broad as the hall-door of a house; and the length, breadth, position, and motion of the swarm in 1865, before it reached the earth, would be represented on the same scale by a thread of the finest sewing silk, about a foot and a half or two feet long, creeping inwards along the orbit, the rear of the column having been between the orbits of Jupiter and Saturn, and the front of it nearly as far in as the earth's orbit. The actual train which is thus represented was so amazingly long that even moving at the rate of twenty-seven miles a second, it took upwards of two years to pass the point where its path crosses the earth's orbit. The earth passes this point on the morning of the 14th of November in every year. The head of the dense part of the stream seems to have reached the same point early in the year 1866. The earth was then in a distant part of its orbit, but on the following 14th of November we came round to the place where the great stream of


Neptune
Path of Meteors before
A.D. 126
Orbit of Meteors after
A.D. 126
Position of Orbit
at present
Uranus
Saturn
Jupiter
Earth


Position of Orbit at present
Orbit of Meteors after A.D. 126
Saturn
Jupiter
Earth

meteors was pouring across our path. The earth then passed through the swarm, just as you might imagine a speck, too small to be seen by the eye, to be carried on the point of a fine needle in a sloping direction through the thread which represents the meteors. The earth took about five hours to pass through the stream; and it was Europe, Asia, and Africa, which happened at the time to be moving forwards. Accordingly it was upon this side of the earth on that occasion, that the meteors were poured, and they produced the gorgeous display in our atmosphere which many here must remember. In 1867, when we came round again to the same place, the stream of meteors was still there. America, this time, chanced to be the part of the globe which was turned in the right position to receive the shower. In 1868, the mighty swarm had not passed, and in subsequent years, when we came round to the proper place, we still found ourselves among outlying stragglers of the great procession.

[The lecturer next attempted to give an outline of the successive steps by which the path over which the meteors travel had been determined, and in doing so had an opportunity of adding other particulars to the marvellous history of these bodies.]

HUMBOLDT.

In 1799 Humboldt was travelling in South America, and on the morning of the 12th of November in that year the November shower was poured out over the New World. Humboldt's description of this shower seems first to have fixed the attention of scientific men upon the subject. But he contributed still more to the advance of our knowledge by the success with which he insisted that nearly all such phenomena are periodic, and that therefore there is reason to hope that the causes of them are discoverable. Shortly after, the periodic character of the August meteors was established; and when the next return of the November meteors to the earth took place, when there was a magnificent display of them exhibited to Europe in 1832, and a still more impressive spectacle seen in America in the following year, the attention of scientific men was thoroughly aroused.

PROFESSOR H. A. NEWTON.

In England, meteors began to be systematically observed, and in this way all that knowledge about them has been acquired which was referred to in the beginning of the lecture. In France, the records of antiquity and the annals of distant nations were ransacked; and by this most useful antiquarian search, no less than ten visits of the November swarm, previous to the shower observed by Humboldt in 1799, have been brought to light. But the first great step towards gaining a knowledge of their orbit was made by Professor H. Newton, of New Haven in America, who published in 1864 two memoirs, in which he discussed all the accounts that had been collected, extending back to

the year A.D. 902. He found by comparing the dates of the old observations with the modern ones, that the phenomenon is one which recurs three times in a century, or more exactly, that the middle of the swarm crosses the earth's path at intervals of $33\frac{1}{4}$ years. He further showed that meteors which thus visit the earth three times in a century must be moving in one or other of five orbits which he described; and that therefore if means could be found for deciding between these five orbits, the problem would be solved. The five possible orbits are —the great oval orbit which we now know the meteors actually do traverse every 33 years and a quarter; a nearly circular orbit, very little larger than the earth's orbit, which they would move round in a few days more than a year; another similar orbit in which their periodic time would be a few days short of a year; and two other small oval orbits lying within the earth's orbit. But we owe even more to Professor Newton. He also pointed out how it was *possible* to ascertain which of these orbits is the true one, although the test he indicated was one so difficult of application that there was at the time little hope that any astronomer would attempt it. Fortunately our own Professor Adams, of Cambridge, was found able to grapple with the difficulties of the problem, and willing to encounter its immense labour, and to him we owe the completion of this great discovery.

PROFESSOR ADAMS.

A comparison of the dates of the successive showers which have been recorded shows that the point where the path of the meteors crosses the earth's orbit is not fixed, but that every time the meteors come round they strike the earth's orbit at a point which is twenty-nine minutes (i. e. nearly half a degree) farther on in the direction in which the earth is travelling. In other words, the meteors do not describe exactly the same orbit over and over again: their path in one revolution is not exactly the same as their path in the next revolution, although very close to it. Thus, their path in A.D. 126 was that which is represented by the strong oval line in the diagram, but in the seventeen centuries which had since elapsed, it has gradually shifted round into the position represented by the dotted ellipse. This kind of motion is well known to astronomers, and its cause is well known. It would not happen if the sun were the only body attracting the meteors, but arises because the planets also draw them in other directions; and although the attraction of the planets is very weak compared with the immense power of the sun, still they are able to drag the meteors a little out of their course round the sun, and in this way occasion that shifting round of the orbit of which we are speaking. Now, in the case of meteors which are really travelling in the large orbit, this shifting of the orbit must be due to the attraction of the planets Jupiter, Saturn, Uranus, and the Earth, while, if they had travelled in any of the four smaller orbits, the planets that would be near enough and large enough to act sensibly upon them would be the

Earth, Venus, and Jupiter. Accordingly, if anyone could be found able to calculate how much effect would be produced in each of the five cases, the calculated amount of shifting of the orbit could be compared with the observed amount, which is 29′ in 33¼ years, and this would at once tell which of the five possible orbits is the true one.

These papers of Professor Newton's were published in 1864. Before the computations which he had indicated could be attempted, it was necessary that the direction in which the meteors enter the earth's atmosphere should be known much more accurately than it then was, in order to enable astronomers to compute the *exact* forms and positions of the five possible orbits. This observation then was of the greatest importance in 1866, and it was on this account that all the astronomers on that occasion devoted nearly all their efforts to determining with the utmost precision the exact point of the constellation Leo from which the meteors seemed to radiate. This important direction was ascertained during the great meteoric shower on the morning of the 14th of November, 1866, and immediately after Professor Adams and his two assistants in the Cambridge Observatory set to work at their arduous task. This great calculation required the solution of a problem in mechanics which had never before been attempted, and involved an immense amount of tedious labour. Amidst all these difficulties Professor Adams triumphed; and after months of toil he was able to announce in the following March that if the meteors are moving in the large orbit, Jupiter would produce a shifting of the orbit in each revolution amounting to 20′, the attraction of Saturn would add to this 7′, Uranus would add 1′; the effect of the earth and the other planets would be insensible. Adding these numbers together, the whole effect, according to Mr. Adams's computation, is 28′, almost exactly the same as the observed amount which had been determined by Professor Newton, which was 29′. But if the meteors were in any of the other four possible orbits, the total amount would never exceed 12′. Here, then, we have reached the final result: *the long orbit is the orbit of the meteors.* This great discovery was published in March, 1867.

PROFESSOR SCHIAPPARELLI.

Meanwhile Signor Schiapparelli, of Milan, was labouring in another direction. It was evident from the observations that the meteors were drawn out into a long stream. What was the cause of this? Signor Schiapparelli pointed out that if a cloud of meteors were started under conditions which were not quite the same, each meteor would pursue its own orbit, which would differ from the others. If they were treated almost exactly, although not quite, alike at starting, their various orbits would lie excessively close to one another, and would be undistinguishable in most respects. But if there be any effect which goes on accumulating from revolution to

revolution, such an effect would in the end become very sensible. And such an effect there is. The periodic times differ a little in these different orbits. At the end of the first revolution those meteors which have the longest periodic times are the last to get back to the starting point, and have therefore already fallen a little into the rear of the group, while those with the shortest periodic time have gone a little ahead. At the end of the second revolution the separation is doubled, and in each successive revolution the column is still more lengthened out. After a sufficient number of revolutions it will be spread out over the whole length of the orbit, and form a complete oval ring. This has not yet happened to the November meteors, and we are thus assured that it cannot be any enormous period, speaking cosmically, since the time when they first started on their present path. On the other hand the August meteors, which have returned punctually *every* year since they were first observed, are probably a complete ring, and are at all events of far greater antiquity than the November meteors. But they are also, as might be expected, more scattered, so that the sprinkling of meteors they discharge upon the earth as it passes through them has nothing like the splendour of the great November shower. Signor Schiapparelli also pointed out that there is a comet moving in the track of the August meteors, and another in the track of the November meteors. We shall presently see the significance of this observation.

M. LE VERRIER.

The next great step was made by M. Le Verrier, the late Director of the Paris Observatory. Acting on the suggestion made by Sig. Schiapparelli, M. Le Verrier pointed out that the orbit of the meteors intersects the orbit of Uranus, as represented in the diagram. From its inclined position it does not intersect the path of any of the intermediate planets Saturn, Jupiter, and Mars. M. Le Verrier also calculated back the epochs at which the planet and the meteors were at the point of intersection, and found that early in the year A.D. 126 they were both at that spot, but that this has not happened since. Taking this in conjunction with what Sig. Schiapparelli pointed out, we seem to have a clue to a truly wonderful past history. All would be explained if we may suppose that before the year 126, the meteors had been moving beyond the solar system; and that in that year they chanced to cross the path of the planet Uranus, travelling along some such path as that represented in the diagram. Had it not been for the planet they would have kept on the course marked out with a dotted line, and after having passed the sun, would have withdrawn on the other side into the depths of space, to the same measureless distance from which they had originally come. But their stumbling on the planet changed their whole destiny. Even so great a planet would not sensibly affect them until they got within a distance, which would look very short indeed upon our diagram. But they seem to

have almost grazed his surface, and while they were very close to such a planet, he would be able to drag them quite out of their former course. This the planet Uranus seems to have done, and when, pursuing his own course, he again got too far off to influence them sensibly, they found themselves moving slowly backwards, and slowly inwards; and accordingly began the new orbit round the sun, which corresponds to the situation into which they had been brought, and the direction and moderate speed of their new motion.

They seem to have passed Uranus while they were still a small compact cluster. Nevertheless those members of the group which happened to be next the planet as they swept past, would be attracted with somewhat more force than the rest, the farthest members of the group with the least. The result of this must inevitably have been that when the group were soon after abandoned to themselves, they did not find themselves so closely compacted as before, nor moving with an absolutely identical motion, but with motions which differed, although perhaps very little, from one another. These are conditions which would have started them in those slightly differing orbits round the sun, which, as we have seen, would cause them, as time wore on, to be drawn out into the long stream in which we now, after seventeen centuries, find them.

What is here certain is, that there was a definite time when the meteors entered upon the path they are now pursuing—that this time was the end of February or beginning of March in the year 126 is still a matter of probability only. It is, however, *highly* probable, because it explains all the phenomena at present known; but astronomers are not yet in a position to assert that it is ascertained, since one link in the complete chain of proof is wanting. We who live now should be in possession of this link if our ancestors had made sufficiently full observations; and our posterity will have it when they compare the observations they can make with those which we are now carefully placing on record for their use. They will then know whether the rate at which the stream is lengthening out is such as to indicate that A.D. 126 was the year in which this process began. If so, Le Verrier's hypothesis will be fully proved.

MR. STONEY.

Another episode in the eventful history of these meteors is also known with considerable probability. It has been already mentioned that a comet is travelling along the same path as the meteors. It is moving a very little slower than they, and is at present just at the head of the procession which they make through space. Another comet is similarly moving in the track of the great elliptic ring of August meteors. In 1867, the lecturer ventured to suggest an important function which these comets seem to have discharged. Picture to yourselves a mass of gas before it became connected with the solar system, travelling through space at a distance from the sun or any other star. Meteors

would now and then pass in various directions, and with various velocities through its substance. For the most part they would go entirely through and pass out again; but in every such case the meteor would leave the comet with less velocity than it had when approaching it. And in some cases this reduced velocity would be such that the future path of the meteor would be an ellipse round the comet. Whenever this was once brought to pass, the meteor would inevitably return again and again to the comet, each time passing through some part of its substance, and at every passage losing speed. After each loss of speed the ellipse it would next proceed to describe would be smaller than the one before, until at last the meteor would sink entirely into the gas and be engulfed by it. In this way meteor after meteor would settle down through the comet, and, in the end, just such a cluster would be formed as came across the planet Uranus in the year 126, or, if such a cluster existed originally within the mass of gas, it would in this way be augmented. As the comet swept past the planet, its outlying parts would seem to have grazed his surface, and in this way the gas was probably somewhat more retarded than the meteors; and in the centuries which have since elapsed the meteors have gone so much ahead of the comet that they are now treading on his heels and on the point of overtaking him, while probably the gas has again brought together a smaller cluster of the meteors.

PROFESSOR GRAHAM.

The question now arises, How the deserts of space which extend from star to star come to be tenanted here and there by a patch of gas or an occasional meteorite? Light has been thrown on this inquiry by discoveries made with the spectroscope in modern times and by observations during eclipses. These have revealed to us the fact that violent outbursts occur upon the sun, and doubtless on other stars, so swift that the up-rush must sometimes carry matter clear away into outer space. Imagine such a mass consisting in part of fixed gas and in part of condensable vapours ejected from some star. As it travels forward the vapours cool into meteorites, while the fixed gas spreads abroad like a great net, to entangle other meteors. In some cases both might travel together; in others the gaseous portion would be retarded before it passed beyond the neighbourhood of the star, and the denser meteors would get ahead. But even so in the lapse of ages other meteors would be caught, so that in any event a cluster would at length be formed. Now, the reasonable suspicion that this is the real origin of meteors has received striking confirmation from the discovery of the late Professor Graham, that meteoric iron contains so much hydrogen occluded within it as indicates that the iron had cooled from a high temperature in a dense atmosphere of hydrogen—precisely the conditions under which the vapour of iron would cool down while escaping from a large class of stars, including our sun.

RECAPITULATION.

We have now traced an outline of the marvellous history of these Arabs of the sky. We have met with outbursts upon stars sometimes of sufficient violence to shoot off part of their substance. We have found the gaseous portion sweeping through space like a net, and the vapours that accompanied it condensed into spatters that have consolidated into meteorites. We have seen this system travelling through boundless space, with nothing near it except an occasional solitary meteor, and we have seen it in the long lapse of ages slowly augmenting its cluster of these little strangers. As it wandered on it passed within the far-spreading reach of the sun's attraction, and perhaps has since been millions of years in descending towards him. Its natural course would have been to have glided round him in a curve, and to have then withdrawn to the same vast abyss from which it had come; but in attempting this, it became entangled with one of the planets, which dragged it out of its course and then flung it aside. Immediately, it entered upon the new course assigned to it, which it has been pursuing ever since. After passing the planet the different members of the group found themselves in paths very close to one another, but not absolutely the same. These orbits differed from one another very slightly in all respects, and amongst others in the time which a body takes to travel round them. Those meteors which got round soonest found themselves, after the first revolution, at the head of the group; those which moved slowest fell into the rear, and the comet was the last of all. Each succeeding revolution lengthened out the column, and the comet soon separated from the rest. Fifty-two revolutions have now taken place, and the little cloud has crept out into an extended stream, stretching a long way round the orbit, while the comet has fallen the greater part of a revolution behind. We can look forward too, and see that in seventeen centuries more the train will have doubled its length, and that ultimately it will form a complete ring round the whole orbit. When this takes place, a shower of these meteors will fall every year upon the earth, but the swarm will be then so scattered that the display will be far less imposing than it now is.

Such is the history of one of the many meteoric streams which cross the path of the earth. There are several of these streams, and no doubt the story of every one of them is quite as strange. And if there are several streams of meteors, which come across that little line in space which constitutes the earth's orbit, what untold multitudes of them must be within the whole length and breadth of the solar system! Perhaps it may even turn out that the mysterious zodiacal light which attends the sun, is due to countless hordes of these little bodies flying in all directions through the space that lies within the earth's orbit.

POSTSCRIPT.

Professor Newton's Memoirs will be found in 'Silliman's Journal' for 1864, vol. xxxvii. p. 377; and vol. xxxviii. p. 53.

The result of Professor Adams's investigations was announced in the 'Comptes Rendus' of the Academy of Sciences of Paris, of the 25th March, 1867, p. 651; and a fuller account of it will be found in the 'Monthly Notices of the Astronomical Society' for April, 1867, p. 247.

An account of Sig. Schiapparelli's contributions will be found in 'Les Mondes' for December, 1866, and the first quarter of 1867. An outline of them in English, from the pen of Professor Newton, will be found in the 'Philosophical Magazine' for July, 1867, p. 34.

M. Le Verrier's communication was made to the French Academy of Sciences, and is published in the 'Comptes Rendus' of the 21st of January, 1867, p. 94.

Mr. Stoney's paper will be found in the 'Monthly Notices of the Astronomical Society' for June, 1867, p. 271; and in the 'Philosophical Magazine' for September, 1867, p. 188.

Professor Graham's experiments are described in the 'Proceedings of the Royal Society' for May, 1867, vol. xv. p. 502, and in the 'Comptes Rendus' of the Academy of Sciences of Paris of the 27th May, 1867, vol. lxiv. p. 1067.

Professor Newton has recently delivered in America an interesting lecture on "The Relation of Meteorites to Comets," which travels over part of the same ground as the present lecture. A report of it is given in the numbers of 'Nature' for the 6th and 13th of February, 1879.

WEEKLY EVENING MEETING,

Friday, February 21, 1879.

William Spottiswoode, Esq. M.A. D.C.L. Pres. R.S. Vice-President, in the Chair.

Professor Roscoe, LL.D. F.R.S.

A New Chemical Industry, established by M. Camille Vincent.

"After I had made the discovery of the *marine acid air*, which the vapour of spirit of salt may properly enough be called, it occurred to me that, by a process similar to that by which this *acid* air is expelled from the spirit of salt, an *alkaline* air might be expelled from substances containing the volatile alkali. Accordingly I procured some volatile spirit of sal-ammoniac, and having put it into a thin phial and heated it with the flame of a candle, I presently found that a great quantity of vapour was discharged from it, and being received into a basin of quicksilver it continued in the form of a transparent and permanent air, not at all condensed by cold." These words, written by Joseph Priestley rather more than one hundred years ago, describe the experiment by which ammonia was first obtained in the gaseous state.

Unacquainted with the composition of this alkaline air, Priestley showed that it increased in volume when electric sparks are passed through it, or when the alkaline air (ammonia) is heated the residue consists of inflammable air (hydrogen).

Berthollet, in 1785, proved that this increase in bulk is due to the decomposition of ammonia into nitrogen and hydrogen, whilst Henry and Davy ascertained that two volumes of ammonia are resolved into one volume of nitrogen and three volumes of hydrogen.

The early history of sal-ammoniac and of ammonia is very obscure. The salt appears to have been brought into Europe from Asia in the seventh century, probably from volcanic sources. An artificial mode of producing the ammoniacal salts from decomposing animal matter was soon discovered, and the early alchemists were well acquainted with the carbonate under the name of *spiritus salis urinæ*. In later times sal-ammoniac was obtained from Egypt, where it was prepared by collecting the sublimate obtained by burning camels' dung.

Although we are constantly surrounded by an atmosphere of nitrogen, chemists have not yet succeeded in inducing this inert substance to combine readily, so that we are still dependant for our supply of combined nitrogen, whether as nitric acid or ammonia, upon the decomposition of the nitrogenous constituents of the bodies of plants and animals. This may be effected either by natural decay,

giving rise to the ammonia which is always contained in the atmosphere, or by the dry distillation of the same bodies, that is, by heating them strongly out of contact with air; and it is from this source that the world derives the whole of its commercial ammonia and sal-ammoniac.

Coal, the remains of an ancient vegetable world, contains about 2 per cent. of nitrogen, the greater part of which is obtained in the form of ammonia when the coal undergoes the process of dry distillation. In round numbers two million tons of coal are annually distilled for the manufacture of coal gas in this country, and the ammoniacal water of the gasworks contains the salts of ammonium in solution.

According to the most reliable data 100 tons of coal were distilled so as to yield 10,000 cubic feet of gas of specific gravity 0·6, giving the following products, in tons:—

Gas.	Tar.	Ammonia Water.	Coke.
22·25	8·5	9·5	59·75 average.

This ammonia water contains about 1·5 per cent. of ammonia, hence the total quantity of the volatile alkali obtainable from the gasworks in England amounts to some 9000 tons per annum.

A singular difference is observed between the dry distillation of altered woody fibre as we have it in coal, and woody fibre itself. In the products of the first operation we chiefly find in the tar the aromatic hydrocarbons, such as benzene, whilst in the second we find acetic acid and methyl alcohol are predominant.

The year 1848 is a memorable one in the annals of revolutionary chemistry, for in that year Wurtz proved that ammonia is in reality only one member of a very large family. By acting with caustic potash on the nitriles of the alcohol radicals he obtained the first series of the large class of compound ammonias, the primary monamines. Of these methylamine is the first on our list:—

$$\left.\begin{matrix} CH_3 \\ CO \end{matrix}\right\} N + 2\,KOH = \left.\begin{matrix} CH_3 \\ H_2 \end{matrix}\right\} N + CO \left\{\begin{matrix} OK \\ OK \end{matrix}\right.$$

The years that followed, 1849–51, were prolific in ammoniacal discoveries. Hofmann pointed out that not only one atom of hydrogen in ammonia can be replaced by its equivalent of organic radical, but that two or all the three atoms of the hydrogen in ammonia can be likewise replaced, giving rise to the secondary and tertiary amines, by the following simple reactions:—

$$1.\ CH_3I + \left.\begin{matrix} H \\ H \\ H \end{matrix}\right\} N = HI + \left.\begin{matrix} CH_3 \\ H \\ H \end{matrix}\right\} N$$

$$2.\ CH_3I + \left.\begin{matrix} CH_3 \\ H \\ H \end{matrix}\right\} N = HI + \left.\begin{matrix} CH_3 \\ CH_3 \\ H \end{matrix}\right\} N$$

$$3.\ CH_3I + \left.\begin{matrix} CH_3 \\ CH_3 \\ H \end{matrix}\right\} N = HI + \left.\begin{matrix} CH_3 \\ CH_3 \\ CH_3 \end{matrix}\right\} N$$

To these bodies the names of methylamine, di-methylamine, and tri-methylamine were given. They resemble ammonia in being volatile alkaline liquids or gases, which combine with acids to form crystalline and well-defined salts.

Hitherto these compound ammonias have been chemical curiosities; they have, however, recently become, as has so often been the case in other instances, of great commercial importance, and are now manufactured on a large scale.

We are all well aware that the French beet-root sugar industry is one of great magnitude, and that it has been largely extended in late years. In this industry, as in the manufacture of cane sugar, large quantities of molasses or treacle remain behind after the whole of the crystallizable sugar has been withdrawn. These molasses are invariably employed to yield alcohol by fermentation. The juice of the beet, as well as that of cane sugar, contains, in addition to the sugar, a large quantity of extractive and nitrogenous matters, together with considerable quantities of alkaline salts. In some sugar-producing districts the waste-liquors or spent-wash from the stills—called *vinasses* in French—are wastefully and ignorantly thrown away, instead of being returned to the land as a fertilizer, and thus the soil becomes impoverished. In France it has long been the custom of the distiller to evaporate these liquors (*vinasses*) to dryness, and to calcine the mass in a reverberatory furnace, thus destroying the whole of the organic matter, but recovering the alkaline salts of the beet-root. In this way 2000 tons of carbonate of potash are annually produced in the French distilleries. For more than thirty years the idea has been entertained of collecting the ammonia-water, tar, and oils which are given off when this organic matter is calcined, but the practical realization of the project has only quite recently been accomplished, and a most unexpected new field of chemical industry thus opened out, through the persevering and sagacious labours of M. Camille Vincent, of Paris.

The following is an outline of the process as carried out at the large distillery of Messrs. Tilloy, Delaune, and Co., at Courrières. The spent-wash having been evaporated until it has attained a specific gravity of 1·31, is allowed to run into cast-iron retorts, in which it is submitted to dry distillation. This process lasts four hours; the volatile products pass over, whilst a residue of porous charcoal and alkaline salts remains behind in the retort. The gaseous products given off during the distillation are passed through coolers, in order to condense all the portions which are liquid or solid at the ordinary temperature, and the combustible gases pass on uncondensed and serve as fuel for heating the retorts.

The liquid portion of the distillate is a very complex mixture of chemical compounds, resembling in this respect the corresponding product in the manufacture of coal gas. Like this latter, the liquid distillate from the spent-wash may be divided into

1. The ammonia-water.
2. The tar.

The ammonia-water of the vinasse resembles that of the coal-gas manufacture in so far as it contains carbonate, sulphydrate, and hydrocyanide of ammonia; but it differs from this (and approximates to the products of the dry distillation of wood) by containing in addition methyl alcohol, methyl sulphide, methyl cyanide, many of the members of the fatty acid series, and, most remarkable of all, *large quantities of the salts of trimethylamine.*

The tar, on re-distillation, yields more ammonia-water, a large number of oils, the alkaloids of the pyridene series, solid hydrocarbons, carbolic acid, and lastly, a pitch of fine quality.

The crude alkaline aqueous distillate is first neutralized by sulphuric acid, and the saline solution evaporated, when crystals of sulphate of ammonia are deposited; and these, after separating and draining off, leave a mother liquor, which contains the more soluble sulphate of trimethylamine. During the process of concentration, vapours of methyl alcohol, methyl cyanide, and other nitrils are given off, these being condensed, and the cyanide used for the preparation of ammonia and acetic acid by decomposing it with an alkali.

Trimethylamine itself is at present of no commercial value, though perhaps the time is not far distant when an important use for this substance will be found. The question arises as to how this material can be made to yield substances capable of ready employment in the arts. This problem has been solved by M. Vincent in a most ingenious way. He finds that the hydrochlorate of trimethylamine, when heated to a temperature of 260°, decomposes into (1) ammonia, (2) free trimethylamine, and (3) chloride of methyl.

$$3\ NMe_3HCl = 2\ NMe_3 + NH_3 + 3\ MeCl.$$

By bubbling the vapours through hydrochloric acid the alkaline gases are retained, and the gaseous chloride of methyl passes on to be purified by washing with dilute caustic soda and drying with strong sulphuric acid. This is then collected in a gas-holder, whence it is pumped into strong receivers and condensed.

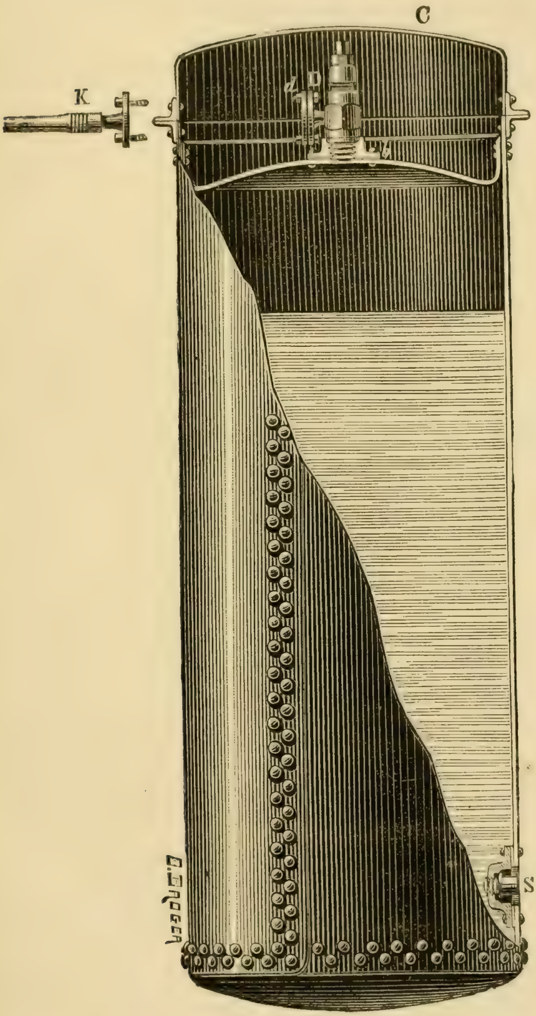
The construction of these receivers is shown in Fig. 1. They consist of strong wrought-iron cylinders, tested to resist a pressure of 20 kilos. per square centimetre, and containing 50, 110, 220 kilos. chloride of methyl. The liquid is drawn from these receivers by opening the screw tap D, which is covered by a cap C, to prevent injury during transit.

Both ammonia and chloride of methyl are, however, substances possessing a considerable commercial value. The latter compound has up to this time, indeed, not been obtained in large quantities, but it can be employed for two distinct purposes: (1) it serves as a means of producing artificial cold; (2) it is most valuable for preparing methylated dyes, which are at present costly, inasmuch as they have hitherto been obtained by the use of methyl iodide, an expensive substance.

Methyl chloride was discovered in 1804 by MM. Dumas and

Péligot, who obtained it by heating a mixture of common salt, methyl alcohol, and sulphuric acid. It is a gas at the ordinary temperature, possesses an ethereal smell and a sweet taste, and its specific gravity

Fig. 1.



is 1·738. It is somewhat soluble in water (about 3 volumes), but much more in acetic acid (40 volumes), and in alcohol (35 volumes). It burns with a luminous flame, tinged at the edges with green,

yielding carbonic and hydrochloric acids. Under pressure, methyl chloride can be readily condensed to a colourless, very mobile liquid, boiling at − 23° C. under a pressure of 760 mm. As the tension of the vapour is not high, and as it does not increase very rapidly with the temperature, the liquefaction can be readily effected, and the collection and transport of the liquefied chloride can be carried on without danger.

The following table shows the tension of chloride of methyl at varying temperatures:—

At	0°	the tension of	CH_3Cl is	2·48	atmospheres.
,,	15°	,,	,,	4·11	,,
,,	20°	,,	,,	4·81	,,
,,	25°	,,	,,	5 62	,,
,,	30°	,,	,,	6·50	,,
,,	35°	,,	,,	7·50	,,

From these numbers we must of course subtract 1 to obtain the pressure which the vapour exerts on the containing vessel.

As a means of producing low temperatures chloride of methyl will prove of great service both in the laboratory and on a larger industrial scale. When the liquid is allowed to escape from the receiver into an open vessel, it begins to boil, and in a few moments the temperature of the liquid is lowered by the ebullition below −23°, the boiling point of the chloride. The liquid then remains for a length of time in a quiescent state, and may be used as a freezing agent. By increasing the rapidity of the evaporation by means of a current of air blown through the liquid, or better by placing the liquid in connection with a good air-pump, the temperature of the liquid can in a few moments be reduced to −55°, and large masses of mercury easily solidified. The construction of a small freezing machine employed by M. Camille Vincent is shown in Fig. 2. It consists of a double-cased copper vessel, between the two casings of which the methyl chloride (A) is introduced. The central space (M) is filled with some liquid such as alcohol, incapable of solidification. The chloride of methyl is allowed to enter from the cylindrical reservoir by the screw tap (B) and the screw (S) left open to permit of the escape of the gas. As soon as the whole mass of liquid has been reduced to a temperature of − 23°, ebullition ceases, the screw (S) may be replaced, and if a temperature lower than −23° be required, the tube (B) placed in connection with a good air-pump. By this simple means a litre of alcohol can be kept for several hours at temperatures either of − 23° or − 55°, and thus a large number of experiments can be performed for which hitherto the expensive liquid nitrous oxide or solid carbonic acid was required.

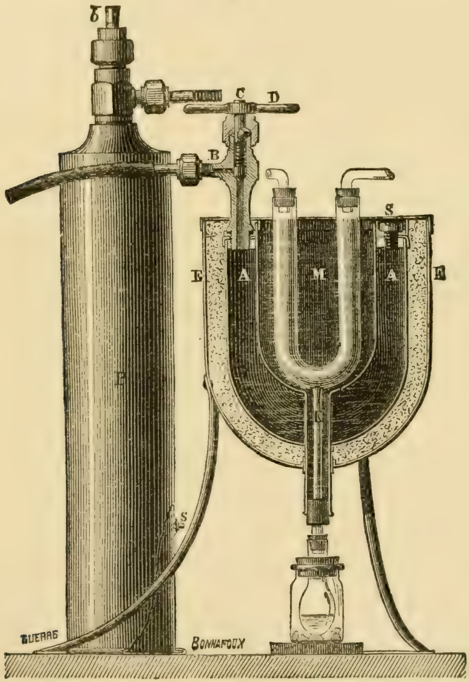
M. Vincent has recently constructed a much larger and more perfect and continuous form of freezing machine, in which by means of an air-pump and a forcing pump the chloride of methyl is evaporated in the freezing machine and again condensed in the cylinders. This enlarged form of apparatus will probably compete favourably with the

ether, and sulphurous acid, freezing machines now in use, as they can be simply constructed, and as the vapour and liquid do not attack metal and are non-poisonous, and as the frigorific effects which it is capable of producing are most energetic.

The second and perhaps more important application of methyl chloride is to the manufacture of methylated colours.

It is well known that rosaniline or aniline-red, $C_{20}H_{19}N_3$, yields compounds possessing a fine blue, violet, or green colour, when a

FIG. 2.



portion of the hydrogen has been replaced by the radicals methyl or ethyl, and the larger the proportion of hydrogen replaced the deeper is the shade of violet which is produced. Thus we have triethyl rosaniline or Hofmann's * violet, $C_{20}H_{16}(C_2H_5)_3N_3$.

By replacing one or two atoms of the hydrogen of aniline by methyl and by oxidizing the methyl anilines thus obtained, Charles Lauth obtained fine violet colours, whilst about the same time Hofmann observed the production of a bright green colouring matter,

* Hofmann, 'Proc. Roy. Soc.' xiii. 13 (1863).

now known as iodine green, formed during the manufacture of the violet, and produced from the latter colour by the action of methyl iodide.

In order to prepare aniline green from the pure chloride of methyl, a solution of methyl-aniline violet in methyl alcohol is placed in an iron digester and the liquid rendered alkaline by caustic soda. Having closed the digester, a given quantity of liquid chloride of methyl is introduced by opening a tap, and the digester thus charged is placed in a water bath and heated by a jet of steam, until the temperature reaches 95°, and the indicated pressure amounts to from 4 to 5 atmospheres. As soon as the reaction is complete, the hot water is replaced by cold, and the internal pressure reduced by opening the screw tap of the digester. The product of this reaction heated and filtered, yields the soluble and colourless base, whose salts are green. To the acidulated solution a zinc salt is added to form a double salt, and the green compound is then precipitated by the addition of common salt. By adding ammonia to a solution of the green salt, a colourless liquid is obtained, in which cloth mordanted with tannic acid and tartar emetic becomes dyed of a splendid green.

If rosaniline be substituted for methyl aniline in the preceding reaction Hofmann's violet is obtained. The application of methyl chloride to the preparation of violets and greens is, however, it must be remembered, not due to M. Vincent; it has been practised for some years by aniline-colour makers. M. Vincent's merit is in establishing a cheap method by which perfectly pure chloride of methyl can be obtained, and thus rendering the processes of the manufacture of colours much more certain than they have been hitherto.

The production of methyl violet from di-methyl aniline, may be easily shown by heating this body with a small quantity of chloral hydrate, and then introducing some copper turnings into the hot liquid. On pouring the mixture into alcohol, the violet colour is well seen.

In reviewing this new chemical industry of the beet-root vinasses, one cannot help being struck by the knowledge and ability which have been so successfully expended by M. Camille Vincent on the working out of the processes.

Here again we have another instance of the utilization of waste chemical products and of the preparation on a large scale of compounds hitherto known only as chemical rarities.

All those interested in scientific research must congratulate M. Camille Vincent on this most successful issue of his labours.

[H. E. R.]

WEEKLY EVENING MEETING,

Friday, February 28, 1879.

C. William Siemens, D.C.L. F.R.S. Vice-President, in the Chair.

Sir William Thomson, LL.D. F.R.S.

The Sorting Demon of Maxwell.

[Abstract deferred.]

GENERAL MONTHLY MEETING,

Monday, March 3, 1879.

The Duke of Northumberland, LL.D. D.C.L. the Lord Privy Seal, President R.I. in the Chair.

Charles William Bell, Esq.
Arthur Brandreth, Esq.
James T. Chance, Esq.
William Crookes, Esq. F.R.S.
Hugh Ernest Diamond, Esq.
Stuart Forster, Esq.
Charles Friend Hardy, Esq.
Donald William Charles Hood, Esq. M.B.
Charles Ed. Jerningham, Esq.
Stephen Lanigan, Esq. B.A.
Daniel Pidgeon, Esq.
Montagu Somes Pilcher, Esq. B.A.
Mrs. Edward Pollock,
Rev. Thomas Cornish Pratt, B.D.
Mrs. Edmund Round,
Mrs. Gertrude Simonds,
Mrs. John Singleton,
John Alexander Swanston, Esq.
Mrs. Michael Wills,

were *elected* Members of the Royal Institution.

The Thanks of the Members were given to Professor A. Graham Bell for his Present of a Pair of Telephones.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

Accademia dei Lincei, Reale, Roma—Atti, Serie Terza, Transunti, Vol. III. Fasc. 1, 2. 4to. 1879.
Asiatic Society, Royal—Journal, New Series, Vol. XI. No. 1. 8vo. 1879.
Astronomical Society, Royal—Monthly Notices, Vol. XXXI. No. 3. 8vo. 1878.
Bavarian Academy of Sciences, Royal—Sitzungsberichte, 1878, Heft 3, 4. 8vo.
British Architects, Royal Institute of—1878–9: Proceedings, Nos. 6, 7, 8. 4to.
Transactions, Nos. 6, 7. 4to.
Chemical Society—Journal for Feb. 1879. 8vo.
Editors—American Journal of Science for Feb. 1879. 8vo.
Analyst for Feb. 1879. 8vo.
Athenæum for Feb. 1879 4to.
Chemical News for Feb. 1879. 4to.
Engineer for Feb. 1879. fol.
Horological Journal for Feb. 1879. 8vo.
Iron for Feb. 1879. 4to.
Journal for Applied Science for Feb. 1879. fol.
Monthly Journal of Science, Feb. 1879.
Nature for Feb. 1879. 4to.
Telegraphic Journal for Feb. 1879. 8vo.
Franklin Institute—Journal, No. 638. 8vo. 1879.
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Geological Society—Quarterly Journal, No. 137. 8vo. 1879.
Gladstone, John Hall, Esq. Ph.D. F.R.S. M.R.I. (*the Author*)—Spelling Reform. Second Edition. 16to. 1879.
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Jablonowski'sche Gesellschaft, Leipsic—Preisschriften, No. 21. 4to 1878.
Linnean Society—Journal, No. 79. 8vo. 1879.
Meteorological Office—Quarterly Weather Report, 1875, Part 4. 4to. 1879.
Photographic Society—Journal, New Series, Vol. III. Nos. 4, 5. 8vo. 1879.
Preussische Akademie der Wissenschaften—Monatsberichte: Nov. 1878. 8vo.
Royal Society of London—Proceedings, No. 192. 1879.
Sandys, R. H. Esq. M.A. (*the Author*)—"In the Beginning." 8vo. 1879.
Society of Arts—Journal for Feb. 1879.
Statistical Society—Journal, Vol. XLI. Part 4. 8vo. 1878.
Symons, G. J.—Monthly Meteorological Magazine, Feb. 1879. 8vo.
Tuson, Professor R. V. (*the Editor*)—Cooley's Cyclopædia of Practical Receipts. Part 10. 8vo. 1879.
Vereins zur Beförderung des Gewerbfleisses in Preussen—Verhandelungen, 1879. Hefte 1, 2. 4to.
Victoria Institute—Journal, Nos. 48, 49. 8vo. 1879.
Winthrop, Robert C. Esq. LL.D. (*the Editor*)—Correspondence of Hartlib, Haak, Oldenburg, and Others of the Founders of the Royal Society of London, with Governor Winthrop of Connecticut. (L 17) Boston, U.S. 8vo. 1878.

WEEKLY EVENING MEETING,

Friday, March 7, 1879.

Sir W. Frederick Pollock, Bart. M.A. Vice-President,
in the Chair.

Professor Huxley, LL.D. F.R.S.

Sensation, and the Uniformity of Plan of Sensiferous Organs.

[Abstract deferred.]

WEEKLY EVENING MEETING,

Friday, March 14, 1879.

William Spottiswoode, Esq. D.C.L. President R.S. Vice-President,
in the Chair.

E. B. Tylor, Esq. D.C.L. F.R.S.

The History of Games.

[Abstract deferred.]

WEEKLY EVENING MEETING,

Friday, March 21, 1879.

The Duke of Northumberland, LL.D. D.C.L. Lord Privy Seal, President, in the Chair.

Professor Abel, C.B. F.R.S.

Recent Contributions to the History of Detonating Agents.

Among the many explosive preparations which have during the last thirty years been proposed as substitutes for gunpowder, on account of greater violence and other special merits claimed for them, not one has yet competed with it successfully as a propelling agent, nor even as a safe and sufficiently reliable explosive agent for use in shells; for industrial applications and for very important military or naval uses, dependent upon the destructive effects of explosives, it has had, however, to give place, to a very important extent, and in some instances altogether, to preparations of gun-cotton and nitro-glycerine.

But there appeared little prospect that either gun-cotton or nitro-glycerine, whether used in their most simple condition or in the forms of various preparations, would assume positions of practical importance as explosive agents of reliable, and therefore uniformly efficient, character, until the system of developing their explosive force through the agency of a detonation, instead of through the simple agency of heat, was elaborated.

Before the first step in this important advance in the application of explosive agents was made by Alfred Nobel, about twelve years ago, the very variable behaviour of such substances as gun-cotton and nitro-glycerine, when exposed to the heat necessary for their ignition under comparatively slight modifications of attendant conditions (e. g. as regards the completeness and strength of confinement or the position of the source of heat with reference to the main mass of the material to be exploded) rendered them uncertain in their action, and at any rate, only applicable under circumstances which confined their usefulness within narrow limits. The employment by Nobel of an initiative detonation, produced by the ignition of small quantities of mercuric fulminate or other powerful detonating substances, strongly confined, for developing the violent explosion, or detonation, of nitro-glycerine, opened a new field for the study of explosive substances, and the first practical fruit was the successful application of plastic preparations of nitro-glycerine and of compact forms of compressed gun-cotton, with simplicity and certainty, to the production of destruc-

tive effects much more considerable than could be accomplished through the agency of much larger amounts of gunpowder, applied under the most favourable conditions. Whereas very strong confinement has been essential for the complete explosion of these substances, so long as the only known means of bringing about their explosion consisted simply of the application of fire or sufficient heat, no confinement whatever is needed for the development, with certainty, of a decidedly more violent explosive action than they are capable of exerting when thus applied, if they are detonated by submitting some small portion of the mass to the blow or concussion developed by a sharp detonation, such as is produced by the ignition of a small quantity of strongly confined mercuric fulminate.

The conditions essential to the development of detonation in masses of nitro-glycerine and gun-cotton, or preparations of them, and the relations to and behaviour towards each other of these and other explosive bodies, in their character or functions as detonating agents, have been made the subject of study by the lecturer during the last ten years, and some of the earlier results published by him in connection with this subject also led to the pursuit of experimental inquiries of analogous character by Champion and Pellet and others.

Some of the chief results attained by Mr. Abel's experiments may be briefly summarized.

It was found that the susceptibility to detonation, as distinguished from explosion, through the agency of an initiative detonation, is not confined to gun-cotton, nitro-glycerine, and preparations containing those substances, but that it is shared, though in very different degrees, by all explosive compounds and mixtures.

It was demonstrated that the detonation of nitro-glycerine and other bodies, through the agency of an initiative detonation, is not ascribable simply to the direct operation of the heat developed by the chemical changes of the charge of detonating material, and that the remarkable property possessed by the sudden explosion of small quantities of certain bodies (the mercuric and silver fulminates) to accomplish the detonation of nitro-glycerine and gun-cotton, is accounted for satisfactorily by the mechanical force thus suddenly brought to bear upon some part of the mass operated upon. Most generally, therefore, the degree of facility with which the detonation of a substance will develop similar change in a neighbouring explosive substance, may be regarded as proportionate to the amount of force developed *within the shortest period of time* by that detonation, the latter being in fact analogous in its operation to that of a blow from a hammer or of the impact of a projectile.

Thus, explosive substances which are inferior to mercuric fulminate in the suddenness, and the consequent momentary violence of their detonation, cannot be relied upon to effect the detonation of gun-cotton, even when used in comparatively considerable quantities. Percussion cap composition, for example, which is a mixture of fulminate with potassium chlorate, and is therefore much less rapid

in its action than the pure fulminate, must be used in comparatively large quantities to accomplish the detonation of gun-cotton.

The essential difference between an explosion and what we now distinguish as a detonation lies in the comparative suddenness of the transformation of the solid or liquid explosive substance into gas and vapour.

The gradual nature of the explosion of gunpowder is illustrated, in its extreme, by burning a train of powder in open air; the rapidity and consequent violence of the explosion is increased in proportion to the degree of confinement of the exploding charge, or to the resistance opposed to the escape or expansion of the gases generated upon the first ignition of the confined substance. In proportion as the pressure is increased under which the progressive transformation of the explosive takes place, the rapidity with which its particles are successively subjected to the action of heat is increased.

In the case of a very much more sensitive and rapidly explosive substance than gunpowder, such as mercuric fulminate, the increase in the rapidity of its transformation, by strong confinement, is so great that the explosion assumes the character of a detonation in regard to suddenness and consequent destructive effect. A still more sensitive and rapidly explosive material (such as the silver fulminate and iodide of nitrogen) produces when exploded in open air effects akin to those of detonation; yet even with these bodies, confinement operates in increasing the rapidity of the explosive to suddenness, and consequently in developing a more purely detonative action. Thus, the violence of explosion of silver fulminate is decidedly increased by confining the substance in a stout metal case, and the enclosure of iodide of nitrogen in a shell of plaster of paris has a similar effect. With chloride of nitrogen, the suddenness of detonation, and consequently the violence of action, was found to be very greatly increased even by confining the liquid beneath a thin layer of water.

Detonation, developed in some portion of a mass, is transmitted with a velocity approaching instantaneousness throughout any quantity, and even if the material is laid out in the open air in long trains composed of small masses. The velocity with which detonation travels along trains thirty or forty feet in length, composed of distinct masses of gun-cotton and of dynamite, has been determined by means of Noble's chronoscope, and was found to range from 17,000 to 24,000 feet per second. Even when trains of these explosive agents were laid out with intervening spaces of half an inch between the individual masses composing the trains, detonation was still transmitted along the separated masses with great though diminished velocity.

The suddenness with which detonation takes place has been applied as a very simple means of breaking up shells into small fragments and scattering these with considerable violence, with employment of very small charges of explosive agent. Thus by filling a 16-pr. common shell completely with water and inserting a charge

of $\frac{1}{2}$ oz. of gun-cotton fitted to a detonating fuze, the shell being thoroughly closed by means of a screw plug, the force developed by the detonation of the small charge of gun-cotton is transmitted instantaneously in all directions by the water, and the shell is thus broken up into a number of fragments averaging fourteen times the number produced by bursting a shell of the same size by means of the full amount of powder which it will contain (13 oz.). Employing 1 oz. of powder, in place of $\frac{1}{2}$ oz. of gun-cotton, in the shell filled with water, the comparatively very gradual explosion of the powder charge is rendered evident by the result; the shell being broken up into less than twenty fragments by the shock produced by the first ignition of the charge, transmitted by the water. In this case the shell is broken up by the minimum amount of force necessary for the purpose, before the explosive force of the powder charge is properly developed. Extensive comparative experiments carried on not long since by the Royal Artillery, at Okehampton, demonstrated that this simple expedient of filling common shells with water and attaching a small charge of gun-cotton with its detonator to the fuze usually employed, allowed of their application as efficient substitutes for the comparatively complicated and costly shrapnel and segment shells.

Another illustration of the sharpness of action developed by detonation as compared with explosion, consequent upon the almost instantaneous character of the metamorphosis which the explosive agent undergoes in the case of detonation, is afforded by a method which the lecturer applied some years since for comparing the violence of action of charges of gun-cotton and of dynamite arranged in different ways. The charges (5 lb.) to be detonated were freely suspended over the centres of plates of very soft steel of the best quality, which rested upon the flat face of a massive block, or anvil, of iron, having a large central circular cavity. The distance between the upper surface of the plate and the charge suspended over it, was 4 feet. The sharp blow delivered upon the plate by the air suddenly projected against it by the force of the detonation when the charge was fired, forced the metal down into the cavity of the anvil, producing cup-shaped indentations, the dimensions of which afforded means of comparing the violence of the detonation. A much larger charge of powder exploded in actual contact with the plate, would produce no alteration of form in the metal, and the same negative result would be furnished by the explosion over the plate of a heap of loose gun-cotton of the same or greater weight than the charges detonated. The above method of experiment was devised, in the first instance by Mr. Abel, in July 1875, for comparing the quality of some specimens of Llandore steel proposed to be used by the Admiralty for ship-building purposes, with samples of malleable iron, and it has since been employed by Mr. Adamson in carrying out a very useful series of experiments, recently communicated to the Iron and Steel Institute.

It has been stated that detonation can be transmitted from one mass of gun-cotton or dynamite to another through intervening air-spaces. The extent to which such spaces can be introduced without checking detonation is obviously regulated by the size of the masses of explosive detonated; but the distances of air-space through which the detonation of a moderate quantity of the explosive agent will communicate to similar masses, are very limited, a space of 2 inches being sufficient to prevent the detonation produced by a mass of 8 oz. of gun-cotton, freely exposed, from communicating to contiguous ones. If the dispersion of the force is prevented in part, and direction is given to the gases violently projected from the centre of detonation, the power of transmitting detonation to separated masses of explosive is increased to a remarkable degree. This is readily accomplished through the agency of tubes, the charge first detonated being just inserted into one extremity, while that to which the detonation is to be transmitted is inserted into the other; or separate charges may be placed at different distances inside a long tube, with long intervening spaces, the initiative charge being inserted at one end. A few illustrations of the results thus obtained may be given. The detonation of a 1-oz. disk of gun-cotton in open air will not transmit detonation with *certainty* to other disks placed at a greater distance than half an inch from it; but if it be just inserted into one end of an iron tube 2 feet long and 1·25 inch in diameter, a similar disk, or even a plug of loose gun-cotton inserted into the other extremity of the tube, will invariably be detonated. With employment of 2 oz. of gun-cotton, in a tube of the same material, thickness, and diameter, detonation was transmitted to a distance of 5 feet. In tubes of the same kind, of very considerable length, 2-oz. disks of gun-cotton placed at intervals of 2 feet, were detonated through the initiative detonation of one such disk inserted into one extremity of the tube. In other experiments a long tube of this kind was fitted with branch pipes, 2 feet long, at those parts where the intermediate disks were placed, and charges of gun-cotton were placed at the extremities of these pipes. By the initiative detonation of 1 oz. of gun-cotton all the charges were detonated, the effect on the air being that of one single explosion. The results obtained with equal quantities of gun-cotton varied with the diameter, strength, and nature of the material of the tubes used. Dynamite and mercuric fulminate, applied to their own detonation, furnished results quite analogous to those obtained with gun-cotton; but in applying fulminate to the detonation of gun-cotton through the agency of tubes, some singular and instructive results were obtained, for an account of which the lecturer referred to his Memoir on this subject.

Silver fulminate was employed for the purpose of instituting more precise experiments than could be made in operating on a larger scale, with gun-cotton, on the influence of the material composing the tubes, of the condition of their inner surfaces, and of

other variable circumstances, upon the transmission of detonation. Half a grain of silver fulminate freely exposed and ignited by a heated body, will transmit detonation to some of the compound placed at a distance of 3 inches from it, but does not do so with certainty through a distance of 4 inches. But when the quantity of the fulminate is just inserted into one end of a stout glass tube 0·5 inch in diameter, and 3 feet long, its detonation is invariably induced by that of a similar quantity of the fulminate placed just inside the other extremity of the tube; this result is uncertain when the length of the tubes of the same thickness and diameter exceeds 3 feet 3 inches. Glass tubes were found to transmit the detonation of silver fulminate much more rapidly than tubes of several other materials of the same diameter and thickness of substance. Thus, with the employment of double the quantity of fulminate required to transmit the detonation with certainty through a glass tube of the kind described, 3 feet in length, it was only possible to obtain a similar result through a pewter tube 31·5 inches long, a brass tube 23·7 inches long, an indiarubber tube 15·8 inches long, and a paper tube 11·8 inches long. The difference in the results obtained was not ascribable to a difference in the escape of force on the instant of detonation, in consequence of the fracture of the tube, nor to the expenditure of force in work done upon the tube at the seat of detonation, since the glass tubes were always destroyed by the first explosion to a much greater distance along their length than any of the others, and the brass tubes, which were in no way injured at the seat of the explosion, did not transmit detonation to so great a distance as the pewter tubes, which were always deeply indented. The transmission of detonation appeared also not to be favoured by the sonorosity or the pitch of the tube employed, as the sonorous brass tube was not found to favour the transmission to the same extent as the pewter tube. Moreover the transmission of detonation by the glass tubes was not found to be at all affected by coating these tubes with several layers of paper, or by encasing them in tightly fitting indiarubber tubes. These differences appeared on further investigation not to be ascribable, to any important extent, if at all, to the difference in the nature of the material composing the tubes, but to be simply, or at any rate almost entirely, due to differences in the condition of the inner surfaces of the tubes. Thus, brass tubes, the inner surfaces of which were highly polished, and paper tubes, when coated inside with highly glazed paper, transmitted the detonation of the silver fulminate to about the same distance as the glass tubes; on the other hand, when the inner surfaces of the latter were slightly roughened by coating them with a film of fine powder, such as French chalk, they no longer transmitted detonation to anything like the distance which they did when the inner surfaces were in the normally smooth condition. Other very slight obstacles to the unimpeded passage of the gas wave through the tubes were found greatly to reduce the facility with which detonation could be transmitted by

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means of tubes; thus, when a diaphragm of thin bibulous paper was inserted into the glass tube about half-way between the two extremities, detonation was not transmitted, even with the employment of about six times the quantity of fulminate that gave the result with certainty under ordinary conditions; and similarly the transmission of detonation by increased charges of mercuric fulminate and of gun-cotton was prevented by the introduction into the tubes of light tufts of carded cotton wool just sufficient in quantity to shut out the light in looking through the tubes.

Among several other interesting results furnished by an examination into the conditions governing and results attending the transmission of detonation by tubes, a remarkable want of reciprocity was found to exist between mercuric fulminate and gun-cotton. The latter substance is more susceptible to the detonative power of mercuric fulminate than of any other substance, as will presently be further shown. The quality of fulminate required to detonate gun-cotton is regulated by the degree to which the sharpness of its own detonation is increased by the amount of resistance to rupture offered by the envelope in which the fulminate is confined. From 20 to 30 grains are required if the detonative agent is confined in a thin case of wood, or in several wrappings of paper; but as small a quantity as 2 grains of the fulminate suffices to effect the detonation of compressed gun-cotton, provided the fulminate be confined in a case of stout metal (sheet tin) and be closely surrounded by being tightly imbedded in the mass of gun-cotton. If there be no close contact between the two, the quantity of fulminate must be very considerably increased to ensure the detonation of the gun-cotton, and, in attempting to transmit detonation from mercuric fulminate to gun-cotton by means of tubes, it was found necessary to employ comparatively very large quantities of fulminate in order to accomplish this, even through short lengths of tubes. But when the quantity of fulminate used reaches certain limits, the detonation may be transmitted from it to gun-cotton through very long lengths of tube. In applying gun-cotton, on the other hand, to accomplish the detonation of mercuric fulminate, it was found that this result could be attained, and through considerable lengths of tube (7 feet and upwards) by means of very much smaller quantities of gun-cotton than is needed of fulminate to induce the detonation of gun-cotton through the corresponding distances.

This want of reciprocity between two detonating agents corresponds to one even more remarkable, which was observed by the lecturer in his earlier investigations on this subject. In the first place it was found that the detonation of $\frac{1}{4}$ oz. of gun-cotton (the smallest quantity that can be thus applied) induced the simultaneous detonation of nitro-glycerine, enclosed in a vessel of sheet tin and placed at a distance of 1 inch from the gun-cotton; while with $\frac{1}{2}$ oz. of the latter, the same effect was produced with an intervening space

of 3 inches between the two substances. But on attempting to apply nitro-glycerine to the detonation of gun-cotton, the quantity of the former, which was detonated in *close contact* with compressed gun-cotton, was gradually increased in the first instance to $\frac{3}{4}$ oz. and subsequently even to 2 oz. without accomplishing the detonation of the latter, which was simply dispersed in a fine state of division, in all instances but one in a large number of experiments.

The force developed by the detonation of nitro-glycerine was found, by careful comparison of the relative destructive effects of corresponding quantities, to be decidedly greater than that of the fulminate, of which from 2 to 5 grains suffice for developing the detonation of gun-cotton, when it is in close contact with them. The non-susceptibility of gun-cotton to detonation by nitro-glycerine is therefore, it need scarcely be said, not ascribable to any deficiency in mechanical force suddenly applied when the nitro-glycerine is detonated.

That the power possessed by different very highly explosive substances, of inducing the detonation of such bodies as gun-cotton and nitro-glycerine, is not solely ascribable to the operation of mechanical force very suddenly developed, is indicated not only by the singular inertness of gun-cotton to the influence of nitro-glycerine as a detonating agent, but also by a comparison of the behaviour of other detonating substances with that of the mercuric fulminate, when applied to the detonation of gun-cotton. Thus the detonation of silver fulminate is very decidedly sharper than that of the mercury compound, yet it is in no way superior to the latter in its power as an initiative detonating agent; indeed, a somewhat larger amount of it appeared to be required than of the mercury salt to induce detonation of gun-cotton with certainty. Again, the iodide and chloride of nitrogen are far more susceptible of sudden detonation than the silver fulminate; yet while 5 grains of the latter, confined in a stout metal envelope, suffice to detonate gun-cotton, 50 grains of chloride of nitrogen confined by water, appeared to be the minimum amount with which the detonation of gun-cotton could be accomplished with certainty, while no success attended the employment of confined iodide of nitrogen in quantities ranging up to 100 grains.

The incompatibility of these results with the general conclusion, based upon numerous and greatly varied experiments, that the facility with which the detonation of gun-cotton and nitro-glycerine, and bodies of a similar character as explosives, is induced by an initiative detonation, is proportionate to the mechanical force aided by the heat developed by the latter, led the lecturer to the conclusion that a synchronism or similarity in character or quality of the vibrations developed by the detonation of particular substances, operates in favouring the detonation of one such substance by the initiative detonation of a small quantity of another, while in the absence of such synchronism, a much more powerful detonation, or the application of

much greater force, would be needed to effect the detonation of the material operated upon. This view has received considerable support from results since obtained by other experimenters, especially by MM. Champion and Pellet; but the subject is one which still needs further experimental elucidation.

The physical character of explosive substances, as also the mechanical condition of a mass of the particular explosive substance operated on, are of great influence in determining its behaviour when submitted to the action of an initiative detonation. The liquid nitro-glycerine is far more sensitive to detonation than gun-cotton; one grain of mercuric fulminate, confined in a metal case, suffices to detonate nitro-glycerine when surrounded by it: but, in order to attain this result with any degree of certainty, it is necessary so to confine the nitro-glycerine as to prevent its yielding to the blow developed by the initiative detonation, and thus to some extent escaping from the operation of the sudden concussion to which the particles contiguous to the fulminate charge are submitted.

If nitro-glycerine be mixed with solid substances in a fine state of division, plastic mixtures may be obtained, and the liquid may thus be presented in something like a solid form to the detonating agent; if the particles of absorbent material be moreover of porous nature, as is the case with the infusorial earth called Kieselguhr used in the production of dynamite, a solid nitro-glycerine preparation may be obtained which contains a very large proportion of the liquid (75 per cent. by weight). In this condition nitro-glycerine may be detonated without any difficulty when freely exposed to air; and although it is diluted with a considerable proportion of absolutely inert material, its sensitiveness to detonation is not in the least diminished. Each particle of the diluent is enveloped in the liquid, so that no portion of the latter becomes isolated from the remainder by the admixture of inert solid matter; hence, when the initiative detonator is surrounded by such a mass, it is in contact at all points with some portion of the nitro-glycerine, and the latter is in continuous connection throughout, though no longer in a mobile condition; detonation is consequently as readily established and transmitted through the mass as though it consisted entirely of nitro-glycerine. Indeed, while the liquid in its undiluted state, if freely exposed to air in a long layer, transmits detonation with difficulty, and very slowly as compared with compressed gun-cotton (the observed rate of progression being, in several experiments, below 6000 feet per second), detonation is transmitted with ease and certainty through very long trains of a solid preparation of nitro-glycerine, such as dynamite, and the rate of transmission is decidedly more rapid than it is with compressed gun-cotton, a result which is in harmony with the greater sensitiveness to detonation and the greater violence of action of nitro-glycerine.

It has already been stated that gun-cotton may be detonated if a confined charge of not less than 2 grains of mercuric fulminate be

detonated when closely surrounded by the substance. But in order to attain this result, the cellulose-product must be presented to the detonating agent in a mechanical condition favourable to its action.

Gun-cotton in a loose flocculent condition, or even if in the more compact form of a spun yarn or thread, cannot be detonated through the agency of a large charge of fulminate buried in the material. The light and loose gun-cotton is simply scattered with violence; portions are sometimes inflamed by the heat developed where the fulminate is detonated, a result which is obtained with greater certainty the less violent the detonation produced by the fulminate-charge. If, however, the gun-cotton be converted into a compact form, either by ramming the wool or thread very tightly into a case, or better still, by reducing the gun-cotton fibre to a very fine state of division, and compressing it, when in that condition, into compact masses, it becomes susceptible of detonation by the initiative action of mercuric fulminate, and the quantity of the latter required to bring about detonation is small (down to the limit which has been named above) in proportion as the compactness or density of the compressed material is increased.

Detonation, when established in compressed gun-cotton, is transmitted with great velocity throughout the mass, as already stated, or from one to another of contiguous masses, laid out in long rows, and even, though at a reduced rate, if small spaces exist between the individual masses. But, if a small mass of compressed gun-cotton freely exposed to air be detonated when in immediate contact with gun-cotton wool or loosely twisted yarn, the detonation will not be transmitted to these, but they will merely be scattered and perhaps inflamed.

The difference in the behaviour of nitro-glycerine and of gun-cotton when presented to the action of a so-called initiative detonation under the different conditions spoken of above, admits of ready explanation.

It was established, in the first instance, that the action of an initiative detonation is not ascribable to the heat developed within the denoting material itself, in undergoing chemical metamorphosis. If it were so, the detonating mixture known as percussion cap composition and other explosive mixtures, the detonation of which is attended by much greater development of heat than is obtained by the action of pure mercuric fulminate, should detonate gun-cutton more readily than the latter does, whereas very much larger quantities of such materials are required to attain that result; moreover, the readiness with which gun-cotton is detonated should be solely proportionate to the *amount* of fulminate used, which has been shown not to be the case; and gun-cotton should be more readily detonated when in the loose and open condition than in the highly compressed or compact form, because the latter presents it in the condition least favourable, and the former in that most favourable, to ready and rapid transformation by heat. Again, the actual temperature required for the explosion of nitro-glycerine is very considerably above the

exploding temperature of gun-cotton, yet a very much smaller charge is required for the detonation of nitro-glycerine than is needed for the detonation of gun-cotton. On the other hand, a quantity of confined percussion cap composition which, if it were pure mercuric fulminate, would be altogether inadequate for the detonation of gun-cotton, suffices for the detonation of nitro-glycerine.

The action of an initiative detonation has already been compared to that of a blow from a hammer or falling weight. The readiness and certainty with which gunpowder, gun-cotton, and other explosive agents are detonated by the latter agency are regulated by several circumstances; they are in direct proportion to the weight of the falling body, to the height of its fall, and to the force with which it is impelled downwards; to the velocity of its motion; to the mass and rigidity or hardness of the support upon which the substance to be detonated rests; lastly, to the quantity and mechanical condition of the explosive agent struck, and to its sensitiveness.

Gunpowder is much more readily detonated by a sharp blow from a small hammer, than by the simple fall of a heavy hammer, or by a comparatively weak blow from the latter. It is very difficult by repeated blows, applied at very brief intervals, to detonate gun-cotton if placed upon a support of wood or lead, both of which materials yield to a blow, the force applied by that blow being transferred through the explosive agent and absorbed in work done upon the material composing the support. But if the latter be of iron, which does not yield permanently to the blow of the hammer, the detonation of those substances is easily accomplished. If the quantity of the explosive agent employed be so considerable as to form a thick layer between the hammer and support, the force applied is to so great an extent expended in imparting motion to the particles of the compressible mass, that there remains little or none by which its detonation can be accomplished, and if the material be in a loose or porous condition (as in the case of a powder or of loose wool), much work has to be accomplished in moving particles of the mass through a comparatively considerable space, in the operation of compressing them, so that a second or even a third blow is required for their detonation; whereas if, by blows or pressure previously applied, the explosive material will be presented in the form of a compact mass, the particles of which have little tendency to motion when force is applied to them, detonation will be much more readily developed. It appears therefore that the detonation of an explosive substance by means of a blow is the result of the development of heat sufficient to bring about most energetic chemical action, or change, by expenditure of force in the compression of the material, or by establishing violent friction between its particles, consequent upon the motion momentarily imparted to them, and that it is brought about with a readiness proportionate to the resistance which they oppose to their motion by the degree of their contiguity to each other.

The exceedingly violent motion of particles resulting from the

sudden or extremely rapid transformation of a solid or liquid explosive body into highly heated gas or vapour (which is the effect of a detonation), must obviously exert force which operates upon a body opposed to it in a manner precisely similar to the force applied by opposing a body in the path of a solid mass which is set into very rapid motion. In other words, a detonation exerts a mechanical effect upon resisting bodies precisely similar to that of a blow from a hammer or from a projectile propelled from a gun. Just as the force of a sufficiently sudden or powerful blow from a hammer is transformed into heat by the resistance to the motion of the hammer which the particles of an opposing body offer, and by the consequent friction established between them, so the force or concussive action exerted by the matter set in motion when a solid or liquid is converted into gas or vapour, will also be transformed into heat, the development of which in an opposing body will be proportionate to the resistance to motion which its particles offer, and to the suddenness and violence of the concussion to which it is subjected. The power of accomplishing the detonation of nitro-glycerine, gun-cotton, or other highly explosive substances, freely exposed to the air, through the agency of detonation produced in their vicinity or in close contact with them, appears therefore correctly ascribable to the heat suddenly developed in some portion of the mass by the mechanical effect, or blow exerted by that detonation, and is regulated by the violence and suddenness (either singly or combined) of the detonation, by the extent to which the particles composing the mass of the explosive material are in a condition to oppose resistance to the force, and by the degree of sensitiveness of the substance to detonation, or to sudden metamorphosis, under the influence of heat thus developed.

It will now be evident why the readily yielding nature of the particles of liquid nitro-glycerine tends to counteract its great sensitiveness to detonation, and why, when the motion of the liquid particles is impeded by their admixture with solid matter, and when they are consequently placed in a position to resist mechanical motion by the force applied through the agency of detonation, its natural sensitiveness to detonation, and the rapidity with which it can be transmitted from particle to particle became fully developed.

Again, the reduction of gun-cotton fibre to a fine state of division, which renders the material readily convertible into very compact and dense masses, places the particles in the condition most favourable to resist mechanical motion upon the application of a blow, or of the concussion resulting from a detonation ; hence, compressed gun-cotton is readily susceptible of detonation in proportion to the extent of compression, or to its density and compactness, while loose gun-cotton wool, or the lightly twisted or compressed material cannot be readily detonated, because the force applied is expended in imparting motion to the readily yielding particles of the mass. If the force applied through the agency of a detonator to a mass of explosive material just borders

upon that required for the development of the detonation, or if the condition of the mass is such as hardly to present the requisite resistance to mechanical motion essential for its detonation, then, results intermediate between the mechanical dispersion of the mass and its violent chemical dispersion or disintegration, i. e. detonation, are obtained. Thus, frequent instances have been observed, especially in the experiments in the transmission of detonation through tubes, in which the initiative detonation has brought about an explosion attended with little, if any, destructive effect, portions of the mass being at the same time dispersed and occasionally inflamed. Not only have such results often been obtained with gun-cotton and dynamite, but even mercuric fulminate, exposed to the concussion of a distant detonation transmitted through a tube, has frequently been exploded in a manner quite distinct from the violent detonation developed in other instances. Silver fulminate, which under ordinary conditions detonates violently, even when only a particle of the mass is subjected to a sufficient disturbing influence, has been exploded without the usual demonstrations of force, by the transmitted effect of a detonation of mercuric fulminate. In these instances the violence of the concussion produced by the initiative detonation was only just bordering on that required for the development of detonation, and it appears probable that only some small portion of the mass operated upon was in a condition or position favourable to the action of the initiative blow. The remainder of the mass would then be dispersed by the gases developed from the detonated portion; in some instances the particles would be inflamed at the moment of their dispersion, in others, they would even escape ignition. The latter appears to be always the case when gun-cotton is exploded by a blow from a hammer or falling weight. However carefully the arrangements are adjusted with a view to distribute such a blow uniformly over the entire mass struck, the concentration of a preponderance of the force applied upon some portion or portions of the entire mass, appears almost inevitable; hence only a small portion is actually detonated, the remainder being instantaneously dispersed by the gases suddenly generated while the weight is still resting upon the support.

Some experiments made in firing at masses of compressed gun-cotton, differently arranged and of different thicknesses, with a Martini-Henry rifle, at short ranges, afforded interesting confirmation of the correctness of the explanation given of the operation of a blow upon masses of explosive material under different conditions. Disks of gun-cotton of the same density and diameter, but differing in thickness, were fired at; they were freely suspended, and their distance from the marksman was in all instances 100 yards. The thinnest disks were simply perforated by the bullets, not a particle of the gun-cotton being ignited. Somewhat thicker disks were inflamed by the impact of the bullet, while still thicker disks, fired at under the same conditions, were exploded, portions being in some instances dispersed in a burning state. No instance of detonation was, however,

obtained. These differences in effect, obtained with masses of different thickness and weight, are due to the difference in their power to resist mechanical motion when struck by the bullet, and in the different amount of resistance to penetration presented by the thin and the thicker disks.

It has been explained that nitro-glycerine may be largely diluted with inert solid matters without its sensitiveness to detonation being reduced, while its detonation in open air becomes very much facilitated, because the mobility of the particles, and their consequent tendency to yield to the force of a blow or detonation, is very greatly diminished. But if a *solid* explosive agent is diluted with inert *solid* matter the case is different; for in such a mixture of the finely divided solid with non-explosive solid particles, there must be a partial and sometimes a complete separation of the particles of the explosive by the interposed inert particles with which it is diluted; hence the sensitiveness to detonation is reduced, and its transmission by the particles is retarded or altogether impeded, by a diminution of the extent of contact between the substance to be detonated and the initiative detonation, and by the barrier which the interposed non-explosive particles oppose to the transmission of detonation. Thus a mixture of mercuric fulminate with more than one-fifth its weight of French chalk could not be detonated by means of one grain of pure fulminate enclosed in a copper capsule, which was inserted into the mixture; that quantity, similarly confined, sufficed to detonate undiluted fulminate through a tube 8 inches long and 0·5 inch in diameter. In experiments made in this direction with finely divided gun-cotton, it was found that although dilution with an inert solid, applied in the *solid form*, reduced the sensitiveness of the material to detonation, this was not the case when it was incorporated with a salt soluble in water, the mixture being then compressed while in the wet state. The compressed masses thus obtained were, when dried, in a condition of greater rigidity than could be attained by submitting undiluted gun-cotton to considerably more powerful pressure, because the crystallisation of the soluble salt used as the diluent upon evaporation of the water, cemented the particles composing the mass more rigidly together. The gun-cotton was therefore presented in a form more capable of resisting the mechanical action of a small charge of fulminate, than a more highly compressed undiluted gun-cotton, and hence the reduction in sensitiveness due to the detonation of the explosive compound is nearly counterbalanced by the greater rigidity imparted to the mass. If a soluble oxidising agent (a nitrate or chlorate) be employed as the diluting material, the predisposition to chemical reaction between it and the gun-cotton (which is susceptible of some additional oxidation), appears to operate in conjunction with the effect of the salt in imparting rigidity to the mixture, thus rendering the latter quite as sensitive to the detonating action of the minimum fulminate charge as undiluted gun-cotton. Moreover, the interesting fact has been conclusively established, that these compressed mixtures

of gun-cotton with a nitrate or a chlorate are much less indifferent to the influence of detonating nitro-glycerine than gun-cotton in its pure state. Chlorated and nitrated gun-cotton are detonated with certainty by means of $\frac{1}{2}$ oz. of nitro-glycerine, whereas the detonation of 2 oz. of the latter accomplished the detonation of ordinary compressed gun-cotton only once in a large number of experiments.

If compressed gun-cotton is diluted by impregnating the mass with a *liquid*, or with a solid which is introduced into the mass in a fused state, its susceptibility of detonation is reduced to a very much greater extent, than by a corresponding quantity of a solid inert body, incorporated as such with the gun-cotton, the cause being the converse of that which operates in preventing a reduction of the sensitiveness to detonation of nitro-glycerine by its dilution with an inert solid. In this case, the explosive liquid envelopes the solid diluent, and remains continuous throughout, occupying the spaces which exist between the solid particles; hence detonation is readily established and transmitted. But in the case of the solid explosive, the diluent, which is liquid, or at any rate is introduced into the mass in the liquid state, envelopes each particle of the solid, so that a film of inert material surrounds each, isolating it from its neighbours, and thus opposing resistance to the transmission of detonation, which is proportionate to the original porosity or absorbent power of the mass.

While compressed gun-cotton, in the air-dry state, is detonated by 2 grains of mercuric fulminate imbedded in the material, its detonation by 15 grains, applied in the same manner, becomes doubtful when it contains 3 per cent. of water, over and above the 2 per cent. which exists normally in the air-dry substance. Specimens which had been impregnated with oil or soaked in melted fat and allowed to cool, could not be detonated by means of 15 grains of fulminate. These diluted samples of gun-cotton could only be detonated by adding very considerably to the power of the initiative detonation ; 100 grains of confined fulminate generally failed to detonate gun-cotton containing from 10 to 12 per cent. of water, and if the amount reached 17 per cent., 200 grains of fulminate were needed to ensure its detonation.

But moist or wet compressed gun-cotton is decidedly more susceptible of detonation by (dry) compressed gun-cotton itself than by mercuric fulminate.

Thus 100 grains of dry gun-cotton, detonated through the agency of the ordinary fulminate fuze, suffice to detonate wet gun-cotton containing 17 per cent. of water, though this result is somewhat uncertain. If the diluting agent amounts to 20 per cent., detonation is not certain with less than 1 oz. of dry gun-cotton, and if the compressed material be completely saturated with water (i.e. containing 30 to 35 per cent.), 4 oz. of the air-dry substance, applied in close contact, are needed to ensure its detonation.

Detonation is transmitted through tubes from dry compressed gun-cotton to a moist disk of the material with the same facility as to the dry substance; and this is also the case with regard to the

propagation of detonation from one mass of moist gun-cotton to another, in open air, all the pieces being ranged in a row, in contact with each other, provided that the piece first detonated does not contain less water than the others to which detonation is transmitted. Some curious results, obtained in experiments on the transmission of detonation, with gun-cotton containing different proportions of water, appeared to indicate that the character or quality of detonation developed by gun-cotton is subject to modification by the proportion of water which the latter contains.

Gun-cotton containing 12 to 14 per cent. of water is ignited with much difficulty on applying a highly heated body. As it leaves the hydraulic press upon being converted from the pulped state to masses having about the density of water, it contains about 15 per cent. of water; in this condition it may be thrown on to a fire or held in a flame without exhibiting any tendency to burn; the masses may be perforated by means of a red-hot iron or with a drilling tool, and they may with perfect safety be cut into slices by means of saws revolving with great rapidity. If placed upon a fire and allowed to remain there, a feeble and transparent flame flickers over the surface of the wet gun-cotton from time to time as the exterior becomes sufficiently dry to inflame; and in this way a piece of compressed gun-cotton will burn away very gradually indeed. A pile of boxes containing in all 6 cwt. of gun-cotton, impregnated with about 20 per cent. of water, when surrounded by burning wood and shavings in a wooden building, was very gradually consumed, the gun-cotton burning as already described when the surfaces of the masses became partially dried. In two other experiments quantities of wet gun-cotton of 20 cwt. each, packed in one instance in a large, strong wooden case, and, in the other, in a number of strong packing cases, were placed in small magazines, very substantially constructed of concrete and brickwork. Large fires were kindled around the packages in each building, the doors being just left ajar. The entire contents of both buildings had burned away, without anything approaching explosive action, in less than two hours. This comparatively great safety of wet gun-cotton, coupled with the fact that its detonation in that condition may be readily accomplished through the agency of a small quantity of dry gun-cotton, which, through the medium of a fulminate fuze or detonator, is made to act as the initiative detonating agent, gives to gun-cotton important advantages over other violent explosive agents for purposes which involve the employment of more or less considerable quantities at one time, on account of the comparative safety attending its storage and the necessary manipulation of it. Moreover, it has been well established by experiments of many kinds carried out on a considerable scale, as well as by accurate scientific observations, that the detonation of wet gun-cotton is decidedly sharper or more violent than that of the dry material; a circumstance which affords an interesting illustration of the influence exerted by the physical

condition of the mass upon the facility with which detonation is transmitted from particle to particle. In the determinations made by means of the Noble chronoscope, of the velocity with which detonation is transmitted along layers or trains of gun-cotton and nitro-glycerine, the lecturer has included experiments with gun-cotton containing different proportions of water. When the material contained 15 per cent. of the liquid, some indications were obtained that the rate of transmission of detonation was a little higher than with dry gun-cotton; the difference was very decidedly in favour of wet gun-cotton, when the latter was thoroughly *saturated* with water. (With air-dry gun-cotton the mean rate of transmission ranged in several experiments between 17,000 and 18,900 feet per second; with gun-cotton containing about 30 per cent. of water, the mean rate of transmission ranged between 19,300 and 19,950 feet per second.) The air in the masses of compressed gun-cotton being replaced entirely by the comparatively incompressible body, water, the particles of explosive are in a much more favourable condition to resist displacement by the force of the detonation, and hence they are more readily susceptible of sudden chemical disintegration. Moreover, the variations in the rate of travel of detonation in dry gun-cotton, resulting from differences in the compactness or rigidity of different masses of the material, are very greatly reduced, if not entirely eliminated, by saturating the disks with water and thus equalising their power of resisting motion by a sudden blow.

Another striking illustration of the influence which the physical character of an explosive substance exercises over its susceptibility to detonation and the degree of facility with which its full explosive force is developed, is furnished by one of the most recently devised, and one of the most interesting of existing, explosive agents.

Twelve years ago, soon after the process of producing compressed and granulated gun-cotton had been elaborated by the lecturer, it occurred to him to employ these forms of gun-cotton as vehicles for the application of nitro-glycerine. A considerable proportion of the liquid was absorbed by the porous masses of gun-cotton, and a nitro-glycerine preparation analogous in character to dynamite was thus obtained. The absorbent was in this case a violently explosive body instead of an inert solid as in dynamite, but the quantity of nitro-glycerine in a given weight of the preparation (to which the name of *Glyoxilin* was given), was considerably less than in the Kieselguhr-preparation; hence the latter was nearly on a point of equality with it, in regard to power, as an explosive agent.

Nobel has observed that if, instead of making use of the most explosive form of gun-cotton, or trinitrocellulose, a lower product of nitration of cellulose (the so-called soluble or collodion gun-cotton) is added to nitro-glycerine, the liquid exerts a peculiar solvent action upon it, the fibrous material becoming gelatinised while the nitro-glycerine becomes at the same time fixed, the two substances furnishing a product having almost the characters of a compound. By macerat-

ing only from 7 to 10 per cent. of soluble gun-cotton with 90 to 93 per cent. of nitro-glycerine, the whole becomes converted into an adhesive plastic material, more gummy than gelatinous in character, from which, if it be prepared with sufficient care, no nitro-glycerine will separate even by its exposure to heat in contact with bibulous paper, or by its prolonged immersion in water, the components being not easily susceptible of separation even through the agency of a solvent of both. As the nitro-glycerine is only diluted with a small proportion of a solidfying agent which is itself an explosive (though a somewhat feeble one), this *blasting gelatine*, as Nobel has called it, is more powerful not only than dynamite but also than the mixture of a smaller quantity of nitro-glycerine with the most explosive gun-cotton, as the liquid substance is decidedly the most violent explosive of the two. Moreover, as nitro-glycerine contains a small amount of oxygen in excess of that required for the *perfect* oxidation of its carbon and hydrogen constituents, while the soluble gun-cotton is deficient in the requisite oxygen for its complete transformation into thoroughly oxidised products, the result of an incorporation of the latter in small proportion with nitro-glycerine, is the production of an explosive agent which contains the proportion of oxygen requisite for the development of the maximum of chemical energy by the complete burning of the carbon and hydrogen, and hence this blasting gelatine should, theoretically, be even slightly more powerful as an explosive agent than pure nitro-glycerine.

That such is the case has been well established by numerous experiments, but although this blasting gelatine may be detonated like dynamite by means of small quantities of confined detonating composition, when it is employed in strongly tamped blast-holes, or under conditions very favourable to the development of great initial pressure, it behaves very differently from that material, or other solid though plastic preparations of nitro-glycerine, if the attempt is made to detonate it when freely exposed to the air or only partially confined. It not only needs a much more considerable amount of strongly confined detonating composition than dynamite and similar preparations do, to bring about a detonation with it under those conditions; but when as much as 15 or 20 grains of confined fulminate are detonated in direct contact with it, although a sharp explosion occurs, little or no destructive action results, and a considerable portion of the charge operated upon is dispersed in a finely-divided condition. This dispersion appears to take place to some slight extent with dynamite also, when a small charge is detonated in open air, in consequence of its want of rigidity, though the amount of explosive which thus escapes detonation is very small as compared with the gelatine.

In comparing the effects of these nitro-glycerine preparations with each other and with compressed gun-cotton and preparations of it, by detonating equal quantities quite unconfined upon iron plates, the

results appear to establish great superiority, in point of violence of action, or destructive effect, of the more rigid explosive agents (the gun-cotton preparations). Thus, employing iron plates 1 inch thick (supported upon an anvil with a central cavity), and 4 oz. of each material unconfined, the charges being all about the same diameter, exploded by detonators of equal strength, and simply resting upon the upper surface of the plate, compressed gun-cotton produced a considerable indentation of the upper surface of the plate, and long cracks in the lower surface; a species of nitrated gun-cotton, called tonite, produced a much shallower indentation, though still a very marked one, but did not crack the lower surface. Dynamite produced only a very slight impression upon the plate, and none could be detected by the eye on the plate upon which the blasting gelatine was exploded. The difficulties, brought out by past experience, which attend the contrivance of really comparative tests of the explosive power of such substances as those under discussion, is well exemplified by the foregoing results, which were influenced to the maximum extent by the physical characters of the several substances when applied, as they were upon these iron plates, in a perfectly unconfined condition, so that the particles were free to yield to the force of the initiative detonation in proportion to their mobility. But, for this very reason, these experiments afford excellent illustration of the extent to which the development of detonation and the sharpness of its transmission through the mass is influenced not only by the inherent sensitiveness of the substance to detonation (or by its chemical instability) but also by the degree of proneness of their particles to yield mechanically to the force of a blow as applied by an initiative detonation. Thus, although in comparing two substances of similar physical characters, compressed gun-cotton and compressed nitrated gun-cotton or tonite, the superiority of the pure compound over the mixture, in point of sharpness and violence of action, is well illustrated, a comparison of the result furnished by the weakest of the four explosive agents tried, viz. tonite, with that of the substance which should be superior to all the others in explosive force (i. e. the blasting gelatine) demonstrates the important influence which the comparatively great rigidity of the mass in the one case exerts in favouring the completeness and sharpness of its detonation in open air, and the great disadvantage under which the other explosive is applied, arising out of the plastic and therefore readily yielding nature of the material. But if, by exposure to a moderate degree of cold, this plastic nitro-glycerine preparation is made to freeze (for it partakes of the property of the liquid itself of freezing at a temperature above the freezing point of water, and becomes thereby converted into at least as rigid a substance as the two descriptions of gun-cotton) its detonation upon an iron plate produces an indentation, as well as a destructive effect upon the lower surface of the plate, very decidedly greater than those furnished by the corresponding amount of pure compressed gun-cotton. Similarly, the effect produced by the detonation of dynamite upon a

plate of the kind used, is but little inferior to that of gun-cotton, and decidedly greater than that of tonite, if it is employed in the frozen condition.

A series of experiments has been made with cylinders of lead having a central perforation 1·3 inch in diameter extending to a depth of 7 inches and leaving solid metal beneath of a thickness ranging from 3·5 to 5·5 inches, according to the size of the cylinders used. These furnished results of considerable interest as illustrating the action of these several detonating agents. Charges of 1·25 oz. of each explosive substance were used throughout the experiments, and were placed at the bottoms of the holes. By the detonation of the charges the cylindrical holes in the lead were enlarged into cavities of a pear shape (and sometimes approaching the spherical form), of various diameters; in some instances the metal was besides partially torn open in a line from the bottom of the charge-hole to the circumference of the lower face of the cylinder; and in the case of some of the gun-cotton charges, the fissure in the metal in this direction was complete, the base of the block being separated from the remainder, in the form of a cone. In the first place the portions of the holes above the charges were simply left open; in the subsequent experiments they were filled up to a level with the upper surface, with dry, fine, loose sand, or with water. The dimensions of the cylinders were increased in successive experiments until, in the case of every one of the explosives used, the mass of metal was sufficiently great to resist actual fracture at the base of the cylinder. Under the conditions of these experiments, more or less considerable resistance being opposed to the mechanical dispersion of the plastic explosive substances, their detonation was greatly facilitated, though even then, the holes in the lead blocks being left open to the air, some amount of the blasting gelatine evidently escaped detonation; the widening of the upper part of the charge-hole, in experiments of this nature made with the gelatine, indicated that detonation was transmitted to small portions dispersed in the first instance and in the act of escaping from the block. In all the experiments, whether the holes were left open or filled with sand or water, the effect produced upon the base of the block by the detonation of compressed gun-cotton was considerably more violent than with the other explosive agents, indicating a sharpness of action which was only shared by the blasting gelatine when used *in a frozen state* in one of these experiments. The dimensions of the cavities produced by the gelatine were, at the largest part, considerably greater than those produced by the dynamite and nitrated gun-cotton (tonite), and slightly greater than those of the gun-cotton charges; but in the latter, the fracture of the base of the cylinder gave rise in most of the experiments to an escape of force, so that in these cases the effects of the detonation could not be well compared by measurements of the cavities. When the gelatine was converted by freezing into a rigid mass its superiority in explosive force even over compressed gun-cotton was well illustrated; the base of the lead block

was all but blown out, the cavity produced was considerably the largest, and the suddenness and violence with which motion was imparted to the water tamping caused the top of the block also to be blown off in the form of a cone. An excellent illustration was obtained, by comparing this result with those furnished by the gelatine in its normal plastic state, of the influence exercised by the physical condition of an explosive substance upon the rapidity and completeness with which detonation is transmitted through its mass.

The difficulties attending the application of blasting gelatine, in some directions in which explosive agents are applied, on account of the uncertainty attending the development of its explosive force, even with the use of a comparatively powerful detonator, unless it be very strongly confined, has led to attempts to reduce its non-sensitiveness to detonation by mixing it with materials intended to operate either by virtue of their comparatively great sensitiveness, or of their property, as solids, of reducing the very yielding character of the substance, or in both ways.

Some of these attempts have been attended with considerable success. Thus the incorporation of about 10 per cent. of the most explosive form of gun-cotton or trinitrocellulose, in a very finely divided state, with the gelatine, renders it so much more sensitive that it can be detonated with certainty in the open air by means of the strongest detonating cap now used for exploding dynamite. This effect appears to be less due to the comparative sensitiveness of gun-cotton to detonation than to the modification effected in the consistency of the material, which, though still plastic, offers decidedly greater resistance to a blow than the original gummy substance. The particles of hollow fibre of the gun-cotton appear also to have the effect of absorbing small quantities of nitro-glycerine which are less perfectly united with the soluble gun-cotton than the remainder, and which, existing as they do in somewhat variable proportions in the gelatine, have occasionally an objectionable tendency to exudation, if the incorporation of the ingredients has been less perfect than usual. The substance, when modified as described, has no longer that great adhesiveness which is exhibited by it in the original state, and which renders it less easy to manipulate.

Lastly, its explosive force appears to be in no way diminished by this modification of its composition, on the contrary, its superiority in this respect to compressed gun-cotton becomes more manifest, as demonstrated by some of the experiments with lead blocks, while its action partakes of that sharpness peculiar to the detonation of the rigid gun-cotton, as indicated by the fissure of that part of the metal situated beneath the charge. Finely divided cotton fibre has a similar effect to trinitrocellulose in modifying the physical character and increasing the sensitiveness to detonation of the blasting gelatine, but its explosive force is, of course, proportionately reduced with its dilution with an inert substance.

Nobel has made the interesting observation, that an addition to the

blasting gelatine of small proportions of certain substances rich in carbon and hydrogen, which are soluble in nitro-glycerine, such as benzol and nitro-benzol, increases to a remarkable extent the non-sensitiveness to detonation of the original material; and this observation has led to experiments being conducted by engineer officers in Austria, with a view of endeavouring to convert the blasting gelatine into a material which should compete, as regards some special advantages in point of safety, with wet gun-cotton in its application to military and naval purposes, and especially as regards non-liability to detonation or explosion by the impact of rifle bullets. If boxes containing dry compressed gun-cotton are fired into from small arms even at a short range, the gun-cotton is generally inflamed, but has never been known to explode; the sharpness of the blow essential to the latter result, which the bullet might otherwise give, being diminished by its penetration through the side of the box before reaching the explosive. It is scarcely necessary to state that wet gun-cotton, containing even as little as 15 per cent. of water, is never inflamed under these conditions. On the other hand, dynamite is invariably detonated when struck by a bullet on passing through the side of the box, and blasting gelatine, though so much less sensitive than dynamite, behaves in the same way in its ordinary as well as in the frozen condition. The Austrian experiments indicated that the gelatine when intimately mixed with only 1 per cent. of *camphor*, generally, though not invariably, escaped explosion by the impact of a bullet, but that when the proportion of camphor amounted to 4 per cent. the material was neither exploded nor inflamed, though, in the frozen state, it was still liable to occasional explosion. These results were considered indicative of a degree of safety in regard to service exigencies, approaching that of wet compressed gun-cotton. The camphoretted gelatine still labours, however, under the disadvantage of being readily inflammable, and of burning fiercely, and consequently, of giving rise, like dynamite and dry gun-cotton, to violent explosion, or detonation, if burned in considerable bulk; a result which was explained by the lecturer in his discourse delivered at the Royal Institution in 1872. Moreover, the camphoretted blasting gelatine is so difficult of detonation by the means ordinarily applied, that a large initiative charge of a very violent detonating mixture consisting of pure specially prepared trinitrocellulose and nitro-glycerine is prescribed, by the Austrian experimenters, as being indispensable to its proper detonation.

The action of camphor and of other substances rich in carbon and hydrogen in reducing greatly the sensitiveness to detonation of the preparation of soluble gun-cotton and nitro-glycerine, is not to be traced to any physical modification of that material produced by the addition of such substances, and no satisfactory theory can at present be advanced to account for it on chemical grounds.

The camphoretted gelatine, like Nobel's original gelatine itself, may be kept immersed in water for a considerable time without any

important change; the surface of the mass thus immersed becomes white and opaque, apparently in consequence of some small absorption of water, but no nitro-glycerine is separated, and on re-exposure to the air the material gradually assumes once more its original aspect. It has consequently been proposed to render the storage of blasting gelatine comparatively safe by keeping it immersed in water till required for use, but the test of time is still needed to establish the unalterableness of the material under this condition.

There can be little question that this interesting nitro-glycerine preparation, either in its most simple form, or modified in various ways, by the addition of other ingredients, promises, by virtue of its great peculiarities as a detonating agent, to present means for importantly extending the application of nitro-glycerine to industrial purposes; and it is not improbable that, through its agency, this most violent of all explosive agents at present producible upon a large scale may also come to acquire special value for important war-purposes.

It has been pointed out that the complete solidification, by freezing, of plastic preparations containing nitro-glycerine, such as dynamite and the blasting gelatine, has the effect of facilitating the transmission of detonation throughout the mass, and of thus developing or increasing the violence of their action, under certain conditions of their applications, i. e. when they are either freely exposed to air or not very closely or rigidly confined. But while, under circumstances favourable to the detonation of these substances, when in the frozen state, their full explosive force is thus much more readily applied than when they are in their normal (thawed) condition, the frozen substances are less sensitive to detonation by a blow or an initiative detonation. On the other hand, if subjected to the rapid application of great heat (as for example by the burning of portions of a mass of the explosive substance itself), a detonation is much more readily brought about with the frozen material than if it be in its normal condition. Thus a package containing 50 lb. of cartridges of plastic dynamite, when surrounded by fire, burned away without any indication of explosive action; on submitting 10 lb. of frozen dynamite to the same treatment, that quantity also burned without explosion, though at one time the combustion was so fierce as to indicate an approach to explosive action; but when the experiment was repeated on the same occasion with 15 lb. of frozen dynamite a very violent detonation took place after the material had been burning for a short time, a deep crater being produced in the ground beneath.

The following is offered as the most probable explanation of this result. When a mass of dynamite, as in these cartridges, is exposed to sufficient cold to cause the nitro-glycerine to freeze, it does not become uniformly hardened throughout, partly because of slight variations in the proportion of nitro-glycerine in different portions of the mixture composing the cartridge, and partly because unless the exposure to cold be very prolonged the external portions of the

cartridges will be frozen harder or more thoroughly than the interior. It may thus arise that portions of only partially frozen or still unfrozen dynamite may be more or less completely enclosed in hard crusts, or strong envelopes, of perfectly frozen and comparatively very cold dyamite. On exposure of such cartridges to a fierce heat very rapidly applied, as would result from the burning of a considerable quantity of the material, some portion of one or other of the cartridges would be likely to be much more readily raised to the igniting or exploding point than the remaining more perfectly frozen part in which it is partly or completely imbedded. If the ignition of this portion be brought about (which it will be with a rapidity proportionate to the intensity of heat to which the cartridge is exposed), the envelope of hard frozen dynamite by which it is still more or less completely surrounded and strongly confined, will operate like the metal envelope of a detonator, in developing the initial pressure essential for the sudden raising of the more readily inflammable portion of the dynamite to the temperature necessary for the sudden transformation of the nitro-glycerine into gas, and will thus bring about the detonation of a portion of the cartridge, which will act as the initiative detonator to the remainder of the dynamite. On igniting separately, at one of their extremities, some dynamite cartridges which had been buried in snow for a considerable period, the lecturer has observed that, as the frozen material gradually burned away, very slight but sharp explosions (like the snapping of a small percussion cap on a gun nipple) occurred from time to time, portions of the frozen dynamite being scattered with some violence. He did not succeed in obtaining actual detonation by thus burning frozen cartridges surrounded by others in a similar condition, but he has been informed by Mr. McRoberts, of the Ardeer Dynamite Works, that he has more than once detonated a small heap of hard-frozen cartridges weighing altogether one pound, by igniting one cartridge which was surrounded by the remainder. These facts appear to substantiate the correctness of the foregoing explanation. They point to the danger of assuming that, because dynamite in the frozen state is less sensitive to the effects of a blow or initiative detonation, than the thawed material, it may therefore be submitted without special care to the action of heat, for the purpose of thawing it. Instances of the detonation, with disastrous results, of even single cartridges of frozen dynamite, through the incautious application of considerable heat (as for example by placing them in an oven, or close to a fire), have been, and are still, of not unfrequent occurrence, even though Mr. Nobel has insisted upon the application of heat through the agency only of warm water, as the sole reliable method of safely thawing dynamite cartridges.

While the sensitiveness to detonation of air-dry gun-cotton remains unaffected by great reduction in temperature of the mass, and while in this respect it presents advantages over nitro-glycerine

preparations, wet gun-cotton becomes very decidedly more susceptible to detonation when frozen. Thus the detonation of gun-cotton containing an addition of from 10 to 12 per cent. of water is somewhat uncertain with the employment of 100 grains of strongly confined fulminate, and 200 grains are required for the detonation of the substance when containing 15 to 17 per cent. of water; but the latter in a frozen state can be detonated by means of 30 grains of fulminate, and 15 grains are just upon the margin of the amount requisite for detonating, with certainty, frozen gun-cotton containing 10 to 12 per cent. of water. The deadening effect of solid water upon the sensitiveness of gun-cotton to detonation is, in fact, intermediate between those of a liquid and of inert solid substances.

The effects produced and products formed by the explosion of gun-cotton in perfectly closed spaces, both in the loose, and the compressed form, and by its detonation in the dry and the wet state, have been made the subject of study by Captain Noble and Mr. Abel, the method of research pursued being the same as that followed in their published researches on fired gunpowder; results of considerable interest in regard to the heat of explosion, the pressures developed, and the products of explosion of dry and wet gun-cotton have been obtained, which are about to be communicated to the Royal Society.

It may briefly be stated that the temperature of explosion of gun-cotton is more than double that of gunpowder (being about 4400° C.); that the tension of the products of explosion, assuming the material to fill entirely the space in which it is fired, is considerably more than double that of the powder-products under the same conditions; that the products obtained by the explosion of dry gun-cotton are comparatively simple and very uniform under different conditions as regards pressure; that the products of *detonation* of *dry* gun-cotton do not differ materially from those of its explosion in a confined space, but that those furnished by the detonation of *wet* gun-cotton present some interesting points of difference. Messrs. Noble and Abel are extending their investigations to the nitro-glycerine preparations.

The great advance which has been made within the last twelve years in our knowledge of the conditions which determine the character of the metamorphosis that explosive substances undergo, and which develop or control the violence of their action, finds its parallel in the progress which has been made in the production, perfection, and application of the two most prominent of modern explosive agents, nitro-glycerine and gun-cotton. Discovered at nearly the same time, less than forty years ago, the one speedily attained great prominence, on account of the apparent ease with which it could be prepared and put to practical use; a prominence short-lived, however, because the first, and somewhat rash, attempts to utilise it preceded the acquisition of sound and sufficient knowledge of its nature and properties. Even many years afterwards, when the difficulties attending its employ-

ment appeared to have been surmounted, the confidence of its most indefatigable partisans and staunchest friends received a rude shock, from which it needed the support of much faith and some fortitude to recover.

Meanwhile, the other substance, which now shares with it the honours of important victories won over gunpowder, continued to be generally regarded as a dangerous chemical curiosity, even for some time after its present position as one of the most important industrial products and useful explosive agents was being gradually but firmly secured for it, step by step, by the talent and untiring energy of a single individual.

Almost from the day of its discovery, the fortunes of gun-cotton continued to fluctuate, and much adversity marked its career, until at last its properties became well understood, and its position as a most formidable explosive agent, applicable on a large scale, with ease, great simplicity, and with a degree of safety far greater than that as yet possessed by any other substance of this class, has now become thoroughly established. Since the lecturer last discoursed on the properties of gun-cotton, seven years ago, this material has attained a firm footing as one of the most formidable agents of defence and offence. For all military engineering operations, and for employment in submarine mines and torpedoes, compressed gun-cotton, stored and used in the wet condition, has become the accepted explosive agent in Great Britain; within the last five years upwards of 550 tons have been manufactured for this purpose, and are distributed over our chief naval stations at home and abroad. Germany some years since copied our system of manufacture and use of gun-cotton; France has provided itself with a large supply for the same purposes, and Austria, where the acquisition of bitter experience of the uncertainty of gun-cotton in the earlier stages of history, naturally gave rise to a persistent scepticism regarding its present trustworthiness, appears now also about to adopt wet gun-cotton for military and naval uses.

But while the usefulness and great value of compressed gun-cotton in these important directions has been established, its technical application has made but slow progress as compared with that of the simple nitro-glycerine preparation known as dynamite, which, in point of cost of production and convenience for general blasting purposes, can claim superiority over compressed gun-cotton. Already in 1867 a number of dynamite factories, working under Nobel's supervision, existed in different countries; in that year the total quantity manufactured amounted to 11 tons; in another year the produce had risen to 78 tons; in 1872 it had attained to 1350 tons. Two years afterwards the total production of dynamite was nearly trebled, and in 1878 it amounted to 6140 tons.

There are as many as fifteen factories in different parts of the world (including a very extensive one in Scotland) working under the supervision of Mr. Nobel, the originator of the nitro-glycerine

industry, and some six or seven other establishments exist where dynamite or preparations of very similar character are also manufactured.

How far the rate of production of dynamite will be affected by the further development of the value of Nobel's new preparation, the blasting gelatine, it is difficult to foresee, but there appears great prospect of an important future for this very peculiar and interesting detonating agent.

It is hoped that the subjects dealt with in this discourse afford interesting illustration of the intimate connection of scientific research with important practical achievements.

[F. A. A.]

Royal Institution of Great Britain.

WEEKLY EVENING MEETING,

Friday, January 31, 1879.

WILLIAM SPOTTISWOODE, Esq. M.A. D.C.L. Pres. R.S. &c. Vice-President, in the Chair.

H. HEATHCOTE STATHAM, Esq.

The Logic of Architectural Design.

ARCHITECTURE may be most comprehensively defined as "the art of building with expression;" but in order to estimate rightly the capabilities and the limits of the art as thus defined, it is necessary to bear in mind that it differs from other arts which appeal to the sense of sight, in two essential particulars. In the first place, it is an art arising out of practical requirements and governed by practical conditions. If we did not want buildings to shelter us, there would be no architecture; if we do not build them in accordance with true statical conditions, they fall down. Secondly, architecture as an art has no direct reference to nature, and does not copy natural forms: which is probably one reason why there is so much more uncertainty and divergence of opinion in regard to this art than in regard to sculpture or painting. The latter arts express themselves through forms directly imitated from nature; so that if (to put an extreme case) a painter represented a man with two heads, we need listen to no reason on the subject, we simply condemn him on our knowledge of the facts of nature. It may, however, be just as wrong for an architect to put two towers where there should be only one, or a pillar where there should be a buttress; but the right or wrong of the matter is in this case based not on reference to the concrete facts of nature in her physical aspect, but on a process of abstract reasoning which few people take the trouble to go through. The main principles which should form the basis of such reasoning may be briefly summarized in the following axioms, which embody the fundamental principles of what may be called "architectural morality."

1. Architecture, being based on practical requirements, can only be true and logical so far as it is in accordance with and expresses these.

2. The plan of the building is the basis of the whole design. A good plan is one in which the various departments are arranged and combined in such a manner as to insure the greatest convenience and the best possible effect.

3. The exterior grouping and design of a building should arise out of and indicate the interior plan and arrangement.

4. The architectural design, both internal and external, should arise out of and express the scientific construction or the "statics" of the building.

5. Ornament must be so introduced as either to emphasize the construction or to be manifestly independent thereof, and must be designed with reference to the material in which it is to be executed, and the climate under which it is to be seen.

6. No feature, not arising out of the plan or construction, can be added to the architectural design of a building, under the pretext that it is "ornamental." Such a feature is an architectural falsehood.

It would not be to the purpose, however, to endeavour here to illustrate these maxims by an attempt to evolve, in accordance with them, a perfectly logical architecture out of the depths of our inner consciousness. That is, in fact, what never has been done in the whole history of the art, as far as we know; for the very reason that all true architecture, as remarked above, arises out of practical requirements, and is a continual attempt to improve upon the method of meeting these requirements and of giving æsthetic expression to them, so that every change in the form and style of architectural design is linked with and developed from that which preceded it.

Whenever this has not been so, it is because some particular form of the architecture of the past has been seized upon for reproduction and imitation, consciously and on sentimental rather than logical grounds; a proceeding which has always resulted in producing a sense of pretence and unreality, dissatisfying in the end even to those who have initiated the movement.

We may, however, usefully study the illustration of the principles of logical design in architecture as exhibited in the two greatest and most complete of existing styles—the Greek and the Gothic.

The Greek Doric temple was structurally a very simple erection, consisting of a central cell with a kind of "verandah" round it, formed of upright pillars which carried horizontal beams, which again carried short vertical blocks supporting the cornice, which on the flanks of the building formed the termination of the slope of the roof, while at the ends there was also a raking cornice defining the angle of the roof-slope, and forming the finish of the roof in the longitudinal direction.

Now, if we denude the Doric temple of every feature which is not necessary to its stability, and reduce it to its mere constructive elements, we have such a building as is represented in the left-hand side of Fig. 1; an erection merely of square stone supports, carrying stone lintels laid from one to the other, these in turn carrying the shorter vertical blocks which support the cornice, and between which are left square openings—"metopes" (μέτωπα), intermediate spaces—which, probably, in the simpler and earlier form of structure that preceded the complete temple, were left merely as openings for light or air. If we regard such a structure as representing the

primary elements of the Doric temple, and consider what are the motives for the architectural treatment given to the various features, we can without much difficulty trace in imagination the stages of transformation through which the square column might have gone in attaining its complete form as a Doric column. It would soon appear, for example, that the square column had a heavy and ponderous effect, and that it would fulfil its practical ends just as well if the angles were cut off, so as to lighten it in appearance and render it more elegant in effect, and from this it would be an obvious step to remove the angles again, and reduce it to a figure of sixteen sides (A, B, C, Fig. 1). But it would be found that on doing this the planes of the contiguous faces of the shaft lay at too small an angle to one another for effect, that in certain lights the angle of incidence of the two planes would be nearly lost to the eye, and hence the practice of slightly hollowing each face so as to emphasize the meeting angle by a line of light and shadow. The reducing of the area of the column by thus cutting away the angles would, however, leave a less satisfactory bearing on the top of the column for the ends of the cross lintels; the column would carry their weight just as well, but it would not appear to do it so well—the lintel, for the appearance of security (which in architecture is only second in importance to actual security), would require a broader seat of a square form, and this would be supplied by the interposition of a broad stone or tile (abacus) between the lintel and the top of the column. This is the actual form which is found in what have been called the "Proto-Doric" columns at Beni-Hassan, many centuries before the Greek Doric took its complete form. The intermediate steps between the two, which doubtless once existed, are lost; we have indeed forms of early Doric at Pæstum and elsewhere in Sicily, as well as at Corinth, but these, though ruder than the Athenian Doric, have already gone through many stages of advance since the first Egyptian type. Passing over these (for we are not now dealing with architecture historically) and turning to the complete Doric, it is very significant to observe what were the additions and refinements which were arrived at in this completed form of the style. One of these is the more full and marked hollowing of the faces of the column, so as to give more decided shadow and strengthen the vertical lines. These channels or "flutes" are increased to twenty-four, and the logical suitability of this division is seen on considering the plan of the column in connection with that of the abacus (Fig. 5), where it will be observed that with twenty-four flutes the sinking of a channel is brought under the centre of the flat of the abacus (D), and the edge of a channel is brought under the angle of the abacus (E), so that a more complete relation between these two parts of the design is established. The diminution of the column upward is a very important change, and one the necessity of which, æsthetically speaking, appeals perhaps more to our instinct than our reasoning faculty. It may be reasoned, however, that greater stability, both in reality and appearance, is imparted to the column by slightly widening it at

the base; but the demand for upward diminution which the eye instinctively makes* in regarding such a feature is, perhaps, partly traceable to an unconscious generalization from the observation of the almost universal tendency to upward diminution in vertical objects in nature, in the trunks of trees and stems of plants, &c. But the line of the diminishing column makes an awkward angle with the horizontal face of the abacus, and to join this feature more harmoniously with the neck of the column, we find a rounded member introduced between them (F, Fig. 3, 4), spreading under the abacus and appearing to collect the weight of the superstructure and concentrate it on the neck, or we will rather say the wrist, of the column. This juncture of the rounded moulding (echinus) with the shaft is therefore an important point in the architectural design: it is the transference of the weight of the superstructure to its support; it is the point where, following upwards from the ground, the vertical tendency of the design ends and its horizontal tendency begins. And we find it duly marked by a series of striations (annulets) cutting across the shaft (G), emphasizing this point in the design, stopping the vertical flutings of the shaft by lines in an opposing direction (just as in a piece of music we stop the progress of the composition at its close by a repetition several times of the chord of the key), and serving to bind together and strengthen the appearance of the whole feature at this point, very much as (to compare the physical with the æsthetic) the annular ligature in the human wrist binds together the muscles of the arm. We have here, then, a feature entirely specialized to represent the capability of carrying vertical weight; a feature in which all the decorative treatment is directly designed in furtherance of that idea, and not the slightest ornament is introduced which does not assist constructive expression.

The principal lintel (architrave), which rests on the columns and carries the whole of the superstructure, is subjected to the most trying stress to which building material can be subjected, that of "cross-strain" at right angles to the direction of its bearing, a strain which acts with special disadvantage upon a granular material with little tensile strength, such as stone or marble. All its substance is therefore required for stability; nothing of it is cut away, and no decoration is introduced in a feature which is doing too much hard work to afford a suitable field for ornament.† Above the architrave we again find vertical supports in the shape of the triglyphs or three-channelled members which carry the cornice, and here again the vertical channelling of these features (H, Fig. 3) has the same function as the flutings of the column, that of assisting the expression of verticality; while the cornice, which essentially is only the overhanging of the roof

* That the eye instinctively and unconsciously demands this is obvious, from the unquestionable fact that a column or pilaster with absolutely parallel sides appears larger at the top than at the bottom.

† In accordance with the principle so admirably summarized by Mr. Ruskin in the single sentence, "Where you can rest, there decorate."

to keep the rain off the walls, is on the same principle strongly marked with horizontal lines and mouldings (for, as it has no weight to carry, we can play with it as we please), in order to emphasize horizontality, and also to form a decisive stop and finish to the vertical lines of the structure, just as the annulets of the column form a stop to the vertical lines of the fluting. The small flat blocks under the cornice (mutules) have probably a very distant reference to the wooden roof of a timber building and the ends of its rafters, almost the only point which gives any ground for the idea of the wooden origin of the style, which is in general very doubtful, or more than doubtful: the part which these features play in the complete Doric is to break the long line of the cornice and its shadow, and connect it with the repetition of parts which forms an essential element in the substructure. The slope of the pediment expresses, of course, the slope of the roof, and would have no meaning otherwise. The triangular space beneath the pediment is structurally unimportant; in a small building (or in a timber one) it might be open; in a larger one its masonry is required to carry the blocks of the cornice, but it is structurally a secondary portion, and therefore is not unfittingly made a receptacle for sculpture, which is also suitably applied in the spaces formed by the metopes; spaces which are interstructural, and might be left empty, and in which the sculpture is, in fact, executed on the face of comparatively thin slabs of stone not calculated to carry any great weight, and having only empty space behind them.*

So far we have been dealing with a method of building in which all the pressures exerted by the materials are vertical, and, as we have seen, the design precisely expresses this constructive system. Greek architecture is, constructively, the placing of a horizontal beam on vertical supports; and no construction is so simple in its problems and so enduring in its stability as this: it is however a construction wasteful in material, necessitating also the use of large blocks, and limited in the size of its openings by the incapability of the material to carry over more than a small distance without breaking even under its own weight alone. The principle of the arch, first employed on a large scale and in a systematic manner by the Romans, relieves the architect from these restrictions; it enables him to bridge over large spaces with comparatively small stones, and it employs the material in the way in which it is strongest, in a state of simple compression, and without the disadvantageous effect of cross-strain. But the pressures which an arched building exercises differ entirely from those of a lintel building. An arch is exercising upon its points of support an outward or expanding pressure, the angle of which varies with the shape of the arch and the weight and position upon it of the super-

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* In Mr. Tadema's picture, "The Love Missile," a sculptured metope slab is shown as movable on a pivot, and turned round on its centre at right angles to the wall, so as to leave the space open at either side: an arrangement which very likely was at times made in Roman imitations of Greek building.

structure: and the æsthetic expression which is true for a lintel building cannot be true for an arched building. This was exactly what the Romans, who were great engineers but bad artists, did not perceive. They could not invent an architectural expression of their own; they faced their arched constructions with a mask borrowed from the columnar architecture of the Greeks (Fig. 6), but having no reference to the real construction of the building, which in fact it contradicted: or they interposed a slice of the Greek lintel superstructure uselessly between the capital and the arch which should have sprung from it (Fig. 7); being accustomed to see a column carry an architrave and cornice, they could not conceive its existence without these members even when their use and meaning were gone. Their design was therefore æsthetically false and illogical.

Let us consider the problem of the arched building a little more in detail. Its simplest form is what is called a "barrel-vaulted" building, roofed by a continuous arch from end to end (Fig. 10). To resist the expansive thrust of this arch we must have very thick walls, so as to keep the line of pressure within the thickness of the wall, the equilibrium of which is otherwise endangered: but this again is a method very wasteful of material. If, however, we can introduce cross-arches intersecting the main arch (11), we find the result is to collect the pressure of the arches at certain equidistant points along the wall, and if we can secure these points, we can afford to build the intervening portions almost as thin as we like, or even (theoretically) to omit the wall altogether between the points of support, as it does nothing towards carrying the arches. Leaving for a moment the question of the treatment of these supporting points or piers, we have one or two more difficulties to contend with in our arched construction. A circular arch on a large scale has practically a tendency to sink at the crown, the joints on either side of the keystone becoming so nearly vertical as to allow of the stone slipping under the influence of very slight settlement in the abutments; and this danger is increased in the cross-vaulting, where the oblique arch formed by the intersecting vaulting surfaces is an ellipse, and therefore still flatter at the crown. But a new difficulty meets us again as soon we have to cross-vault a space which is not a square (12), but is larger on one side than the other. For as the two intersecting arches must rise to the same height, and as the relation of height to width is rigidly fixed in the semi-circular arch, we must either employ a segment of a circle only for the wider arch (12*a*), or else spring the smaller one from a higher point than the other (12*b*), so that the crowns of the two may coincide in height. The latter expedient (called "stilting" the arch) was the one usually adopted by the Roman and Romanesque builders; but the effect in either case is that the oblique arch formed by the intersection of these two curves of different elements gives a crippled and sinuous line very unsightly and weak in appearance.

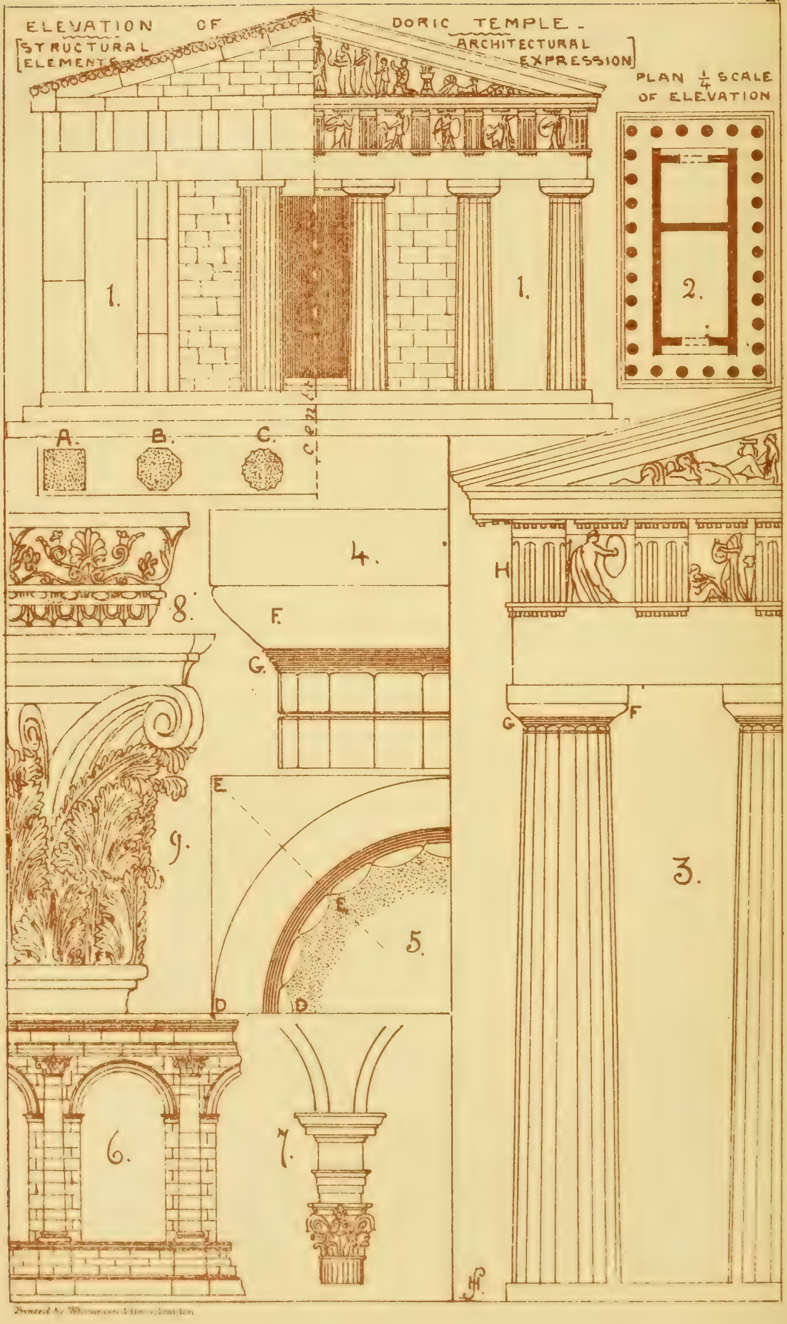
These three difficulties with the round arch are what *made* Gothic architecture, the architecture of pointed arches and buttresses, which

has been often supposed to have arisen from a mere sentimental preference for the form of the pointed arch. But architectural styles have in their growth and origin little to do with sentiment; the sentimental interest is projected back upon them from our own feelings. So completely is this idea of the sentimental origin of the pointed arch contradicted by fact, that in buildings of the Transition period, such as Kirkstall and Fountains Abbeys, we actually find the larger arches which carry the main structure pointed, and the smaller ornamental arches in higher parts of the building circular; the necessary conclusion being that the round arch was preferred and the pointed form only used in the larger arches for constructional reasons. But the main value of the pointed arch to the mediæval builders lay in the fact that by its use the difficulty of intersecting vaults of different spans was got over, practically at least if not quite theoretically. As will be seen (at 12*c*), the use of the pointed arch enables us to build arches of varying widths and of the same height, and having a curvature so nearly similar as to render it practically easy to treat them as intersecting arches without an unsightly twist of the intersecting lines; and what little difficulty there would have been in securing the neat adjustment of the two curves was obviated by the introduction of the "vaulting rib" at the angles of intersection of the curves (13), on either side of which the curves of the vaulting surfaces, divided from each other by the deep and strongly-marked mouldings of the rib, could be adjusted as far as necessary to harmonize with the curve of the rib; and this introduction of the vaulting rib, and the æsthetic prominence given to it in the design, constituted a perfectly logical treatment, for it represented the real construction of the Gothic vault, which, instead of being, like the Roman vault, an intersection of two arched surfaces, became in reality a framework of arched ribs, between which the vaulting surfaces were carried, the ribs assuming the real constructive function (Fig. 14).

Only one practical difficulty remained in designing a vault, a very slight one, but which had important results in determining the latest and in some respects most beautiful form of Gothic vaulting. This difficulty arose from the fact that the vaulting ribs near the springing, where they come down on the top of the capital, had to be run into each other ("mitred") in order to collect them on the small space of the capital from which they were to appear to spring. And as the ribs were still of varying lengths and curvatures and left the capital at different angles, their adjustment so as to break off from each other symmetrically and at the same height was a matter of some difficulty and necessitated a good deal of humouring of the vaulting lines at this point. The invention of the fan vault, the effect of which is so well known in King's College Chapel and Henry VII.'s Chapel, got over this defect, and rendered the whole perfectly logical. By intercepting all the vaulting ribs by an arc of a circle (*plan*, Fig. 15) at their highest points, instead of letting them all run up to the ridge of the roof, they were all reduced to precisely the same length, could all have the same

curvature and all leave the point of springing (the capital) with the same curve and at the same angle, and thus the design of the vault became theoretically as well as practically logical, and no adjustment or humouring was necessary to make it look exactly as it was intended to look. The fan vault thus forms a peculiarly significant illustration of the predominance of logic in architectural design, since it is perhaps of all architectural features that which to an inconsiderate observer might seem most entirely the spontaneous result of fancy and artistic taste playing easily with the materials before it, whereas it is in reality, as we see, the final solution of a struggle with a practical difficulty, carried on through many generations. What is, however, a curious fact in regard to the fan vault, is that just as the harmonious treatment of the vaulting ribs in regard to design was secured, their constructive function ceased. The fan vault was really an inverted conoid generated by the revolution of the transverse arch upon its point of springing. The vault had been gradually tending towards this form with the multiplication of the vaulting ribs, as seen in Fig. 14, representing a late form of vault previous to the fan vault; and when this latter form was actually assumed it was found that the ribs were structurally superfluous—that it was more convenient to build the whole as a solid arched conoid than to build ribs and fill in the spaces between them. Thus the ribs in the fan vault became only a "survival," and are really a feature of architectural expression, fulfilling the same kind of function as the flutes of a classic column; that is to say, emphasizing the lines of upward growth of the design, and giving variety and play of light and shade to what would otherwise have been a dead and heavy mass.

But what of the other problem, the securing of those points of the wall against which the thrust of the vaulted roof is collected? Both the Romans and the early Gothic builders felt the necessity of added strength in the wall at these points; but the Romans merely applied columns to the wall at these points (Fig. 16), and the Romanesque builders projected the wall in a flat pilaster. But the pressure of the vault upon the walls is an oblique one, tending to thrust the wall outward; it must therefore logically be met by a feature which should obviously be intended to resist oblique pressure and to express that resistance. It was reserved for the builders of the complete Gothic period, in their desire to find ample abutment for the security of their increasingly daring and adventurous vaulting, to develop the true æsthetic form for meeting this oblique thrust, the buttress (Fig. 17), with its lines sloping upward to meet the line of the vaulting-thrust, and the flying buttress, carrying this thrust of the centre vault clear over the roof of the aisle and resolving it into the opposing mass of the external buttresses which stand like so many giants round the building to hold it up. Thus the expression of the construction in the design became once more complete and logical, as it had not been since the decease of Greek architecture. And it is instructive to see how completely this necessity for buttresses to hold up the



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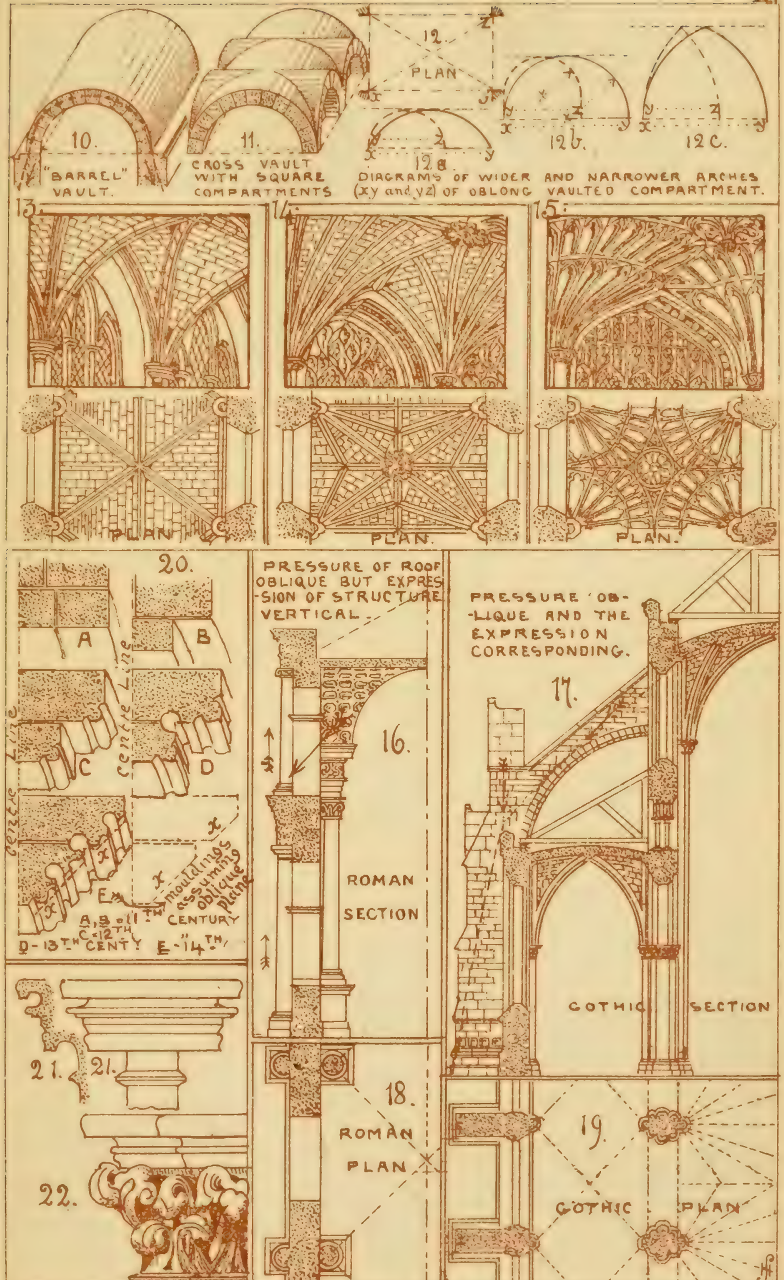

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vaulted roof has altered the whole method of building. If we compare the type of Roman plan with that of Gothic plan, we find that while in the Roman building the main line of wall is parallel with the line of the building (Fig. 18), in the Gothic structure the wall may actually be said to have been cut up into sections and placed edgeways to the building (Fig. 19)—for that is what the buttress in its full development really is.

We might trace this course of logical development through other details in Gothic architecture; we might notice for instance, how the elaborately grouped mouldings of the arches in a fourteenth century Gothic building arose simply from the convenience, when building with the small stones which the mediæval architects used, of dropping one thickness in the courses of arch-stones below the others (Fig. 20), so as to gain strength without heaviness and to avoid the bad appearance of a number of joints on the under face of the arch; how by degrees the edges of the arches thus left were more and more elaborately relieved by mouldings, and how even in the most elaborate and completely moulded arch the order and arrangement of the mouldings retains the impression of their original growth from the rectangular section, formed by the two or more orders of arch-stones one within another. But there is an even broader and more important branch of architectural logic yet to be mentioned: that which involves the relation between the plan and the design of the whole building. For architecture, as the æsthetic expression of building, cannot be rightly estimated unless it is remembered that it is based upon the plan and arrangement of the building, and must in its external features express these. As an example of correct architectural expression in this respect we may take the Houses of Parliament. In this building one great feature of the plan is the central octagon in which the main corridors of the Upper and Lower Houses meet, and which forms the rallying point of the internal traffic of the building; and the position and importance of this point in the plan are indicated externally by the central spire or lantern which rises above it. The royal entrance, again, is marked by the Victoria tower; and the clock-tower, which is a utilitarian feature with quite a different object, is designed quite differently from the Victoria tower, and in accordance with its practical object as a means of carrying a great clock high in the air; the shaft of the tower, as it may be called, being merely a support on which to carry the clock stage. If Barry had designed (as some critics have said he should have done) two similar Victoria towers symmetrically balancing each other, and made one of them the clock-tower, he would have committed an architectural falsity in designing in a similar manner two features the objects of which were entirely different. The want of logical relation between plan and design is unfortunately only too constantly illustrated by modern buildings in which the exterior is treated as a perfectly symmetrical whole, while the interior is divided into numerous small rooms for various practical purposes, in a manner of which not the least hint is conveyed in the exterior aspect

of the building, which thus becomes merely an architectural mask or screen, having no meaning whatever.

The influence of the material used and the atmosphere under which it is to be seen, will also be manifest in any architecture designed in logical accordance with practical conditions. If we compare Greek and Gothic details, such as mouldings and carved ornaments for instance, we find in the former (Figs. 8, 9) delicate contours and minute refinements of modelling, which could only be satisfactorily executed in a hard and enduring material like marble, and only satisfactorily seen under a bright sky and in a clear air: in Gothic detail, on the other hand, we find deep hollows and powerful rounded mouldings, and broad, bold, and deeply-cut foliage ornament (Figs. 21, 22), of much coarser type than the Greek, but precisely suited for execution in a comparatively coarse granulated material, to be seen under a sky frequently obscured with cloud and mist, the result of a damp atmosphere. It is in respect to these conditions that attempts to reproduce Greek architecture in this country have been and always must be failures, even apart from all considerations as to the value of a reproduced architecture. We cannot have Greek architecture here unless we can have the Greek climate and the Pentelic marble.

The almost entire deficiency of proper logical relation between practical facts and their æsthetic expression is the characteristic of modern architecture, which thus has lost its meaning as an art, and its strongest hold over our interest. The art has been reduced by the architects to a series of reproductions of the forms of past historical styles, and by the engineers (who are now doing over again exactly what the Romans in their day did) to the arbitrary application of features supposed to be architectural, in order to cover and mask construction with what they call ornament. This is carried to such an extent that often constructions are absolutely vulgarized and made absurd by this false architectural clothing, which if left in their practical simplicity would be comparatively pleasing, perhaps even positively so; for structures which, like engineering works, are designed to maintain their stability amid the action of the forces of nature, must almost necessarily, if simply treated, assume forms which are to a certain extent in harmony with nature. But the arbitrary bedizening of such structures with borrowed architectural (?) features which have no relation to their structure or purpose, necessarily produces a result which is at variance alike with nature and with art. It is only by the more rigorous exercise of thought in regard to the purpose and meaning of architecture that we can hope to extricate ourselves from these vain repetitions of features of architecture of the past into which we have drifted (and of which the last fashion, called the "Queen Anne style," is the very worst and most unmeaning that has ever been started), and to produce architecture which shall have a meaning and purpose and expression directly related to the conditions of life in this country and in the present day;

and as the demand for any particular quality in art, as well as in other manufactures, generally precedes the supply, perhaps one of the steps towards a renewed life in architecture may have been made if I have been able to persuade a non-professional but cultivated audience that it is worth their while to give a little consideration to the logic of architectural design.

[H. H. S.]

WEEKLY EVENING MEETING,

Friday, February 7, 1879.

Sir W. Frederick Pollock, Bart. M.A. Vice-President, in the Chair.

Rev. H. R. Haweis, M.A.

*Bells.**

I.

[In commenting on *the dignity of bells*, the speaker referred to the long green bell in the leaning tower of Pisa, said to date back to the thirteenth century; the great Carolus at Antwerp, which first rung in 1467, when Charles the Bold entered the city; the storm-bell in Strasburg Cathedral, which still warns the traveller of the tempest seen from afar, sweeping over the Vosges; the small bell Horrida, the tocsin of 1316, covered with mildew, which hangs high up in Notre-Dame at Antwerp, and is never rung, by reason of its age and infirmities; the gate bell in many an old fortified town, that still sounds at the shutting and opening of the city portals; the curfew, which, from time immemorial, has rung over the flats of Cambridge, and the fens of Ely; . . . . the old Tournay bells, which from their city belfry greet the silent, colossal five towers of the grandest church in Belgium, and strike the ears of the traveller as he hurries along the high road from Lille, almost before the beacon light on the summit of the belfry salutes his eyes.]

We can hardly realize what the bells were to the people in the Low Countries struggling with Spain for independence. In those old towns of Bruges, Malines, Ghent, Louvain, Antwerp, he who controlled the bells ruled the town, for he possessed the one means of summoning and directing by their call the movements of his followers —hence the jealousy of the citizens over their bells. . . . . The first thing a conqueror did was to melt down the bells, as a token that the citizens had lost the power and right of defending themselves. The cannons of the conquered, after a successful revolt, were

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* Extracts from the full discourse given from the shorthand report in 'Good Words' for April and May, 1879.

often recast for bells. And still so jealous are the Belgians of their bells, that my utmost efforts to obtain the loan of a Hemony or Van den Gheyn bell of 1650 from a disused carillon, for this lecture proved fruitless; under the best guarantees the people would not let the bells leave the country.

Bells rule and mark each impressive occasion of life. We can perhaps hardly realize the extent to which the montonous life of the old monks was bound up with the ringing of various bells. At the sound of the *signum*, or tower-bell, the whole monastery was roused from slumber at an early hour; the *squilla* summoned them to their frugal meal in the refectory; but if any of the monks were pacing the cloisters at the time and heard not the *squilla*, then the *campanella* or cloister-bell was rung. The *cymbalum* was also used in the cloister. The abbot had his *codon*, or small handbell, shaped like the orifice of a Greek trumpet; with this he summoned to his oratory or study the servile brother whose duty it was to attend to his call, whilst the *petasius*, or larger handbell, would be used occasionally to call the monks in from cultivating the fields. The *tiniolium*, or dormitory-bell, called the monks to bed. In the night-time the *noctula* or *dupla*, or clock-bell, struck to remind the brethren when they should rise and pray; whilst the dreaded *corrigiuncula*, or scourging-bell, summoned the ascetics to their flagellatory devotions. But the bell of all others which awoke, and ever awakes, in the breast of Catholics the profoundest emotion is the silver-toned *nola* or choir-bell, rung at the consecration of the elements: when that shrill and irregular ring is heard through the church the monks fall prostrate and cross themselves; the dread miracle is being at that moment consummated. . . . .

The bells say, "Le roi est mort," and they say, "Vive le roi!" they ring for the decapitation of one king, and the coronation of another; for the marriage of one royal wife, and the execution of another; they rung for the massacre of eight thousand Catholics at the Sicilian Vespers in 1282, and for the massacre of ten thousand Huguenots on St. Bartholomew's day in 1571. . . . .

II.

I pass to the *history of bells.* I have in the 'Encyclopædia Britannica,' defined a bell as "an open percussion instrument, varying in shape and material, but usually cup-like, globular, and metallic; so constructed as to yield one dominant note," a definition intended to exclude gongs, drums, cymbals, metal plates, resonant bars of metal or wood. I wish the English people to regard the bell as a percussion instrument yielding one clear unmistakable musical note when struck. There should be no doubt about the note—you should be able to hum it. This may not have been, nay, is not, always the case with bells; still that is what the bell has grown to, what it arrives at and realizes at its best.

The bell has a long past, and it will have a long future; it did not attain its present shape, or quality, or size all at once, it took thousands of years. . . . .

About A.D. 180, Lucian mentions the clepsydra, or water-bell—a bell rung periodically as the water fell from one level to another, marking the time. The Romans used bells to call to the bath, and the Christian Church adopted them about A.D. 400. France had them in 550, England in 680, and Switzerland in the tenth and eleventh centuries possessed a great many. There is St. Gall's bell, still preserved at the monastery of St. Gall, in Switzerland, and St. Patrick's bell, still to be seen in Belfast; but these are more interesting as curiosities than as bells. They are small quadrilateral handbells, made of metal plates, and can never have had a good sound. In 1400, we get bells of larger calibre: in Paris, the bell Jacqueline, and another of eleven tons, as they say, but I doubt the figures. The great bell Amboise, 1501, of Rouen, is said to have weighed about fifteen tons; but whatever the exact weight, it supplies good evidence of the comparatively heavy calibre of bells in France at that time. But with the dawn of the sixteenth century we are on the threshold of the musical age of bells, and it is a most important epoch because it marks the dawn of modern music also. The elements of music had been in the world for centuries, as you know; the Greeks, even the Jews and Egyptians, had elaborated an art of music; but modern music is an affair of the last four hundred years, and it could not exist before the discovery of the modern octave, or the uniform arrangement of tones and semitones in each key, and the "perfect cadence." This discovery is marked by the name of Monteverdi. . . . .

The rise of music was naturally marked by the rise of singing-schools and the improvement of musical instruments. For centuries the violin had been coming together—every conceivable shape, size, and quality had been tried before it began to assume, in the hands of Magini and the Amatis, something like its classic form; and for centuries bells had been vibrating through every conceivable shape and proportion before the great bellsmiths Van den Gheyn and Hemony fixed the shape, which has never since been seriously departed from with impunity, and to which we shall have to return if we want to make good bells.

It is interesting and, I think, significant to notice how the bell and the violin both settled into their true shapes about the time that the modern octave was prepared for them, and the modern musical art created, and not before. I think I may claim to have been the first to call attention to this in the pages of 'Music and Morals,' and I will once more ask you to note the dates. In 1562, Peter Van den Gheyn, the real father of the modern bell, set up his modest carillon at Louvain. In 1540, Andrew Amati, the father of the violin, set up his school at Cremona. From 1658 to 1750 we have the great bell period, perfected from Hemony to Matthias Van den

Gheyn; and from 1660 to 1730 we have the great violin period, perfected from Nicolas Amati to Stradiuarius.

Some of my friends are up in arms when I say the English bell-founders are probably indebted to the Low Countries for their successes in the art of bell-casting. I only wish they were still more indebted to them. I do not mean to say that English bell-founders have not made good bells—I never said any such thing; I said they could *not make them in tune*—that is a very different matter. You may have an excellent bell, and it may be quite out of tune with its fellows, and that is the case with most English bells. One of the Westminster Abbey bells has this inscription—

> "Thomas Lester made me
> And with the rest I will agree
> Seventeen hundred and forty-three."

But the bell's resolution, like other good resolutions, has never been fulfilled. Many fine bells there are in England, and well enough in tune for the mechanical, arithmetical, and muscular exercise called bell-ringing, but they are not fit for musical purposes. A rough octave of bells is one thing; a suite of forty, tuned accurately in semitones, is quite another. The English have never aspired to this, and they cannot do it. It has been done in the Low Countries for centuries.

I have no wish to detract from the merits of English bell-founders: the Braziers and Brends of Norwich, the Churches of Bury St. Edmunds, Myles Gray of Colchester, and later on Ruddle of Gloucester, Phelps, the Lesters, the Eayres, Mears, Warner, and Taylor of Loughborough. I rejoice to note that Mr. Raven has issued a valuable notice of the Cambridge and Suffolk bells. Mr. Lukis has dealt with Wiltshire, Mr. Tyssen with Sussex, Mr. Ellacombe with Somerset and Devon, and Mr. North with Leicester; and, doubtless, all the other counties will be in due time canvassed, and the merits of their bells done ample justice to. But still, it is odd that when there is an English bell which gives particular satisfaction, it bears a striking resemblance to the Belgian model. If you will cast your eye first upon the section of the much-praised Lavenham tenor, and then upon the section of Severin Van Aerschodt's bell (the Hemony pattern), you will see some striking resemblances between them, in the thickness of the sound-bow, the length of the side, and the width of the crown.

These features can of course only be compared by accurate measurement; but the difference between the shorter bells of Ruddall or Ruddle and the eighteenth century school, and the longer bells of Myles Gray appeals at once to the eye, and the longer bell is far nearer the Hemony model than the later Ruddle. This is certainly unfortunate for those who think that we owe nothing to the Dutch masters. But, indeed, it would have been strange had bells remained the only things unaffected by the constant intercourse between

England and the Low Countries, all through the rise and progress of the great bell-founding period. The Dutch drained our marshes, painted our best pictures—witness Van Dyke and Rubens—taught us criticism with Grotius, inspired our fashions in dress, gave us the loom, and I believe it was from the Hague that William of Orange set sail to become king of England; nor do I think it would be difficult to show that when Dutch influence was fresh, there was a remarkable *rapprochement* between the English and Belgian bell models: but when trade prejudices arose and Dutch popularity waned, the bells also deteriorated; at any rate, note the undoubted fact that the English and Belgian founders flourished side by side. Peter Van den Gheyn of Louvain, 1560, with the Braziers and Brends of Norwich; Hemony of Amsterdam, 1658, with Myles Gray, 1625–59, of Colchester. Between 1679 and 1755 flourished Richard Chandler of Buckingham, Keene of Worksop, Pleasant and Gardner of Sudbury, Ruddle of Gloucester, and Penn of Peterborough. The same period was marked across the water by the Van den Gheyns, Hemony, Dumery, Deklerk, and De Haze. But it is still more unfortunate for those who deny the influence of Dutch models in England, to find a bell of Peter Van den Gheyn hanging at this hour in St. Peter's College, Cambridge; and to note that Wagheven, a Dutchman, had a foundry as far west as Nicolaston in Glamorganshire. You may say perhaps that we taught the Belgians the art and not they us: but if you wish to learn, you go to the people who are experts, and you will find that at the time we were casting those rough bell octaves of which organists are now beginning to be a little ashamed, the Belgians were casting complicated series of thirty, forty, and even sixty bells, which hang to this day in the towers of Mechlin, Antwerp, and Louvain. Of all the Belgian masters, Hemony was the most prolific. As Bernardino Luini has flooded Lombardy with his pictures, so has Hemony flooded Holland and Belgium with his bells. We get quite tired of reading his name. He excelled in little bells, as did Peter Van den Gheyn in big ones; and Severin Van Aerschodt's small bells have all his exquisite qualities. I noted especially four in semitones that hang in the Duke of Westminster's tower at Eaton Hall, as true as any pianoforte. I tried to get over for the Royal Institution an octave of Severin Van Aerschodt's Belgian bells, to show what I mean by a bell octave in tune. I did not succeed, but Messrs. Gillett and Bland lent me four large ones fairly in tune, which served my purpose. Were the St. Paul's peal as well in tune throughout as were those four bells, I should be content. But I obtained two exquisite bells belonging to the carillon then being cast by Severin Van Aerschodt for Cattistock Rectory, Dorchester; they are in tone like a fine violoncello, and are tuned to a minor third. No pianoforte could be better in tune.

We know that the pianoforte is never accurately in tune, and in bells we must expect a lower standard, and all I seek for in any belfry is an ordinary octave good enough to satisfy an ordinary

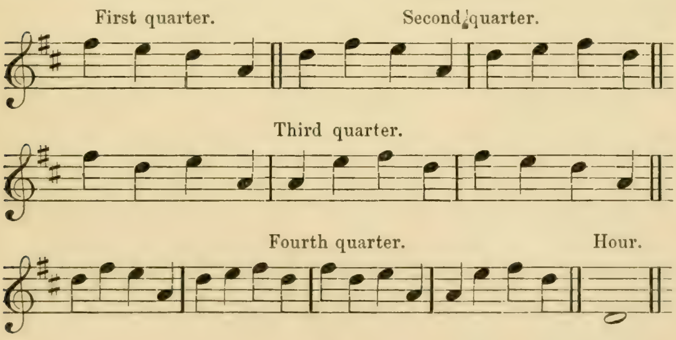
musical ear; but I do not know one single English belfry where there is even one true octave, much less one and a half. In a Belgian suite of forty bells there will probably be bells out of tune; but we pass in forty what we may fairly condemn in twelve. Now note where the difficulty seems to be. It usually begins about the seventh note, sometimes earlier. The difficulty of casting the upper notes right with the lower is considerable; and the St. Paul's peal, like most others, goes wrong at the critical point. Now what I find fault with in the St. Paul's peal is this: the first seven notes are very fairly in tune, but the eighth note is sharp for the octave. That is where they all begin to go wrong, and then commences an altogether new tonal series. That is the incurable plague from which all English bells suffer, a mixed tonal series. You get on very well at first, till you arrive suddenly at a note which is no portion of the series you began with. I have here a bell of Van Aerschodt's, and one which Mr. Lewis, the organ builder, kindly lent me; they are both good bells, but by no possibility can they ever go together, for they belong to two different tonal series; they are trying to be a third, but nothing will ever make them a third or any interval of the same octave. I also produce a specimen of an incurably bad bell, which for quite other reasons could never belong to any tonal series at all. The much-praised bells of St. Saviour's, Southwark, by Knight and Mears, begin well, with the first seven notes fairly true, but the eighth note is sharp, and after that all is wrong. Then there are the bells at Fulham, by Ruddle, 1729, which are very much admired, and they possess a very fair octave; but with the ninth note they too go wrong, and never get right again.

It is just the same with the large suite of twenty bells put up at Manchester Town Hall by Messrs. Taylor of Loughborough. From A to A you get a fair octave; from C to C the upper C is sharp, and the series never recovers itself. Messrs. Gillett and Bland, perceiving this, have wisely, in arranging the tunes, made most use of the lower and medium-sized bells, which are best in tune. Taylor succeeds best in his medium-sized bells. I remember saying to Van Aerschodt, "It is a very odd thing that the English bells all go wrong at the seventh or eighth note!" and he said, "I don't wonder at it, for that is our difficulty. We can tune the first octave, but it is the second one that is difficult, and the third is more difficult still." I said, "How long do you give to tune a bell?" He replied, "About four days to each bell, and to get a carillon right, the upper with the lower, there is no rule, no limit; that is why I cannot supply bells so quickly as my impatient customers desire. Tuning the bells takes away my sleep at night; I lie awake thinking of them; I must have them all together, must have the first octave there, when I go to the second and third octaves." That is how such perfect work as you have before you in these two bells is produced, because M. S. Van Aerschodt loses his sleep at night.

Now I should have thought it did not want a prophet to tell you

that the Westminster quarters are out of tune, but apparently it does, so I will be that prophet. It is astonishing what musical people will say when they are put to it, to what extraordinary opinions they will commit themselves about bells being in tune; and the only conclusion one can come to is, that they have never considered bells as musical notes at all, and therefore they do not expect much from them, and if they get little they are satisfied. . . . What does Mr. Turle, the organist of Westminster Abbey, say about them? No one doubts the ability of Mr. Turle. Well, he says, "I think they are pretty right!" And what does Dr. Pole say? He says he finds they "are not much amiss." And then, when you come to press Mr. Turle, he says that the fourth bell *is* flat, and when you come to press Dr. Pole, he says the first bell *is* sharp. Now what do you suppose is the musical value of four bells, the first of which is sharp and the fourth flat? Why, nothing at all.

The story of the Cambridge chimes, as given by Mr. Raven, the progression adopted at Westminster, and so popular throughout England, is interesting. In 1793 it was determined to have new chimes at St. Mary's at Cambridge. Crotch (afterwards Dr. Crotch), then the pupil of Dr. Randal, was consulted by a certain Dr. Jowett, one of the professors. Crotch was at that time a very clever choir-boy, and suggested the progressions to be chosen for the Cambridge chimes. He took a bar in Handel, which he thought would make a good chime. It is the fifth bar in the prelude to "I know that my Redeemer liveth," and out of that fifth bar came the remaining quarters, half-hour, and hour.



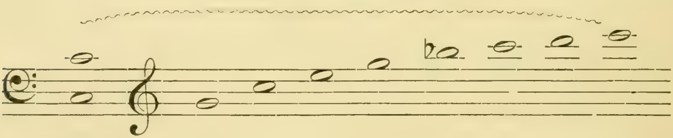
III.

Before proceeding to speak of *the making of bells*, I must offer you some rudimentary remarks on the nature of sound. What is the difference between noise and a musical sound? Roughly speaking, noise is varying and unsteady; sound is steady, constant, one note yielding equal numbers of vibrations in an equal time. Musical sound is produced by alternate air-pulses of condensation and rare-

faction. When the pulses succeed each other with a certain degree of rapidity, a musical sound is generated. The scientific instrument known as the "syren" illustrates this. A musical sound varies in three ways—in loudness, pitch, and quality. The loudness depends on the extent of the vibrations, the pitch on the rapidity of the vibrations, and the quality on the mode of vibration. The tuning-fork demonstrates to the eye the extent and rapidity of these aerial vibrations; the pianoforte and violin will enable us to analyze the quality, which depends on the mode of vibration.

It is obvious that the same note struck on violin, pianoforte, flute, &c., differs in *timbre*, or quality. What causes that difference? Helmholtz has shown us that most musical notes are not simple, but composite, sounds. He calls these clangs; they contain within them certain *buried* notes, belonging to a fixed series, and these he calls overtones; and the quality of every *clang* * depends upon the number, order, and intensity of these overtones. The presence of overtones in a clang can be easily demonstrated by striking a note on the pianoforte, just releasing the damper of its octave, and by the law of resonance the overtones, often up to the eighth, will be heard resounding sympathetically, which would not be the case were they not really *buried* in the clang of the one struck.

Overtones.



The law obeyed by the overtone series can be seen at a glance, and it is to the presence, number, order, and relative intensity of these overtones that we owe all that variety of musical timbre which makes the charm of an orchestra and furnishes with inexhaustible tonal resources the empire of musical sound.

An approximately pure tone like that of a tuning-fork, or the upper notes of a piano, is insipid and characterless. Richness and character come in with the presence of these latent overtones, the order of which the diagram will show you at a glance. All are not always present, nor are those present always in the same place of the series, nor are they always of equal intensity; therefore it is said that quality depends on the number, order, and relative intensity of the overtones present in a clang or composite note. Now as long as we have but one clang, yielding to the ear but one definite musical note, the construction of a scale on violin or piano with such clangs is simple. But when you come to bells, I believe you have to deal in

* Composite note.

one and the same bell with a number of different clangs, each with its series of overtones, the overtones of the different clangs sometimes overpowering each other, at others tones are found in the bell representing intervals less than a minor third, and producing beats, and at the same time we get certain deep hums, which I believe must be due to what in acoustics are called combination tones. Now obviously what a bell aims at musically is one fundamental note, and the problem is how to subordinate all other clangs, extinguishing those tones that are discordant, and subduing the overtones of the fundamental clang, so that none of them drown the desired note of the bell. Thus when you strike the bell on the sound-bow you get one definite note, varying in quality according to the number and proportion of the overtones, varying also according to the character and pitch of any other unextinguished clangs that may be present.

I must content myself with pointing here to the elements of the problems which can only be solved by experiment. The bell-founder has, in a word, to contend first with different clangs, secondly with loud overtones, thirdly with beats, and fourthly with combination hums; and the problem is how to obtain the presence in right proportion of those tones he requires in order to produce the timbre of his fundamental note, and how to extinguish those tones which interfere with the supremacy or quality of his fundamental note.

Now the Belgians have a summary way of settling all this. They fix the note, and have a clear perception of the quality they require, and they find that what they look for goes along with certain properties easily tested; they seek by rule of thumb for the presence of a third, fifth, and octave in each bell, and they tap the bell in certain places, as I have elsewhere described, in order to develop this third, fifth, and octave; and any ear trained in bell sounds will be able to detect the presence of at least the third or fifth, and generally the octave, in a good Belgian bell. The presence of these preponderating and selected notes is important, as constituting the bell in tune with itself, a quality of the last importance in musical suites of bells tuned for the carillon.

If, now, I am further asked upon what depends the manufacture of bells possessing these properties, I must again reply generally, in the first place, quantity and character of the metals; secondly, shape, proportion, and various thicknesses of the bell. Only two metals are now used in large bells, tin and copper. The Belgians use 23 to 30 per cent. of tin; the English lean to more tin, 25 to 31 per cent. Tin makes the bell sound bright, but it also makes the bell brittle; and the reason why the English can afford to put in more of this brittle element is because they make their bells thicker as a rule; and the reason why they are made thicker is, that instead of being merely chimed, they are swung round on a wheel, which brings the hammer with great force upon the bell. If we treated the delicate Belgian bells in this rough fashion we should probably crack them, though if it were known that they would be swung, the Belgian makers could doubtless

thicken them to order; they are not meant in Belgium to be whacked like big drums, but to be struck with hammers from *pp* to *ff*, like a pianoforte. They resonate more easily than English bells, requiring a gentler stroke to elicit their full tone. In a word, the Belgian bell is a musical note, not a gong or a drum. Then again, the thickness and general proportions of the bell are of the greatest moment. Bells vary from one-fifteenth to one-twelfth of the diameter at the thickest part of the sound-bow, and the height is commonly about twelve times the thickness. English bells are, roughly, as broad as they are long, if you measure diameter from outside rim to rim, and length from rim to top of canon. But in truth the thickness of the bell at different levels is all-important. The thickness near the top is as important as that of the sound-bow, and the diameter of the crown as critical a dimension as that of the rim. The deep, rich tone (in proportion to size) of the smaller Belgian bells is probably largely due to the wide top diameter, combined with the thinness in certain proportions of the sides half-way down. The way in which altering the thickness affects the tone, and even the pitch of a bell, is shown by the fact that a sharp bell can be flattened by shaving off the metal inside above the sound-bow; and Mr. Lewis tells me that he has destroyed beats by scooping the bell elsewhere until they disappeared at a certain point, but that on continuing to scoop they reappeared. All this shows how purely tentative and experimental is at present the art of bell-founding in England. In Belgium it is not scientific, but empirical, the accumulated experience of ages. A certain tact or rule of thumb takes the place of science; rules there must be, founded on principles, but the masters cannot explain their secrets. They produce the work of art, others are left to discover the laws they have obeyed. When we have analyzed their methods we may be able to make their bells. So thought the Germans when they measured and analyzed Raffaelle and Tintoret, and produced the correct but lifeless *banalités* of Ary Scheffer; so thought Vuillaume when he imitated the Amati fiddles even to the very worm-holes, but for all that the French fiddles are not Amatis. It may turn out that in the making of rich musical bells like those of Van Aerschodt, there is something which cannot be taught—the instinct, the incommunicable touch.

When Severin Van Aerschodt, the lineal descendant of the Van den Gheyns, the depository of the Hemony traditions, draws his bell, he will vary his model here, giving amplitude to this line and depression to that; he has no fixed or proportionately graduated scale for a suite of bells, but every bell is drawn separately; he has no fixed proportion of tin and copper, but for every four bells or so the quantity of tin is varied. I was present lately at the casting of six large bells for the Courtrai carillon, Belgium. When the glowing pool of metal boiled like a sea of dazzling jasper, and on the surface certain strange lines of sinuous motion began to curl and circle like live things born in the heat of that unearthly atmosphere, the master had a ladleful of the crystal fluid taken out and plunged

into cold water; he then broke it, and after a glance at it, took a couple more blocks of tin and threw them into the furnace. Instantly they dissolved like wax. But what effect could that have upon such a mass? It was rule of thumb; he obeyed an instinct which he could no more explain than the skilled doctor can explain why he varies slightly your prescription, or pitches upon the appropriate remedy—instinct born of accumulated experience which cannot be taught. We may sneer at all this rule of thumb, this want of science, but would it not be wiser to make as good bells before we sneer at the way in which good bells are made?

IV.

I will close with a few remarks on bell music. I shall leave to others the task of descanting on bell-ringing, which I do not call bell music, although it has the sort of musical quality possessed by scales and exercises. It is well known that our peals of bells are swung right round on wheels, and thus each time a stiff blow is delivered, and a proportionate shock imparted to the tower and bell-frame. Before Elizabeth's time only the half wheel was used, so bells could never be swung fairly up; but the art of bell-ringing made a giant stride when Fabian Stedman, about 1567, invented a system of bell notation by which changes on a few bells might be rung *ad infinitum.*

Start with three bells, 1, 2, 3, and proceed 1, 3, 2; 2, 1, 3; 2, 3, 1; 3, 1, 2; 3, 2, 1; it is much simpler than writing a tune, and has the merit of a perfectly purgatorial prolongation, so that it would take ninety-one years to ring the changes on twelve bells, at the rate of two strokes a second, and the full changes on twenty-four bells would occupy one hundred and seventeen thousand billions of years. No one can watch the skilled ringer without admiration at his litheness, readiness, and the deft, clever manipulation of that treacherous rope that has to be coquetted with and released at intervals under penalty of dragging the luckless ringer up to the roof, and there breaking his skull. No one can look at the ingenious arithmetical progressions displayed in Stedman's "Tintinnalogia" without admiration of a kind; but this hunting up and down, the dodging and snapping, the plain bob, and the extreme change, is not music, although it may be prolonged for ninety or a billion years; it is exercise, it develops muscle, quickness, and it promotes thirst. On a summer evening, some way off, it is pleasant enough, especially if heard only at intervals; but the bell-ringer's paradise is the musician's Inferno!

Nothing can justify the practice of putting a citizen of London through two hours of change ringing with twelve heavy bells by Taylor of Loughborough, and the surest way to deter the public from providing a delicious Belgian carillon of forty bells for the sister tower is to make them suppose that it will produce a sound similar to the present peal.

Bell music comes in with the bell struck by a hammer and treated as a musical note.

We hear a good deal about the clapper bringing out the full tone of the bell each time—but who wants that in music? Do you require a Sims Reeves to bawl out each note at the top of his voice, or Joachim to play fortissimo throughout? But in fact the Briton exacts the wheel of torture and the purgatorial clapper because, unlike the Belgian, he has never considered his belfry in the light of a musical instrument.

Bell music comes in with the barrel, the carillon, claveçin, or keyboard, and the suite of bells turned in semitones.

The barrel is similar to the revolving barrel of a musical box; it is fitted all over with spikes which lift tongues at whose extremity is the wire attached to the hammers up aloft, each acting on its own bell. Our clock-chimes are thus played; and in Belgium immense revolving barrels fitted with thousands of spikes liberate a little flood of music every ten minutes, and at the hour some melody with full accompaniment, as from a pianoforte, floats over city towers and ramparts—and why is not this oppressive? One bell is often too much for us, how should we endure sixty? Better far than one; it is the one or two, *ding*, *dong*, that wear out the tympanum and ruin house property. Substitute for this little flights of music, and the ear is charmed and recreated.

We have to learn the use of small bells mixed with the large We deal only with heavy peals of ten or twelve. But substitute a suite of thirty, ranging from two or ten tons to a few pounds or hundredweights, and divide the music between them, using no more of your big bells than you would of your bass notes on the piano or organ and how different the result! Again, it is noise, not music, which the Briton insists upon in his bells, and when he has got it he abuses it.

But the triumph of bell music is only reached with the application of the claveçin or keyboard. In Belgium the keyboard consists of jutting pegs, tones and semitones, ranged like white and black keys, one above the other, with a row of pedals for the feet acting on the big bells. A smart blow is needful to bring out the full tone, as the carilloneur sits stripped to his shirt, and proceeds with hands in gauntlets to manipulate his mighty scales. The English and Belgian keyboards have distinct qualities. The English machinery of Gillett and Bland substitutes for pegs, keys; and the lightness of touch rests with the player. A lady can play the heaviest suite with ease, for the instant the hammer drops it is lifted again by independent machinery, and all that the pressed key does is to let off the hammer as by a hair trigger. In the Belgian claveçin the peg has to lift with appropriate leverage as well as to liberate the hammer—hence the heaviness of the Belgian touch; but musically the Belgian claveçin, rude as it is, bears the palm, for the Belgian carilloneur can impart by his stroke the most delicate *pp* or emphatic *ff*; he can produce at will wonderful crescendos and decrescendos, while of course he who only liberates a

hammer, as in the English patent method, cannot control its stroke. The beautiful English mechanism has been applied by Messrs. Gillett and Bland to the Manchester carillon and elsewhere.

At Mechlin the barrel weighs $1\frac{1}{4}$ ton, containing 16,200 holes, and the present tunes for the hour are produced by 2900 nuts or spikes. The tunes are changed twice a year by the carilloneur, M. Denyn. M. Adolphe Denyn is acknowledged to be the first carilloneur living; he is fifty-two years old, strong, thickset, muscular; he is most genial and obliging, and a musician of the finest quality. He has been carilloneur of Mechlin for twenty-nine years, and, as I failed to hear him six years ago, I communicated with him last year in time, and was fortunate enough to be present at an exceptionally fine performance, which was most courteously protracted for an hour for my benefit. To hear M. Denyn, go to Mechlin and take your stand in the market-place at eleven o'clock on Sunday, or at eleven to twelve on Saturday morning.

It was market-day, and crowds were assembled, and stood in groups, after business, about twelve o'clock, to listen to their favourite player. I stood first at a remote corner of the market-place, and after a short running prelude from the top bells, weighing only a few pounds and hundredweights, to the bottom ones of several tons, M. Denyn broke into a brisk gallop, admirably accented, and sustained at a good tearing pace without flagging for a single bar. Such an exercise could not last long, as I quickly perceived when I ascended the belfry and watched him at work. Whilst he was playing I made my way up the winding stairs of that immense and incomparable tower, which for majesty and beauty combined has always seemed to me to stand absolutely alone. The room of the carillon keyboard is not large, but just suffices to isolate the player from the resonance of the bells above and below him. M. Denyn then played me some admirably selected flowing melodies with full legato accompaniments, in the style of "Adelaide" and "Casta Diva." Then he gave me a specimen of bravura passages, using the great nine-ton and six-ton bells for the melody with his feet, and carrying on a rattling accompaniment of demi-semiquavers on the treble bells. Next, after a rapid passage of sweeping arpeggios, he broke into a solemn and stately movement, which reminded me of Chopin's "Funeral March." This was followed by an elaborate fantasia on airs from the *Dame Blanche*—interrupted by the mechanical clock tune, "Comme on aime à vingt ans." M. Denyn waited patiently until the barrel had finished, and then plunged rapidly into an extempore continuation, which was finely joined on to the mechanical tune, and must have sounded below as if the barrel had become suddenly inspired or gone mad. He called my attention especially to the complete control he had over the *pianos* and *fortes*, now lightly touching the bells, now giving them thundering strokes. He wound up with "God save the Queen," beautifully harmonized, and I must say that I never, on piano or violin, heard more admirable phrasing and expressive rendering of

melody, whilst the vigour and sustained fire—straining every fibre and muscle until the whole man became one with the vast machinery he set in motion—reminded me of some of Rubinstein's finest efforts.

How indispensable, how historic, and how dignified are the uses of bells! And, above all, let us remember that if bells are not music, they are noise, and that noise is prejudicial to health and exhausting to the nervous system. Firstly, then, let us cast our bells in tune; secondly, let us cultivate that excellent quality which was so much admired in the Belgian bells exhibited on the table before you; thirdly, let us encourage the casting of large suites, for it is the admixture of the smaller bells which recreates the ear, throws up by contrast the grandeur of the large ones, and makes possible the performance of full pianoforte scores; and fourthly, let us cultivate the noble art of carillon claveçin playing, so that our organists shall look to the belfry for their second keyboard, and the citizens learn to assemble on Saturday or Sunday afternoon to listen to the compositions of Handel or Mendelssohn rolled forth on a prodigious scale, from the most colossal instrument it has ever entered into the heart of man to conceive or to realize. . . . .

WEEKLY EVENING MEETING,

Friday, Feb. 28, 1879.

C. William Siemens, Esq. D.C.L. F.R.S. Vice-President, in the Chair.

Sir William Thomson, LL.D. F.R.S.

The Sorting Demon of Maxwell.

[Abstract.]

The word "demon," which originally in Greek meant a supernatural being, has never been properly used to signify a real or ideal personification of malignity.

Clerk Maxwell's "demon" is a creature of imagination having certain perfectly well defined powers of action, purely mechanical in their character, invented to help us to understand the "Dissipation of Energy" in nature.

He is a being with no preternatural qualities, and differs from real living animals only in extreme smallness and agility. He can at pleasure stop, or strike, or push, or pull any single atom of matter, and so moderate its natural course of motion. Endowed ideally with arms and hands and fingers—two hands and ten fingers suffice—he can do as much for atoms as a pianoforte player can do for the keys of the piano—just a little more, he can push or pull each atom *in any direction.*

He cannot create or annul energy; but just as a living animal does, he can store up limited quantities of energy, and reproduce them at will. By operating selectively on individual atoms he can reverse the natural dissipation of energy, can cause one-half of a closed jar of air, or of a bar of iron, to become glowingly hot and the other ice cold; can direct the energy of the moving molecules of a basin of water to throw the water up to a height and leave it there proportionately cooled (1 deg. Fahrenheit for 772 ft. of ascent); can "sort" the molecules in a solution of salt or in a mixture of two gases, so as to reverse the natural process of diffusion, and produce concentration of the solution in one portion of the water, leaving pure water in the remainder of the space occupied; or, in the other case, separate the gases into different parts of the containing vessel.

"Dissipation of Energy" follows in nature from the fortuitous concourse of atoms. The lost motivity is essentially not restorable otherwise than by an agency dealing with individual atoms; and the mode of dealing with the atoms to restore motivity is essentially a process of assortment, sending this way all of one kind or class, that way all of another kind or class.

The classification, according to which the ideal demon is to sort

them, may be according to the essential character of the atom; for instance, all atoms of hydrogen to be let go to the left, or stopped from crossing to the right, across an ideal boundary; or it may be according to the velocity each atom chances to have when it approaches the boundary: if greater than a certain stated amount, it is to go to the right; if less, to the left. This latter rule of assortment, carried into execution by the demon, disequalises temperature, and undoes the natural diffusion of heat; the former undoes the natural diffusion of matter.

By a combination of the two processes, the demon can decompose water or carbonic acid, first raising a portion of the compound to dissociational temperature (that is, temperature so high that collisions shatter the compound molecules to atoms), and then sending the oxygen atoms this way, and the hydrogen or carbon atoms that way; or he may effect decomposition against chemical affinity otherwise, thus:—Let him take in a small store of energy by resisting the mutual approach of two compound molecules, letting them press as it were on his two hands, and store up energy as in a bent spring, then let him apply the two hands between the oxygen and the double hydrogen constituents of a compound molecule of vapour of water, and tear them asunder. He may repeat this process until a considerable proportion of the whole number of compound molecules in a given quantity of vapour of water, given in a fixed closed vessel, are separated into oxygen and hydrogen at the expense of energy taken from translational motions. The motivity (or energy for motive power) in the explosive mixture of oxygen and hydrogen of the one case, and the separated mutual combustibles, carbon and oxygen, of the other case, thus obtained, is a transformation of the energy found in the substance in the form of kinetic energy of the thermal motions of the compound molecules. Essentially different is the decomposition of carbonic acid and water in the natural growth of plants, the resulting motivity of which is taken from the undulations of light or radiant heat, emanating from the intensely hot matter of the sun.

The conception of the "sorting demon," is purely mechanical, and is of great value in purely physical science. It was not invented to help us to deal with questions regarding the influence of life and of mind on the motions of matter, questions essentially beyond the range of mere dynamics.

[The discourse was illustrated by a series of experiments.]

[W. T.]

WEEKLY EVENING MEETING,

Friday, March 7, 1879.

Sir W. Frederick Pollock, Bart. M.A. Vice-President, in the Chair.

Professor Huxley, LL.D. F.R.S.

*Sensation and the Unity of Structure of Sensiferous Organs.**

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We are indebted to Descartes, who happened to be a physiologist as well as a philosopher, for the first distinct enunciation of the essential elements of the true theory of sensation. In later times, it is not to the works of the philosophers, if Hartley and James Mill are excepted, but to those of the physiologists, that we must turn for an adequate account of the sensory process. Haller's luminous, though summary, account of sensation in his admirable 'Primæ Lineæ,' the first edition of which was printed in 1747, offers a striking contrast to the prolixity and confusion of thought which pervade Reid's 'Inquiry,' of seventeen years later date.† Even Sir William Hamilton, learned historian and acute critic as he was, not only failed to apprehend the philosophical bearing of long-established physiological truths; but, when he affirmed that there is no reason to deny that the mind feels at the finger points, and none to assert that the brain is the sole organ of thought, he showed that he had not apprehended the significance of the revolution commenced, two hundred years before his time, by Descartes, and effectively followed up by Haller, Hartley, and Bonnet in the middle of the last century.

In truth, the theory of sensation, except in one point, is, at the present moment, very much where Hartley, led by a hint of Sir Isaac Newton's, left it, when, a hundred and twenty years since, the 'Observations on Man: his Frame, his Duty, and his Expectations,' was

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* The whole discourse is given in the 'Nineteenth Century' for April, 1879.

† In justice to Reid, however, it should be stated that the chapters on Sensation in the 'Essays on the Intellectual Powers' (1785) exhibit a great improvement. He is, in fact, in advance of his commentator, as the note to Essay II. chap. ii. p. 248, of Hamilton's edition shows.

laid before the world. The whole matter is put in a nutshell in the following passages of this notable book:—

"External objects impressed upon the senses occasion, first, on the nerves on which they are impressed, and then on the brain, vibrations of the small and, as we may say, infinitesimal medullary particles.

"These vibrations are motions backwards and forwards of the small particles; of the same kind with the oscillations of pendulums and the tremblings of the particles of sounding bodies. They must be conceived to be exceedingly short and small, so as not to have the least efficacy to disturb or move the whole bodies of the nerves or brain.*

"The white medullary substance of the brain is also the immediate instrument by which ideas are presented to the mind; or, in other words, whatever changes are made in this substance, corresponding changes are made in our ideas; and *vice versâ*."

Hartley, like Haller, had no conception of the nature and functions of the grey matter of the brain. But, if for "white medullary substance," in the latter paragraph, we substitute "grey cellular substance," Hartley's propositions embody the most probable conclusions which are to be drawn from the latest investigations of physiologists. In order to judge how completely this is the case, it will be well to study some simple case of sensation, and, following the example of Reid and of James Mill, we may begin with the sense of smell. Suppose that I become aware of a musky scent, to which the name of "muskiness" may be given. I call this an odour, and I class it along with the feelings of light, colours, sounds, tastes, and the like, among those phenomena which are known as sensations.

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The pure sensation of muskiness is almost sure to be followed by a mental state which is not a sensation, but a belief, that there is somewhere close at hand a something on which the existence of the sensation depends. It may be a musk-deer, or a musk-rat, or a musk-plant, or a grain of dry musk, or simply a scented handkerchief; but former experience leads us to believe that the sensation is due to the presence of one or other of these objects, and that it will vanish if the object is removed. In other words, there arises a belief in an external cause of the muskiness, which, in common language, is termed an odorous body.

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It is unnecessary for the present purpose to inquire into the origin of our belief in external bodies, or into that of the notion of

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* 'Observations on Man,' vol. i. p. 11.

causation. Assuming the existence of an external world, there is no difficulty in obtaining experimental proof that, as a general rule, olfactory sensations are caused by odorous bodies; and we may pass on to the next step of the inquiry—namely, how the odorous body produces the effect attributed to it.

The first point to be noted here is another fact revealed by experience; that the appearance of the sensation is governed, not only by the presence of the odorous substance, but by the condition of a certain part of our corporeal structure, the nose. If the nostrils are closed the presence of the odorous substance does not give rise to the sensation; while, when they are open, the sensation is intensified by the approximation of the odorous substance to them, and by snuffing up the adjacent air in such a manner as to draw it into the nose. On the other hand, looking at an odorous substance, or rubbing it on the skin, or holding it to the ear, does not awaken the sensation. Thus, it can be readily established by experiment that the perviousness of the nasal passages is, in some way, essential to the sensory function; in fact, that the organ of that function is lodged somewhere in the nasal passages. And, since odorous bodies give rise to their effects at considerable distances, the suggestion is obvious that something must pass from them into the sense organ. What is this something which plays the part of an intermediary between the odorous body and the sensory organ?

The oldest speculation about the matter dates back to Democritus and the Epicurean School, and it is to be found fully stated in the fourth book of Lucretius. It comes to this: that the surfaces of bodies are constantly throwing off excessively attenuated films of their own substance; and that these films, reaching the mind, excite the appropriate sensations in it.

Aristotle did not admit the existence of any such material films, but conceived that it was the form of the substance, and not its matter, which affected sense, as a seal impresses wax, without losing anything in the process. While many, if not the majority, of the Schoolmen took up an intermediate position, and supposed that a something which was not exactly either material or immaterial, and which they called an "intentional species," effected the needful communication between the bodily cause of sensation and the mind.

But all these notions, whatever may be said for, or against, them in general, are fundamentally defective, by reason of an oversight which was inevitable, in the state of knowledge at the time in which they were promulgated. What the older philosophers did not know, and could not know, before the anatomist and physiologist had done his work, is that, between the external object and that mind in which they supposed the sensation to inhere, there lies a physical obstacle. The sense organ is not a mere passage by which the "tenuia simulacra rerum," or the "intentional species" cast off by

objects, or the "forms" of sensible things, pass straight to the mind; on the contrary, it stands as a firm and impervious barrier, through which no material particle of the world without can make its way to the world within.

Let us consider the olfactory sense organ more nearly. Each of the nostrils leads into a passage completely separated from the other by a partition, and these two passages place the nostrils in free communication with the back of the throat, so that they freely transmit the air passing to the lungs when the mouth is shut, as in ordinary breathing. The floor of each passage is flat, but its roof is a high arch, the crown of which is seated between the orbital cavities of the skull, which serve for the lodgment and protection of the eyes, and therefore lies behind the apparent limits of that feature which in ordinary language is called the nose. From the side walls of the upper and back part of these arched chambers, certain delicate plates of bone project, and these, as well as a considerable part of the partition between the two chambers, are covered by a fine, soft, moist membrane. It is to this Schneiderian, or olfactory, membrane that odorous bodies must obtain direct access if they are to give rise to their appropriate sensations; and it is upon the relatively large surface which the olfactory membrane offers that we must seek for the seat of the organ of the olfactory sense. The only essential part of that organ consists of a multitude of minute rod-like bodies, set perpendicularly to the surface of the membrane, and forming a part of the cellular coat, or epithelium, which covers the olfactory membrane, as the epidermis covers the skin. In the case of the olfactory sense, there can be no doubt that the Democritic hypothesis, at any rate for such odorous substances as musk, has a good foundation. Infinitesimal particles of musk fly off from the surface of the odorous body, and becoming diffused through the air, are carried into the nasal passages, and thence into the olfactory chambers, where they come into contact with the filamentous extremities of the delicate olfactory epithelium.

But this is not all. The "mind" is not, so to speak, upon the other side of the epithelium. On the contrary, the inner ends of the olfactory cells are connected with nerve fibres, and these nerve fibres, passing into the cavity of the skull, at length end in a part of the brain, the olfactory sensorium. It is certain that the integrity of each, and the physical inter-connection of all these three structures, the epithelium of the sensory organ, the nerve fibres and the sensorium, are essential conditions of ordinary sensation. That is to say, the air in the olfactory chambers may be charged with particles of musk; but, if either the epithelium, or the nerve fibres, or the sensorium are injured, or physically disconnected from one another, sensation will not arise. Moreover, the epithelium may be said to be receptive, the nerve fibres transmissive, and the sensorium sensifacient. For, in the act of smelling, the particles of the odorous substance produce a molecular change (which Hartley was in all proba-

bility right in terming a vibration) in the epithelium, and this change being transmitted to the nerve fibres, passes along them with a measurable velocity, and, finally reaching the sensorium, is immediately followed by the sensation.

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None the less, however, does it remain true that no similarity exists, nor indeed is conceivable, between the cause of the sensation and the sensation. Attend as closely to the sensations of muskiness or any other odour, as we will, no trace of extension, resistance, or motion is discernible in them. They have no attribute in common with those which we ascribe to matter; they are, in the strictest sense of the words, immaterial entities.

Thus, the most elementary study of sensation justifies Descartes' position, that we know more of mind than we do of body; that the immaterial world is a firmer reality than the material. For the sensation "muskiness" is known immediately. So long as it persists, it is a part of what we call our thinking selves, and its existence lies beyond the possibility of doubt. The knowledge of an objective or material cause of the sensation, on the other hand, is mediate; it is a belief as contradistinguished from an intuition, and it is a belief which, in any given instance of sensation, may, by possibility, be devoid of foundation. For odours, like other sensations, may arise from the occurrence of the appropriate molecular changes in the nerve or in the sensorium, by the operation of a cause distinct from the affection of the sense organ by an odorous body. Such "subjective" sensations are as real existences as any others and as distinctly suggest an external odorous object as their cause; but the belief thus generated is a delusion. And, if beliefs are properly termed "testimonies of consciousness," then undoubtedly the testimony of consciousness may be, and often is, untrustworthy.

Another very important consideration arises out of the facts as they are now known. That which, in the absence of a knowledge of the physiology of sensation, we call the cause of the smell, and term the odorous object, is only such, mediately, by reason of its emitting particles which give rise to a mode of motion in the sense organ. The sense organ, again, is only a mediate cause by reason of its producing a molecular change in the nerve fibre; while this last change is also only a mediate cause of sensation, depending, as it does, upon the change which it excites in the sensorium.

The sense organ, the nerve, and the sensorium, taken together, constitute the sensiferous apparatus. They make up the thickness of the wall between the mind, as represented by the sensation "muskiness," and the object, as represented by the particle of musk in contact with the olfactory epithelium.

It will be observed that the sensiferous wall and the external world are of the same nature; whatever it is that constitutes them both is expressible in terms of matter and motion. Whatever changes

take place in the sensiferous apparatus are continuous with, and similar to, those which take place in the external world.* But with the sensorium, matter and motion come to an end; while phenomena of another order, or immaterial states of consciousness, make their appearance. How is the relation between the material and the immaterial phenomena to be conceived? This is the metaphysical problem of problems, and the solutions which have been suggested have been made the corner-stones of systems of philosophy.

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Sensations of taste, however, are generated in almost as simple a fashion as those of smell. In this case, the sense organ is the epithelium which covers the tongue and the palate; and which sometimes, becoming modified, gives rise to peculiar organs termed "gustatory bulbs," in which the epithelial cells elongate and assume a somewhat rod-like form. Nerve fibres connect the sensory organ with the sensorium, and tastes or flavours are states of consciousness caused by the change of molecular state of the latter. In the case of the sense of touch there is often no sense organ distinct from the general epidermis. But many fishes and amphibia exhibit local modifications of the epidermic cells which are sometimes extraordinarily like the gustatory bulbs; more commonly, both in lower and higher animals, the effect of the contact of external bodies is intensified by the development of hair-like filaments, or of true hairs, the bases of which are in immediate relation with the ends of the sensory nerves. Everyone must

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* The following diagrammatic scheme may help to elucidate the theory of sensation:—

Mediate Knowledge				Immediate Knowledge
	Sensiferous Apparatus			
Objects of Sense	Receptive (Sense Organ)	Transmissive (Nerve)	Sensificatory (Sensorium)	Sensations and other States of Consciousness
Hypothetical Substance of Matter				Hypothetical Substance of Mind
Physical World				Mental World
Not Self	Self			
Non-Ego or Object				Ego or Subject

Immediate knowledge is confined to states of consciousness, or, in other words, to the phenomena of mind. Knowledge of the physical world, or of one's own body and of objects external to it, is a system of beliefs or judgments based on the sensations. The term "self" is applied not only to the series of mental phenomena which constitute the ego, but to the fragment of the physical world which is their constant concomitant. The corporeal self, therefore, is part of the non-ego; and is objective in relation to the ego as subject.

have noticed the extreme delicacy of the sensations produced by the contact of bodies with the ends of the hairs of the head; and the "whiskers" of cats owe their functional importance to the abundant supply of nerves to the follicles in which their bases are lodged. What part, if any, the so-called "tactile corpuscles," "end bulbs," and "Pacinian bodies" play in the mechanism of touch is unknown. If they are sense organs, they are exceptional in character, in so far as they do not appear to be modifications of the epidermis. Nothing is known respecting the sense organs of those sensations of resistance which are grouped under the head of the muscular sense; nor of the sensations of warmth and cold; nor of that very singular sensation which we call tickling.

In the case of heat and cold, the organism not only becomes affected by external bodies, far more remote than those which affect the sense of smell; but the Democritic hypothesis is obviously no longer permissible. When the direct rays of the sun fall upon the skin, the sensation of heat is certainly not caused by "attenuated films" thrown off from that luminary, but to a mode of motion which is transmitted to us. In Aristotelian phrase, it is the form without the matter of the sun which stamps the sense organ; and this, translated into modern language, means nearly the same thing as Hartley's vibrations. Thus we are prepared for what happens in the case of the auditory and the visual senses. For neither the ear nor the eye receives anything but the impulses or vibrations originated by sonorous or luminous bodies. Nevertheless, the receptive apparatus still consists of nothing but specially modified epithelial cells. In the labyrinth of the ear of the higher animals the free ends of these cells terminate in excessively delicate hair-like filaments; while, in the lower forms of auditory organ, its free surface is beset with delicate hairs like those of the surface of the body, and the transmissive nerves are connected with the bases of these hairs. Thus there is an insensible gradation in the forms of the receptive apparatus, from the organ of touch, on the one hand, to those of taste and smell; and, on the other hand, to that of hearing. Even in the case of the most refined of all the sense organs, that of vision, the receptive apparatus departs but little from the general type. The only essential constituent of the visual sense organ is the retina, which forms so small a part of the eyes of the higher animals; and the simplest eyes are nothing but portions of the integument, in which the cells of the epidermis have become converted into glassy rod-like retinal corpuscles. The outer ends of these are turned towards the light; their sides are more or less extensively coated with a dark pigment, and their inner ends are connected with the transmissive nerve fibres. The light impinging on these visual rods produces a change in them which is communicated to the nerve fibres, and, being transmitted to the sensorium, gives rise to the sensation—if indeed all animals which possess eyes are endowed with what we understand as sensation.

In the higher animals, a complicated apparatus of lenses arranged on the principle of a camera obscura, serves at once to concentrate and to individualize the pencils of light proceeding from external bodies. But the essential part of the organ of vision is still a layer of cells which have the form of rods with truncated or conical ends. By what seems a strange anomaly, however, the glassy ends of these are turned, not towards, but away from, the light; and the latter has to traverse the layer of nervous tissues with which their outer ends are connected, before it can affect them. Moreover, the rods and cones of the vertebrate retina are so deeply seated, and in many respects so peculiar in character, that it appears impossible, at first sight, that they can have anything to do with that epidermis of which gustatory and tactile, and at any rate the lower forms of auditory and visual, organs are obvious modifications.

Whatever be the apparent diversities among the sensiferous apparatuses, however, they share certain common characters. Each consists of a receptive, a transmissive, and a sensificatory portion. The essential part of the first is an epithelium, of the second, nerve fibres, of the third, a part of the brain; the sensation is always the consequence of the mode of motion excited in the receptive, and sent along the transmissive, to the sensorial part of the sensiferous apparatus. And, in all the senses, there is no likeness whatever between the object of sense, which is matter in motion, and the sensation, which is an immaterial phenomenon.

On the hypothesis which appears to me to be the most convenient, sensation is a product of the sensiferous apparatus caused by certain modes of motion which are set up in it by impulses from without. The sensiferous apparatuses are, as it were, factories, all of which at the one end receive raw materials of a similar kind—namely, modes of motion—while, at the other, each turns out a special product, the feeling which constitutes the kind of sensation characteristic of it.

Or, to make use of a closer comparison, each sensiferous apparatus is comparable to a musical-box wound up; with as many tunes as there are separate sensations. The object of a simple sensation is the agent which presses down the stop of one of these tunes, and the more feeble the agent, the more delicate must be the mobility of the stop.

But, if this be the case, if the recipient part of the sensiferous apparatus is, in all cases, merely a mechanism affected by coarser or finer kinds of material motion, we might expect to find that all sense organs are fundamentally alike, and result from the modification of the same morphological elements. And this is exactly what does result from all recent histological and embryological investigations.

It has been seen that the receptive part of the olfactory apparatus is a slightly modified epithelium, which lines an olfactory chamber deeply seated between the orbits in adult human beings. But, if we trace back the nasal chambers to their origin in the embryo, we find that, to begin with, they are mere depressions of the skin of the fore

part of the head, lined by a continuation of the general epidermis. These depressions become pits, and the pits, by the growth of the adjacent parts, gradually acquire the position which they finally occupy. The olfactory organ, therefore, is a specially modified part of the general integument.

The human ear would seem to present greater difficulties. For the essential part of the sense organ, in this case, is the membranous labyrinth, a bag of complicated form, which lies buried in the depths of the floor of the skull, and is surrounded by dense and solid bone. Here, however, recourse to the study of development readily unravels the mystery. Shortly after the time when the olfactory organ appears as a depression of the skin on the side of the fore part of the head, the auditory organ appears as a similar depression on the side of its back part. The depression, rapidly deepening, becomes a small pouch, and then, the communication with the exterior becoming shut off, the pouch is converted into a closed bag, the epithelial lining of which is a part of the general epidermis segregated from the rest. The adjacent tissues, changing first into cartilage and then into bone, enclose the auditory sac in a strong case, in which it undergoes its further metamorphoses; while the drum, the ear bones, and the external ear are superadded by no less extraordinary modifications of the adjacent parts. Still more marvellous is the history of the development of the organ of vision. In the place of the eye, as in that of the nose and that of the ear, the young embryo presents a depression of the general integument; but, in man and the higher animals, this does not give rise to the proper sensory organ, but only to part of the accessory structures concerned in vision. In fact, this depression, deepening and becoming converted into a shut sac, produces only the cornea, the aqueous humour, and the crystalline lens of the perfect eye.

The retina is added to this by the outgrowth of the wall of a portion of the brain into a sort of bag or sac with a narrow neck, the convex bottom of which is turned outwards or towards the crystalline lens. As the development of the eye proceeds, the convex bottom of the bag becomes pushed in, so that it gradually obliterates the cavity of the sac, the previously convex wall of which becomes deeply concave. The sac of the brain is now like a double nightcap ready for the head, but the place which the head would occupy is taken by the vitreous humour, while the layer of nightcap next it becomes the retina. The cells of this layer which lie furthest from the vitreous humour, or, in other words, bound the original cavity of the sac, are metamorphosed into the rods and cones. Suppose now that the sac of the brain could be brought back to its original form; then the rods and cones would form part of the lining of a side pouch of the brain. But one of the most wonderful revelations of embryology is the proof of the fact that the brain itself is, at its first beginning, merely an infolding of the epidermic layer of the general integument. Hence it follows that the rods and cones of the vertebrate eye are modified

epidermic cells, as much as the crystalline cones of the insect or crustacean eye are; and that the inversion of the position of the former in relation to light arises simply from the roundabout way in which the vertebrate retina is developed.

Thus all the higher sense organs start from one foundation, and the receptive epithelium of the eye, or of the ear, is as much modified epidermis as is that of the nose. The structural unity of the sense organs is the morphological parallel to their identity of physiological function, which, as we have seen, is to be impressed by certain modes of motion; and they are fine or coarse in proportion to the delicacy or the strength of the impulses by which they are to be affected.

In ultimate analysis, then, it appears that a sensation is the equivalent in terms of consciousness for a mode of motion of the matter of the sensorium. But, if inquiry is pushed a stage further, and the question is asked, What then do we know about matter and motion? there is but one reply possible. All that we know about motion is that it is a name for certain changes in the relations of our visual, tactile, and muscular sensations; and all that we know about matter is that it is the hypothetical substance of physical phenomena—the assumption of the existence of which is as pure a piece of metaphysical speculation as that of the substance of mind.

Our sensations, our pleasures, our pains, and the relations of these make up the sum total of the elements of positive, unquestionable knowledge. We call a large section of these sensations and their relations matter and motion; the rest we term mind and thinking; and experience shows that there is a certain constant order of succession between some of the former and some of the latter.

WEEKLY EVENING MEETING,

Friday, March 14, 1879.

William Spottiswoode, Esq. M.A. D.C.L. Pres.R.S. Vice-President, in the Chair.

Edward Burnett Tylor, Esq. F.R.S.

*The History of Games.**

Before examining some groups of the higher orders of games, with the view of tracing their course in the world, it will be well to test by a few examples the principles on which we may reason as to their origin and migrations. An intelligent traveller among the Kalmuks, noticing that they play a kind of chess resembling ours, would not for a moment entertain the idea of such an invention having been made more than once, but would feel satisfied that we and they and all chess-players must have had the game from one original source. In this example lies the gist of the ethnological argument from artificial games, that when any such appears in two districts it must have travelled from one to the other, or to both from a common centre. Of course this argument does not apply to all games. Some are so simple and natural that, for all we can tell, they may often have sprung up of themselves, such as tossing a ball or wrestling; while children everywhere imitate in play the serious work of grown-up life, from spearing an enemy down to moulding an earthen pot. The distinctly artificial sports we are concerned with here are marked by some peculiar trick or combination not so likely to have been hit upon twice. Not only complex games like chess and tennis, but even many childish sports, seem well-defined formations, of which the spread may be traced on the map much as the botanist traces his plants from their geographical centres. It may give us confidence in this way of looking at the subject if we put the opposite view to the test of history and geography to see where it fails. Travellers, observing the likeness of children's games in Europe and Asia, have sometimes explained it on this wise: that the human mind being alike everywhere, the same games are naturally found in different lands, children taking to hockey, tops, stilts, kites, and so on, each at its proper season. But if so, why is it that in outlying barbarous countries one hardly finds a game without finding also that there is

* Extracted from the 'Fortnightly Review' for May, 1879.

a civilized nation within reach from whom it may have been learnt? And what is more, how is it that European children knew nothing till a few centuries ago of some of their now most popular sports? For instance, they had no battledore-and-shuttlecock and never flew kites till these games came across from Asia, when they took root at once and became naturalized over Europe. The origin of kite-flying seems to lie somewhere in South-east Asia, where it is a sport even of grown-up men, who fight their kites by making them cut one another's strings, and fly birds and monsters of the most fantastic shapes and colours, especially in China, where old gentlemen may be seen taking their evening stroll, kite-string in hand, as though they were leading pet dogs. The English boy's kite appears thus an instance, not of spontaneous play-instinct, but of the migration of an artificial game from a distant centre. Nor is this all it proves in the history of civilization. Within a century, Europeans becoming acquainted with the South Sea Islanders found them down to New Zealand adepts at flying kites, which they made of leaves or bark cloth, and called *mánu*, or "bird," flying them in solemn form with accompaniment of traditional chants. It looks as though the toy reached Polynesia through the Malay region, thus belonging to that drift of Asiatic culture which is evident in many other points of South Sea Island life. The geography of another of our childish diversions may be noticed as matching with this. Mr. Wallace relates that being one wet day in a Dayak house in Borneo, he thought to amuse the lads by taking a piece of string to show them *cat's-cradle*, but to his surprise he found that they knew more about it than he did, going off into figures that quite puzzled him. Other Polynesians are skilled in this nursery art, especially the Maoris of New Zealand, who call it *maui* from the name of their national hero, by whom, according to their tradition, it was invented; its various patterns represent canoes, houses, people, and even episodes in Maui's life, such as his fishing up New Zealand from the bottom of the sea. In fact, they have their pictorial history in cat's-cradle, and whatever their traditions may be worth, they stand good to show that the game was of the time of their forefathers, not lately picked up from the Europeans. In the Sandwich Islands and New Zealand it is on record that the natives were found playing a kind of draughts which was not the European game, and which can hardly be accounted for but as another result of the drift of Asiatic civilization down into the Pacific.

Once started, a game may last on almost indefinitely. Among the children's sports of the present day are some which may be traced back toward the limits of historical antiquity, and, for all we know, may have been old then. Among the pictures of ancient Egyptian games in the tombs of Beni Hassan, one shows a player with his head down so that he cannot see what the others are doing with their clenched fists above his back. Here is obviously the game called in English *hot-cockles*, in French *main-chaude*, and better described by

its mediæval name of *qui fery?* or "who struck?"—the blindman having to guess by whom he was hit, or with which hand. It was the Greek *kollabismos*, or buffet-game, and carries with it a tragical association in those passages in the Gospels which show it turned to mockery by the Roman soldiers: "And when they had blindfolded him . . . they buffeted him . . . saying, Prophesy unto us, thou Christ, who is he that smote thee?" (Luke xxii. 64; Matt. xxvi. 67; Mark xiv. 65.)

Another of the Egyptian pictures plainly represents the game we know by its Italian name of *morra*, the Latin *micatio*, or flashing of the fingers, which has thus lasted on in the Mediterranean districts over three thousand years, handed down through a hundred successive generations who did not improve it, for from the first it was perfect in its fitting into one little niche in human nature. It is the game of guessing addition, the players both at once throwing out fingers and in the same moment shouting their guesses at the total. Morra is the pastime of the drinking-shop in China as in Italy, and may, perhaps, be reckoned among the items of culture which the Chinese have borrowed from the Western barbarians. Though so ancient, morra has in it no touch of prehistoric rudeness, but must owe its origin to a period when arithmetic had risen quite above the savage level. The same is true of the other old arithmetical game, *odd-and-even*, which the poet couples with riding on a stick as the most childish of diversions, "Ludere *par impar*, equitare in arundine longâ." But the child playing it must be of a civilized nation, not of a low barbaric tribe, where no one would think of classing numbers into the odd-and-even series, so that Europeans have even had to furnish their languages with words for these ideas. I asked myself the question whether the ancient Aryans distinguished odd from even, and curiously enough found that an answer had been preserved by the unbroken tradition not of Greek arithmeticians, but of boys at play. A scholiast on the Ploutos of Aristophanes, where the game is mentioned, happens to remark that it was also known as *ζυγὰ ἢ ἄζυγα*, "yokes or not-yokes." Now this matches so closely in form and sense with the Sanskrit terms for even and odd numbers, *yuj* and *ayuj*, as to be fair evidence that both Hindus and Greeks inherited arithmetical ideas and words familiar to their Aryan ancestors.

Following up the clues that join the play-life of the ancient and modern worlds, let us now look at the ball-play, which has always held its place among sports. Beyond mere tossing and catching, the simplest kind of ball-play is where a ring of players send the ball from hand to hand. This gentle pastime has its well-marked place in history. Thus the ancient Greeks, whose secret of life was to do even trivial things with artistic perfection, delighted in the game of Nausikaa, and on their vases is painted many a scene where ball-play, dance, and song unite in one graceful sport. The ball-dance is now scarcely to be found but as an out-of-the-way relic of old custom; yet it has left curious traces in European languages, where

the *ball* (Low Latin *balla*) has given its name to the dance it went with (Italian *ballare*, *ballo*, French *bal*, English *ball*), and even to the song that accompanied the dance (Italian *ballata*, French *ballade*, English *ballad*). The passion of ball-play begins not with this friendly graceful delivery of the ball into the next hand, but when two hostile players or parties are striving each to take or send it away from the other. Thus, on the one hand, there comes into existence the group of games represented by the Greek *harpaston*, or seizing-game, where the two sides struggled to carry off the ball. In Brittany this has been played till modern times with the hay-stuffed *soule* or *sun*-ball, as big as a football, fought for by two communes, each striving to carry it home over their own border. Émile Souvestre, in his 'Derniers Bretons,' has told the last story of this fierce game in the Ponthivy district—how the man who had had his father killed and his own eye knocked out by François, surnamed le Souleur, lay in wait for that redoubted champion, and got him down, soule and all, half-way across the boundary stream. The murderous soule-play had to be put down by authority, as it had been years before in Scotland, where it had given rise to the suggestive proverb, "All is fair at the ball of Scone." The other class of hostile ball-games differs from this in the ball having not to be brought to one's own home, but sent to the goal of the other side. In the Greek *epikoinos*, or common-ball, the ball was put on the middle line, and each party tried to seize it and throw it over the adversary's goal-line. This game also lasted on into modern Europe, and our proper English name for it is *hurling*, while *football* also is a variety of it, the great Roman blown leather ball (*follis*) being used instead of the small hand-ball, and kicked instead of thrown. Now as hurling was an ordinary classical game, the ancients need only have taken a stick to drive the ball instead of using hands or feet, and would thus have arrived at *hockey*. But Corydon never seems to have thought of borrowing Phillis's crook for the purpose it would have so exactly suited. No mention of games like hockey appears in the ancient world, and the course of invention which brought them into the modern world is at once unexpected and instructive.

The game known to us as *polo* has been traced by Sir W. Ouseley, in Persia, far back in the Sassanian dynasty, and was at any rate in vogue there before the eighth century. It was played with the long-handled mallet called *chugán*, which Persian word came to signify also the game played with it. This is the instrument referred to in the 'Thousand and One Nights,' and among various earlier passages where it occurs is the legend told by the Persian historian of Darius insulting Alexander by sending him a ball and mallet (*guï ve chugán*) as a hint that he was a boy more fit to play polo than to go to war. When this tale finds its way to Scotland, in the romance of King Alisaunde, these unknown instruments are replaced by a whipping-top, and Shakspere has the story in the English guise of a newer period in the scene in Henry V.: "What treasure,

uncle?"—"Tennis-balls, my liege." By the ninth century the game of *chugán* had established itself in the Eastern Empire, where its name appears in the barbarous Greek form *τζυκανίζειν*. In the Byzantine descriptions, however, we find not the original mallet, but a long staff ending in a broad bend, filled in with a network of gut-strings. Thus there appear in the East, as belonging to the great sport of ball-play on horseback, the first shapes of two implements which remodelled the whole play-life of mediæval and modern Europe, the chugán being the ancestor of the mallets used in pall-mall and croquet, and of an endless variety of other playing clubs and bats, while the bent staff with its network was the primitive racket. The fine old Persian drawing of a match at chugán, which is copied by Ouseley in his 'Travels in the East,' justifies his opinion that the horseback game is the original. We should not talk of polo as being "hockey on horseback," but rather regard hockey as dismounted polo, and class with it pall-mall, golf, and many another bat-and-ball games. Indeed, when one comes to think of it, one sees that no stick being necessary for the old foot game of hurling, none was used, but as soon as the Persian horsemen wanted to play ball on horseback, a proper instrument had to be invented. This came to be used in the foot game also, so that the Orientals are familiar both with the mounted and dismounted kinds. The horseback game seems hardly to have taken hold in Europe till our own day, when the English brought it down from Munniepoor, and it has now under the name of *polo* become a world-wide sport again. But the foot-game made its way early into Europe, as appears from a curious passage in Joinville's 'Life of St. Louis,' written at the end of the thirteenth century. Having seen the game on his crusade, and read about it in the Byzantine historians, he argues that the Greeks must have borrowed their *tzycanisterium* from the French, for it is, he says, a game played in Languedoc by driving a boxwood ball with a long mallet, and called there *chicane*. The modern reader has to turn this neat and patriotic argument upside down, the French *chicane* being only a corruption of the Persian *chugán;* so that what Joinville actually proves is, that before his time the Eastern game had travelled into France, bringing with it its Eastern name. Already, in his day, from the ball-game with its shifts and dodges, the term *chicane* had come to be applied by metaphor to the shuffles of lawyers to embarrass the other side, and thence to intrigue and trickery in general. English has borrowed *chicane* in the sense of trickery, without knowing it as the name of a game. Metaphors taken from sports may thus outlast their first sense, as when again people say, "Don't *bandy* words with me," without an idea that they are using another metaphor taken from the game of hockey, which was called *bandy* from the curved stick or club it was played with.

In France, the name of *crosse*, meaning a crutch, or bishop's crosier, was used for the mallet, and thence the game of hockey has its ordinary French name, *jeu de la crosse*. In Spanish the game has

long been known as *chueca*. The Spaniards taught it to the natives of South America, who took kindly to it, not as mere boys' play, but as a manly sport. It is curious to read accounts by modern European travellers, who seem not to recognize their own playground game when transplanted among the Araucanians of Chili, even though it shows its Spanish origin by the name of *chueca*. Seeing this, one asks whence did the North American Indians get their famous ball-play, known from California right across the Indian country? It is to all intents the European *chueca*, *crosse*, or *hockey*, the deerskin ball being thrown up in the middle, each of the two contending parties striving to throw or drive it through the adversaries' goal. The Iroquois say that in old times their forefathers played with curved clubs and a wooden ball, before the racket was introduced, with which to strike, carry, or throw the leather ball. Of all the describers of this fine game, Catlin has best depicted its scenes with pen and pencil, from its beginning with the night ball-play dance, where the players crowded round their goals, held up and clashed their rackets, and the women danced in lines between, and the old men smoked to the Great Spirit, and led the chant for his favour in the contest. The painter would never miss a ball-play, but sit from morning till sundown on his pony studying the forms of the young athletes in their "almost superhuman" struggles for the ball, till at last one side made the agreed number of goals, and divided with yells of triumph the fur robes and tin-kettles and miscellaneous property staked on the match. Now, as to the introduction of the game into North America, the Jesuit missionaries in New France as early as 1636 mention it by their own French name of *jeu de crosse*, at which Indian villages contended "à qui crossera le mieux." The Spaniards, however, had been above a century in America, and might have brought it in, which is a readier explanation than the other possible alternative that it made its way across from South-east Asia.

When the Middle Ages set in, the European mind at last became awake to the varied pleasure to be got out of hitting a ball with a bat. The games now developed need not be here spoken of at length proportioned to their great place in modern life, as the changes which gave rise to them are so comparatively modern and well known. The Persian apparatus kept close to its original form in the game of *pall-mall*, that is, "ball-mallet," into which game was introduced the arch or ring to drive the ball through, whereby enough incident was given to knocking it about to make the sport fit for a few players, or even a single pair. An account of pall-mall and its modern revival in *croquet* will be found in Dr. Prior's little book. Playing the ball into holes serves much the same purpose as sending it through rings, and thus came in the particular kind of bandy called *golf*, from the clubs used to drive the ball. The *stool-ball*, so popular in mediæval merry-makings, was played with a stool, which one protected by striking away with his hands the ball which another bowled at it; the in-player was out if the stool was hit, or he might be caught out, so that here is

evidently part of the origin of cricket, in which the present stumps seem to represent the stool. In *club-ball* a ball was bowled and hit with a club; and a game called *cat-and-dog* was played in Scotland two centuries ago, where players protected not wickets but holes from the wooden cat pitched at them, getting runs when they hit it. We have here the simple elements from which the complex modern cricket was developed. Lastly, among the obscure accounts of ancient ball-play, it is not easy to make out that the ball was ever sent against an opposite wall for the other players to take it at the bound and return it. Such a game, particularly suited to soldiers shut up in castle-yards, became popular about the fourteenth century, under the name of *pila palmaria*, or *jeu de paulme*, which name indicates its original mode of striking with the palm of the hand, as in *fives*. It was an improvement to protect the hand with a glove, as such may still be seen in the ball-play of Basque cities, as at Bayonne. Sometimes a battledore faced with parchment was used, as witness the story of the man who declared he had played with a battledore that had on it fragments of the lost decades of Livy. But it was the racket that made possible the "cutting" and "boasting" of the mediæval tennis-court, with its elaborate scoring by "chases." No doubt it was the real courtyard of the château, with its penthouses, galleries, and grated windows, that furnished the tennis-court with the models for its quaintly artificial grilles and lunes so eruditely discussed in Mr. Julian Marshall's 'Annals of Tennis.' A few enthusiastic amateurs still delight in the noble and costly game, but the many have reason to be grateful for lawn-tennis out of doors, though it be but a mild version of the great game, to which it stands as hockey to polo, or as draughts to chess.

Turning now to the principal groups of sedentary games, I may refer to the evidence I have brought forward elsewhere,* that the use of lots or dice for gambling arose out of an earlier serious use of such instruments for magical divination. The two conceptions, indeed, pass into one another. The magician draws lots to learn the future and the gambler to decide the future, so that the difference between them is that between "will" and "shall." But the two-faced lot that can only fall head or tail can only give a simple yes or no, which is often too simple for either the diviner or the gambler. So we find African negroes divining with a number of cowries thrown together to see how many fall up and how many down; and this, too, is the Chinese method of solemn lot-casting in the temple, when the falling of the spoon-like wooden lots, so many up and so many down, furnishes an intricate result which is to be interpreted by means of the book of mystic diagrams. When this combination of a number of two-faced lots is used by gamblers, this, perhaps, represents the earlier stage of gaming, which may have led up to the invention of dice, in which the purpose of variety is so much more neatly and easily attained. The

* 'Primitive Culture,' chap. iii.

first appearance of dice lies beyond the range of history, for though they have not been traced in the early periods in Egypt, there is in the Rig-Veda the hymn which portrays the ancient Aryan gambler stirred to frenzy by the fall of the dice. It is not clear even which came first of the various objects that have served as dice.

In the classic world, girls used the astragali or hucklebones as playthings, tossing them up and catching them on the back of the hand; and to this day we may see groups of girls in England at this ancient game, reminding us of the picture by Alexander of Athens, in the Naples Museum, of the five goddesses at play. It was also noticed that these bones fall in four ways, with the flat, concave, convex, or sinuous side up, so that they form natural dice, and as such they have been from ancient times gambled with accordingly. In India nature provides certain five-sided nuts that answer the purpose of dice. Of course, when the sides are alike, they must be marked or numbered, as with the four-sided stick-dice of India, and that which tends to supersede all others, the six-sided *kubos*, which gave the Greek geometers the name for the *cube*. Since the old Aryan period many a broken gamester has cursed the hazard of the die. We moderns are apt to look down with mere contempt at his folly. But we judge the ancient gamester too harshly if we forget that his passion is mixed with those thoughts of luck or fortune or superhuman intervention, which form the very mental atmosphere of the soothsayer and the oracle-prophet. With devout prayer and sacrifice he would propitiate the deity who should give him winning throws; nor, indeed, in our own day have such hopes and such appeals ceased among the uneducated. To the educated it is the mathematical theory of probabilities that has shown the folly of the gamester's staking his fortune on his powers of divination. But it must be borne in mind that this theory itself was, so to speak, shaken out of the dice-box. When the gambling Chevalier de Méré put the question to Pascal in how many throws he ought to get double-sixes, and Pascal solving the problem, started the mathematical calculation of chances, this laid the foundation of the scientific system of statistics which more and more regulates the arrangements of society. Thus accurate method was applied to the insurance table, which enables a man to hedge against his ugliest risks, to eliminate his chances of fire and death by betting that he shall have a new roof over his head and a provision for his widow. Of all the wonderful turns of the human mind in the course of culture, scarce any is more striking than this history of lots and dice. Who, in the Middle Ages, could have guessed what would be its next outcome—that magic sunk into sport should rise again as science, and man's failure to divine the future should lead him to success in controlling it?

Already in the ancient world there appear mentions of games where the throws of lots or dice, perhaps at first merely scored with counters on a board, give the excitement of chance to a game which is partly a draught-game, the player being allowed to judge with

which pieces he will move his allotted number. In England this group of games is represented by *backgammon.* When Greek writers mention dice-playing, they no doubt often mean some game of this class, for at mere hazard the Persian queen-mother could not have played her game carefully, as Plutarch says she did, nor would there have been any sense in his remark that in life, as in dicing, one must not only get good throws, but know how to use them. The Roman game of the twelve lines (*duodecim scripta*) so nearly corresponded with our trictrac or backgammon, that M. Becq de Fouquières, in his 'Jeux des Anciens,' works out on the ordinary backgammon board the problem of the Emperor Zeno that has vexed the soul of many a critic. All these games, however, are played with dice, and as there exist other games of like principle where lots are thrown instead of dice, it may, perhaps, be inferred that such ruder and clumsier lot-backgammon was the earlier, and dice-backgammon a later improvement upon it. Of course things may have happened the opposite way. Lot-backgammon is still played in the East in more than one form. The Arabic-speaking peoples call it *tab*, or game, and play it with an oblong board or rows of holes in the ground, with bits of brick and stone for draughts of the two colours, and for lots four palm-stick slips with a black and white side. In this low variety of lot-backgammon, the object is not to get one's own men home, but to take all the adversary's. The best representative of this group of games is the Hindu *pachisi*, which belongs to a series ancient in India. It is played on a cross-shaped board or embroidered cloth, up and down the arms of which the pieces move and take, in somewhat the manner of backgammon, till they get back to the central home. The men move by the throws of a number of cowries, of which the better throws not only score high, but entitle the player to a new throw, which corresponds to our rule of doubles giving a double move at backgammon. The game of pachisi has great vogue in Asia, extending into the far East, where it is played with flat tamarind-seeds as lots. It even appears to have found its way still farther eastward into America, forming a link in the chain of evidence of an Asiatic element in the civilization of the Aztecs.* For the early Spanish-American writers describe, as played at the Court of Montezuma, a game called *patolli*, played after the manner of their European tables or backgammon, but on a mat with a diagram like a + or Greek cross, full of squares on which the different coloured stones or pieces of the players were moved according to the throws of a number of marked beans. Without the board and pieces, the mere throwing hazards with the beans or lots, to bet on the winning throws, furnishes the North American tribes with their favourite means of gambling, the game of plumstones, game of the bowl, &c.

It is a curious inquiry what led people to the by no means obvious

* See the Author's paper in the 'Journal of the Anthropological Institute,' November, 1878.

idea of finding sport in placing stones or pieces on a diagram and moving them by rule. One hint as to how this may have come about is found in the men at backgammon acting as though they were "counters" counting up the throws. The word *abax*, or *abacus*, is used both for the reckoning-board with its counters and the play-board with its pieces, whence a plausible guess has been made that playing on the ruled board came from a sportive use of the serious counting instrument. The other hint is that board-games, from the rudest up to chess, are so generally of the nature of *kriegspiel*, or war-game, the men marching on the field to unite their forces or capture their enemies, that this notion of mimic war may have been the very key to their invention. Still these guesses are far from sufficient, and the origin of board-games is still among the anthropologist's unanswered riddles. The simpler board-games of skill, that is, without lots or dice, and played by successive moves or draws of the pieces, may be classed accordingly as games of *draughts*, this term including a number of different games, ancient and modern.

The ancient Egyptians were eager draught-players; but though we have many pictures, and even the actual boards and men used, it is not clear exactly how any of their games were played. Ingenuity and good heavy erudition have been misspent by scholars in trying to reconstruct ancient games without the necessary data, and I shall not add here another guess as to the rules of the draughts with which Penelope's suitors delighted their souls as they sat at the palace gates on the hides of the oxen they had slaughtered; nor will I discuss the various theories as to what the "sacred line" was in the Greek game of the "five lines," mentioned by Sophokles. It will be more to the purpose to point out that games worth keeping up hardly die out, so that among existing sports are probably represented, with more or less variation, the best games of the ancients. On looking into the mentions of the famous Greek draught-game of *plinthion*, or *polis*, it appears that the numerous pieces, or "dogs," half of them of one colour and half of the other, were moved on the squares of the board, the game being for two of the same colour to get one of the other colour between them, and so take him. The attempt to reason out from this the exact rules of the classic game has not answered. But on looking, instead of arguing, I find that a game just fitting the description still actually exists. The donkey-boys of Cairo play it in the dust with "dogs," which are bits of stone and red brick, and the guides have scratched its *sîga*, or diagram, on the top of the great pyramid. If it was not there before, it would have come with Alexander to Alexandria, and has seemingly gone on unchanged since. There is an account of it in Lane's 'Modern Egyptians,' and anyone interested in games will find it worth trying with draughts on a cardboard square. One kind of the Roman game of *latrunculi* was closely related to this, as appears from such passages as Ovid's "cum medius gemino calculus hoste perit," referring to the stone being taken between two enemies. The poet mentions, a few lines farther on, the little table with its

three stones, where the game is "continuasse suos," to get your men in a line, which is, of course, our own childish game of *tit-tat-to*. This case of the permanence of an ancient game was long ago recognized by Hyde in his treatise, 'De Ludis Orientalibus.' It is the simplest form of the group known to us as *mill*, *merelles*, *morris*, played by children all the way across from Shetland to Singapore. Among the varieties of draught-games played in the world, one of the most elaborate is the Chinese *wei-chi*, or game of circumvention, the honoured pastime of the learned classes. Here one object is to take your enemy by surrounding him with four of your own men, so as to make what is called an "eye," which looks as though the game belonged historically to the same group as the simpler classic draughts, where the man is taken between two adversaries. In modern Europe the older games of this class have been superseded by one on a different principle. The history of what we now call *draughts* is disclosed by the French dictionary, which shows how the men used to be called *pions*, or pawns, till they reached the other side of the board, then becoming *dames* or queens. Thus the modern game of draughts is recognized as being, in fact, a low variety of chess, in which the pieces are all pawns, turned into queens in chess-fashion when they gain the adversary's line. The earliest plain accounts of the game are in Spanish books of the Middle Ages, and the theory of its development through the mediæval chess problems will be found worked out by the best authority on chess, Dr. A. van der Linde, in his 'Geschichte des Schachspiels.'

The group of games represented by the Hindu *tiger-and-cows*, our *fox-and-geese*, shows in a simple way the new situations that arise in board-games when the men are no longer all alike, but have different powers, or moves. Isidore of Seville (about A.D. 600) mentions, under the name of *latrunculi*, a game played with pieces of which some were common soldiers (*ordinarii*), marching step by step, while others were wanderers (*vagi*). It seems clear that the notions of a kriegspiel, or war-game, and of pieces with different powers moving on the chequer-board, were familiar in the civilized world at the time when, in the eighth century or earlier, some inventive Hindu may have given them a more perfect organization by setting on the board two whole opposing armies, each complete in the four forces, foot, horse, elephants, and chariots, from which an Indian army is called in Sanskrit *chaturanga*, or "four-bodied." The game thus devised was itself called *chaturanga*, for when it passed into Persia it carried with it its Indian name in the form *shatranj*, still retained there, though lost by other nations who received the game from Persia, and named it from the Persian name of the principal piece, the *shah*, or king, whence *schach*, *eschecs*, *chess*. According to this simple theory, which seems to have the best evidence, chess is a late and high development arising out of the ancient draught-games. But there is another theory maintained by Professor Duncan Forbes in his 'History of Chess,' and prominent in one at least of our chess handbooks, which practically amounts to saying that chess is

derived from backgammon. It is argued that the original game was the Indian fourfold-chess, played with four half-sets of men, black, red, green, and yellow, ranged on the four sides of the board, the moves of the pieces being regulated by the throws of dice; that in course of time the dice were given up, and each two allied half-sets of men coalesced into one whole set, one of the two kings sinking to the position of minister, or queen. Now this fourfold Indian dice-chess is undoubtedly a real game, but the mentions of it are modern, whereas history records the spread of chess proper over the East as early as the tenth century. In the most advanced Indian form of *pachisi*, called *chupur*, there are not only the four sets of different-coloured men, but the very same stick-dice that are used in the dice-chess, which looks as though this latter game, far from being the original form of chess, were an absurd modern hybrid resulting from the attempt to play backgammon with chess-men. This is Dr. van der Linde's opinion, readers of whose book will find it supported by more technical points, while they will be amused with the author's zeal in belabouring his adversary Forbes, which reminds one of the legends of mediæval chess-players, where the match naturally concludes by one banging the other about the head with the board. It is needless to describe here the well-known points of difference between the Indo-Persian and the modern European chess. On the whole, the Indian game has substantially held its own, while numberless attempts to develop it into philosophers' chess, military tactics, &c., have been tried and failed, bringing, as they always do, too much instructive detail into the plan which in ancient India was shaped so judiciously between sport and science.

In this survey of games I have confined myself to such as offered subjects for definite remark, the many not touched on including cards, of which the precise history is still obscure. Of the conclusions brought forward, most are no doubt imperfect, and some may be wrong, but it seemed best to bring them forward for the purpose of giving the subject publicity, with a view to inducing travellers and others to draw up minutely accurate accounts of all undescribed games they notice. In Cook's 'Third Voyage' it is mentioned that the Sandwich Islanders played a game like draughts with black and white pebbles on a board of 14 by 17 squares. Had the explorers spent an hour in learning it, we should perhaps have known whether it was the Chinese or the Malay game, or what it was; and this might have been the very clue, lost to native memory, to the connection of the Polynesians with a higher Asiatic culture in ages before a European ship had come within their coral reefs.

It remains to call attention to a point which this research into the development of games brings strongly into view. In the study of civilization, as of so many other branches of natural history, a theory of gradual evolution proves itself a trustworthy guide. But it will not do to assume that culture must always come on by regular un-

varying progress. That, on the contrary, the lines of change may be extremely circuitous, the history of games affords instructive proofs. Looking over a playground wall at a game of hockey, one might easily fancy the simple line of improvement to have been that the modern schoolboy took to using a curved stick to drive the ball with, instead of hurling it with his hands as he would have done if he had been a young Athenian of B.C. 500. But now it appears that the line of progress was by no means so simple and straight, if we have to go round by Persia, and bring in the game of polo as an intermediate stage. If, comparing Greek draughts and English draughts, we were to jump to the conclusion that the one was simply a further development of the other, this would be wrong, for the real course appears to have been that some old draught-game rose into chess, and then again a lowered form of chess came down to become a new game of draughts. We may depend upon it that the great world-game of evolution is not played only by pawns moving straight on, one square before another, but that long-stretching moves of pieces in all directions bring on new situations, not readily foreseen by minds that find it hard to see six moves ahead upon a chess-board.

[E. B. T.]

WEEKLY EVENING MEETING,

Friday, March 28, 1879.

Sir W. Frederick Pollock, Bart. M.A. Vice-President, in the Chair.

Major-General Sir Henry C. Rawlinson, K.C.B. F.R.S.

The Geography of the Oxus, and the Changes of its Course at different Periods of History.

[Memoirs on the subject will probably be given in the 'Journal of the Royal Geographical Society.']

WEEKLY EVENING MEETING,

Friday, April 4, 1879.

William Spottiswoode, Esq. D.C.L. LL.D. Pres. R.S. Vice-President, in the Chair.

William Crookes, F.R.S.

Molecular Physics in High Vacua.

When I was asked, a month or two ago, to illustrate in this theatre some of my recent researches on Molecular Physics in High Vacua, I exclaimed "How is it possible to bring such a subject worthily before a Royal Institution audience when none of the experiments can be seen more than three feet off?" If to-night I am fortunate enough to show all the experiments to those who are not far distant, and if I succeed in making most of them visible at the far end of the theatre, such a success will be entirely due to the great kindness of your late Secretary, Mr. Spottiswoode, who has placed at my disposal his magnificent induction-coil,—not only for this lecture, but for some weeks past in my own Laboratory,—thus enabling me to prepare apparatus and vacuum tubes on a scale so large as to relieve me of all anxiety so far as the experimental illustrations are concerned.

Before describing the special researches in molecular physics which I propose to illustrate this evening, it is necessary to give a brief outline of one small department of the modern theory of the constitution of gases. It is not easy to make clear the kinetic theory, but I will try to simplify it in this way:—Imagine that I have in a large box a swarm of bees, each bee independent of its fellow, flying about in all manner of directions and with very different velocities. The bees are so crowded that they can only fly a very short distance without coming into contact with one another or with the sides of the box. As they are constantly in collision, so they rebound from each other with altered velocities and in different directions, and when these collisions take place against the sides of the box pressure is produced. If I take some of the bees out of the box, the distance which each individual bee will be able to fly before it comes into contact with its neighbour will be greater than when the box was full of bees, and if I remove a great many of the bees I increase to a considerable extent the average distance that each can fly without a collision. This distance I will call the bee's *mean free path*. When

the bees are numerous the mean free path is very short; when the bees are few the mean free path will be longer, the length being inversely proportional to the number of bees present. Let us now imagine a loose diaphragm to be introduced in the centre of the box, so as to divide the number of bees equally. The same number of bees being on each side, the impacts on the diaphragm will be equal; and the mean speed of the bees being the same, the pressure will be identical on each side of the diaphragm, and it will not move.

Let me now warm one side of this division so as to let it communicate extra energy to a bee when it touches it. As before, a bee will strike the diaphragm with its normal mean velocity, but will be driven back with extra velocity, the reaction producing an increase of pressure on the diaphragm. It will be found, however, that although the diaphragm is free to move, the extra strength of the recoil on the warm side does not produce any motion. This at first sight seems contrary to the law of action and reaction being equal. The explanation is not difficult to understand. The bees which fly away from the diaphragm have drawn energy from it, and therefore move quicker than those which are coming towards it; they beat back the crowd to a greater distance, and keep a greater number from striking the diaphragm. Near to the heated side of the diaphragm the density is less than the average, while beyond the free path the density is above the average, and this greater crowding extends to all other parts of the box. Thus it happens that the extra energy of the impacts against the warm side of the diaphragm is exactly compensated by the increased number of impacts on the cool side. In spite therefore of the increased activity communicated to a portion of the bees, the pressure on the two sides of the diaphragm will remain the same. This represents what occurs when the extent of the box containing the bees is so great, compared with the mean free path, that the abrupt change in the velocities of those bees which rebound from the walls of the box produces only an insensible influence on the motions of bees at so great a distance as the diaphragm.

I will next ask you to imagine that I am gradually removing bees from our box, still keeping the diaphragm warm on one side. The bees getting fewer the collisions will become less frequent, and the distance each bee can fly before striking its neighbour will get longer and longer, and the crowding in front of them will grow less and less. The compensation will also diminish, and the warmed side of the diaphragm will have a tendency to be beaten back. A point will at last be reached on the warm side, when the mean free path of the bees will be long enough to admit of their dashing right across from the diaphragm to the side of the box, without meeting more than a certain number of in-coming bees in their flight. In this case the bees will no longer fly quite in the same direction as before. They will now fly less sideways, and more forwards and backwards between the heated face of the diaphragm and the opposed wall of the box.

Because of this preponderating motion, and also because they will thereby less effectually keep back bees crowding in from the sides, there will now be a greater proportionate pressure both on the hot face of the diaphragm and on that part of the box which is in front of it. Hence the pressure on the hot side will now exceed that on the cool side of the diaphragm, which will consequently have a backward movement communicated to it.

I may diminish the size of the bees as much as I like, and by correspondingly increasing their number the mean free path will remain the same. Instead of bees let me call them molecules, and instead of having a few hundreds or thousands in the box let me have millions and billions and trillions; and if we also diminish the mean free path to a considerable extent, we get a rough outline of the kinetic theory of gases. (I may just mention that the mean free path of the molecules in air, at the ordinary pressure, is the ten-thousandth of a millimetre.)

Three years ago I had the honour of bringing before you the results of some researches on the Radiometer. Let me now take up the subject where I then left off. I have here two radiometers which have been rotating before you under the influence of a strong light shining upon them.

The explanation of the movement of the radiometer is this,—the light, or the total bundle of rays included in the term "light," falling upon the blackened side of the vanes, becomes absorbed, and thereby raises the temperature of the black side: this causes extra excitement of the air molecules which come in contact with it, and pressure is produced, causing the fly of the radiometer to turn round.

I have long believed that a well-known appearance observed in vacuum tubes is closely related to the phenomena of the mean free path of the molecules. When the negative pole is examined while the discharge from an induction-coil is passing through an exhausted tube, a dark space is seen to surround it. This dark space is found to increase and diminish as the vacuum is varied, in the same way that the ideal layer of molecular pressure in the radiometer increases and diminishes. As the one is perceived by the mind's eye to get greater, so the other is seen by the bodily eye to increase in size. If the vacuum is insufficient to permit the radiometer to turn, the passage of electricity shows that the "dark space" has shrunk to small dimensions. It is a natural inference that the dark space is the mean free path of the molecules of the residual gas.

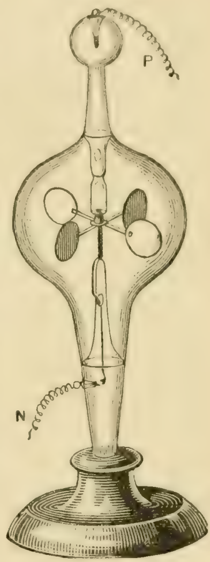
The radiometer which has just been turning under the influence of the lime-light is not of the ordinary kind. Fig. 1 will explain its construction.

It is similar to an ordinary radiometer with aluminium disks for vanes, each disk coated on one side with a film of mica. The fly is supported by a hard steel instead of glass cup, and the needle point on which it works is connected by means of a wire with a platinum terminal sealed into the glass. At the top of the radiometer bulb

a second terminal is sealed in. The radiometer can therefore be connected with an induction-coil, the movable fly being made the negative pole.

As soon as the pressure is reduced to a few millims. of mercury, a halo of velvety violet light forms on the metallic side of the vanes, the mica side remaining dark. As the pressure diminishes, a dark space is seen to separate the violet halo from the metal. At a pressure of half a millim. this dark space extends to the glass, and positive rotation commences. On continuing the exhaustion the dark space further widens out and appears to flatten itself against the glass, when the rotation becomes very rapid.

Fig. 1.



You perceive a dark space behind each vane and moving round with it. In the first experiment, radiation from the lime-light falling on the metallic sides of the vanes, produced a layer of molecular pressure which drove the fly round; so here the induction-current has produced molecular excitement at the surface of the vanes forming the negative pole, extending up to the side of the glass.

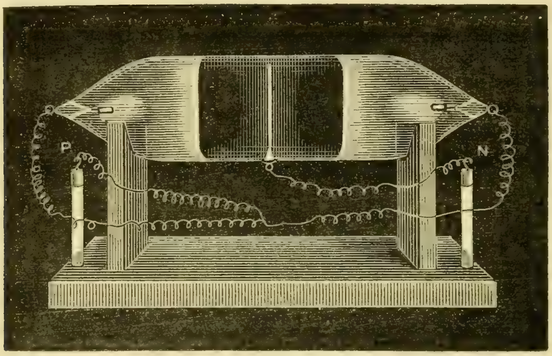
When the negative pole is in rapid rotation it is not easy to see this dark space, so I have arranged a tube in which the dark space will be visible to all present. The tube, as you will see by the diagram (Fig. 2), has a pole in the centre in the form of a metal disk, and other poles at each end. The centre pole is made negative, and the two end poles connected together are made the positive terminal. The dark space will be in the centre. When the exhaustion is not very great the dark space extends only a little distance on each side of the negative pole in the centre. When the exhaustion is very good, as it is in the tube before you, and I turn on the coil, the dark space is seen to extend for about 2 inches on each side of the pole.

Here, then, we see the induction spark actually illuminating the lines of molecular pressure caused by the excitement of the negative pole. The thickness of this dark space—nearly 2 inches—is the measure of the mean free path between successive collisions of the molecules of the residual gas. The extra velocity with which the negatively electrified molecules rebound from the excited pole keeps back the more slowly moving molecules which are advancing towards that pole. The conflict occurs at the boundary of the dark space, where the luminous margin bears witness to the energy of the discharge.

I will endeavour to throw on the screen an illustration of this

dark space. A stream of water falls from a small jet on to a horizontal plate of glass. The water spreads over the plate and forms a thin film. The jet of water in the centre, from the velocity of its fall, drives the film of water before it on all sides, raising it into a ring-shaped heap. As I diminish the force of the jet the ring contracts: this is equivalent to the exhaustion getting less. When I increase the force of water the ring expands in size, the effect being analogous to an increase of exhaustion in my tubes. The extra velocity of the falling particles of water drives the in-coming particles of water before them, and raises a ridge round the side which exactly

Fig. 2.



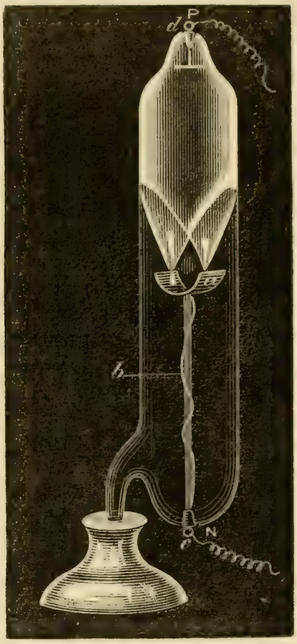
represents the luminous halo to the dark space to be seen in this tube.

If, instead of a flat disk, a metal cup is used for the negative pole, the successive appearances on exhausting the tube are somewhat different. The velvety violet halo forms over each side of the cup. On increasing the exhaustion the dark space widens out, retaining almost exactly the shape of the cup. The bright margin of the dark space becomes concentrated at the concave side of the cup to a luminous focus, and widens out at the convex side. When the dark space is very much larger than the cup, its outline forms an irregular ellipsoid drawn in towards the focal point. Inside the luminous boundary a dark violet light can be seen converging to a focus, and, as the rays diverge on the other side of the focus, spreading beyond the margin of the dark space; the whole appearance being strikingly similar to the rays of the sun reflected from a concave mirror through a foggy atmosphere. This proves a somewhat important point; it shows that the molecules thrown off the excited negative pole leave it in a direction almost normal to the surface.

I can illustrate this property of the molecular rays by an experiment. This diagram (Fig. 3) is a representation of the tube which is before you. It contains, as a negative pole, a hemi-cylinder (*a*) of

polished aluminium. This is connected with a fine copper wire, *b*, ending at the platinum terminal, *c*. At the upper end of the tube is another terminal, *d*. The induction-coil is connected so that the hemi-cylinder is negative and the upper pole positive, and when exhausted to a sufficient extent, as is the case with this tube, the projection of the molecular rays to a focus is very beautifully shown. The rays are driven from the hemi-cylinder in a direction normal to its surface; they come to a focus and then diverge, tracing their path in brilliant green phosphorescence on the surface of the glass.

Fig. 3.



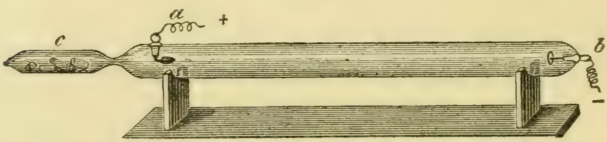
You will notice that the rays which project from the negative pole and cross in the centre have a bright green appearance; that colour is entirely due to the phosphorescence of the glass. At a very high exhaustion the phenomena noticed in ordinary vacuum tubes when the induction spark passes through them—an appearance of cloudy luminosity and of stratifications—disappears entirely. No cloud or fog whatever is seen in the body of the tube, and with such a vacuum as I am working with in these experiments—about a millionth part of an atmosphere—the inner surface of the glass glows with a rich green phosphorescence, the intensity of colour varying with the perfection of the vacuum. It scarcely begins to show much before the 800,000th of an atmosphere. At about a millionth of an atmosphere the phosphorescence is very strong, and after that it begins to diminish until there are not enough molecules left to allow the spark to pass.*

I have here a tube which will serve to illustrate the dependence of the green phosphorescence of the glass on the degree of perfection of the vacuum (Fig. 4). The two poles are at *a* and *b*, and at the end (*c*) is a small supplementary tube connected with the other by a narrow aperture, and containing solid caustic potash. The tube has been exhausted to a very high point, and the potash heated so as to drive off moisture and deteriorate the vacuum. Exhaustion has then

*	1·0 millionth of an atmosphere	=	0·00076 millim.
	1315·789 millionths of an atmosphere	=	1·0 millim.
	1,000,000 ,, ,, ,,	=	760·0 millims.
	,, ,, ,, ,,	=	1 atmosphere.

been re-commenced, and the alternate heating and exhaustion have been repeated until the tube has been brought to the state in which it now appears before you. When the induction spark is first turned on nothing is visible—the vacuum is so high that the tube is non-conducting. I now warm the potash slightly, and liberate a trace of aqueous vapour. Instantly conduction commences, and the green phosphorescence flashes out along the length of the tube. I continue the heat, so as to drive off more gas from the potash. The green gets fainter, and now a wave of cloudy luminosity sweeps over the tube, and stratifications appear. These rapidly get narrower, until the spark passes along the tube in the form of a narrow purple line. I take the lamp away, and allow the potash to cool; as it cools, the aqueous vapour, which the heat had driven off, is re-

Fig. 4.



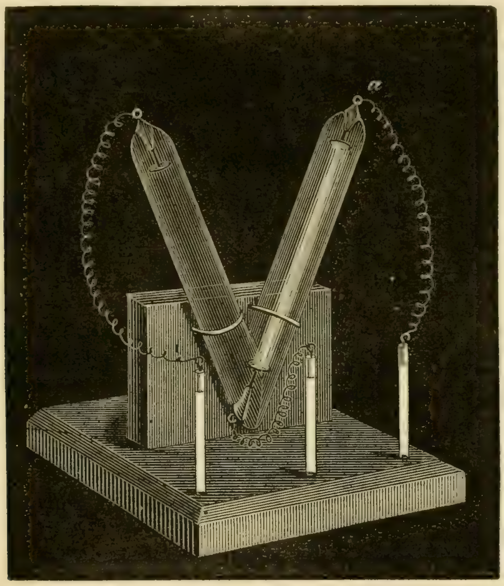
absorbed. The purple line broadens out, and breaks up into fine stratifications; these get wider, and travel towards the potash tube. Now a wave of green light appears on the glass at the other end, sweeping on and driving the last pale stratification into the potash; and now the tube glows over its whole length with the green phosphorescence. Would time allow I might keep it before you, and show the green growing fainter and the vacuum becoming non-conducting; but time is required for the absorption of the last traces of vapour by the potash, and I must pass on to the next subject.

This green phosphorescence is a subject that has much occupied my thoughts, and I have striven to ascertain some of the laws governing its occurrence. I soon perceived that the phosphorescence was not in the body of the tube itself, but was entirely on the surface of the glass. Another peculiarity of the rays producing this green phosphorescence is that they will not turn a corner in the slightest degree. Here is a V-shaped tube (Fig. 5), a pole being at each extremity. The pole at the right side (a) being negative, you see that the whole of the right arm is flooded with green light, but at the bottom it stops sharply, and will not turn the corner to get into the left side. When I reverse the current, and make the left pole negative, the green changes to the left side, always following the negative pole, leaving the positive side with scarcely any luminosity.

In the ordinary phenomena exhibited by vacuum tubes—phenomena with which we are all familiar—it is customary, for the more striking illustration of their contrasts of colour, to have the tubes bent into very elaborate designs. The positive luminosity

caused by the phosphorescence of the residual gas follows all the convolutions and designs into which skilful glass-blowers can manage to twist the glass. The negative pole being at one end and the positive pole at the other, the luminous phenomena seem to depend more on the positive than on the negative at an ordinary exhaustion such as has hitherto been used to get the best phenomena of vacuum tubes. I have here two bulbs (Fig. 6), alike in shape and position of poles, the only difference being that one is at an exhaustion equal to a few millimetres of mercury—such a moderate exhaustion as will give stratifications or the ordinary luminous phenomena—whilst the other is exhausted to about the millionth of an atmosphere. I will

Fig. 5.

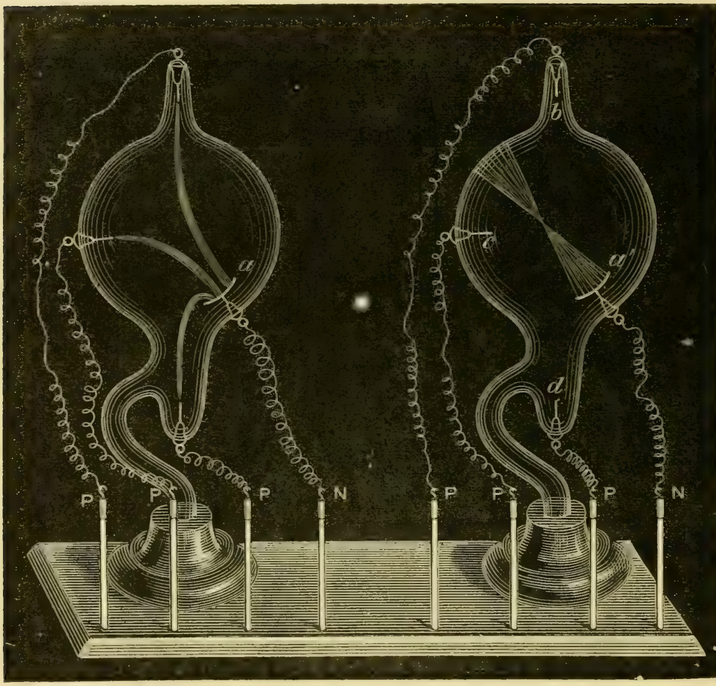


first connect the moderately exhausted bulb with the induction-coil, and, retaining the pole at one side (*a*) always negative, I will put the positive wire successively to the other three poles with which the bulb is furnished. You will see that as I change the position of the positive pole, the line of violet light joining the two poles changes. In this moderately exhausted bulb, therefore, the electric current always chooses the shortest path between the two poles, and moves about the bulb as I alter the position of the wires.

This, then, is the kind of phenomenon we get in ordinary exhaustions. I will now try the same experiment with a tube that is highly

exhausted, and, as before, will make the side pole (a') the negative, the top pole (b) being positive. Notice how widely different is the appearance from that shown by the last bulb. The negative pole is in the form of a shallow cup. The bundle of rays from the cup crosses in the centre of the bulb, and thence diverging falls on the opposite side as a circular patch of green light. As I turn the bulb

FIG. 6.



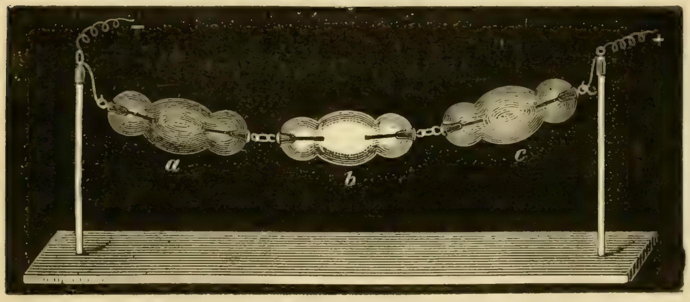
round you will all be able to see the faint blue focus and the green patch on the glass. Now observe, I remove the positive wire from the top, and connect it with the side pole (c). The green patch from the divergent negative focus is still there. I now make the lowest pole (d) positive, and the green patch still remains where it was at first, unchanged in position or intensity.

This, then, gives us another fact which brings us a little nearer to the cause of this green phosphorescence. It is this—that in the low vacuum the position of the positive pole is of every importance, whilst in a high vacuum it scarcely matters at all where the positive pole is; the phenomena seem to depend entirely on the negative pole. In very high vacua, such as we have been using, the phenomena

follow altogether the negative pole. If the negative pole points in the direction of the positive all very well, but if the negative pole is entirely in the opposite direction it does not matter: the line of rays is projected all the same in a straight line from the negative.

I have hitherto spoken of and illustrated these phenomena in connection with *green* phosphorescence. It does not follow, however, that the phosphorescence is always of that colour. This colouration is a property of the particular kind of glass in use in my laboratory. I have here (Fig. 7) three bulbs composed of different glass: one is uranium glass (*a*), which phosphoresces of a dark green colour; another is English glass (*b*), which phosphoresces of a blue colour; and the third (*c*) is soft German glass—of which most of the apparatus before you is made—which phosphoresces of a bright apple-green

Fig. 7.

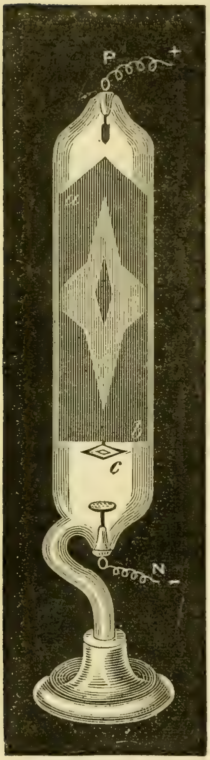


colour. It is therefore plain that this particular green phosphorescence is solely due to the glass which I am using. Were I to use English glass I should have to speak of blue phosphorescence, but I know of no glass which is equal to the German in brilliancy.

My earlier experiments were almost entirely carried on by the aid of the phosphorescence which glass takes up when it is under the influence of the electric discharge *in vacuo;* but many other substances possess this phosphorescent power, and some have it in a much higher degree than glass. For instance, here is some of the luminous sulphide of calcium prepared according to M. Ed. Becquerel's description. When it is exposed to light—even candlelight—it phosphoresces for hours with a rich blue colour. I have prepared a diagram with large letters written in this luminous sulphide; before it is exposed to the light the letters are invisible, but Mr. Gimingham has just exposed it in another room to burning magnesium, and now it is brought into the darkened theatre you will see the word "φως,"—*light*, a very suitable word for so beautiful a phosphorescence—shining brightly in luminous characters. The first letter, φ, shines with an orange light; it is a sulphide of calcium prepared from oyster-shells. The other letters, shining with a blue light, are sulphide of calcium prepared from precipitated carbonate of lime. Once the phospho-

rescence is excited the letters shine for several hours. I will put the diagram at the back, and we shall see how it lasts during the remainder of the lecture. This substance, then, is phosphorescent to light, but it is also much more strongly phosphorescent to the molecular discharge in a good vacuum, as you will see when I pass the discharge through this tube (Fig. 8). The white plate (a, b) in the centre of the tube is a sheet of mica painted over with the luminous sulphide of which the letter ϕ was composed in the diagram you have just seen. On connecting the poles with the coil the mica screen glows with a strong yellowish green light, bright enough to illuminate all the apparatus near it. But there is another phenomenon to which I now desire to draw attention: on the luminous screen is a kind of distorted star-shaped figure. A little in front of the negative pole I have fixed a star (c) cut out in aluminium, and it is the image of this star which you see on the screen. It is evident that the rays coming from the negative pole project an image of anything that happens to be in front of it. The discharge, therefore, must come from the pole in straight lines, and does not merely permeate all parts of the tube and fill it with light as it would were the exhaustion less good. Where there is nothing in the way the rays strike the screen and produce phosphorescence, and where there is an obstacle they are obstructed by it, and a shadow is thrown on the screen. I shall have more to say about this shadow presently; I merely now wish to establish the fact that these rays driven from the negative pole produce a shadow.

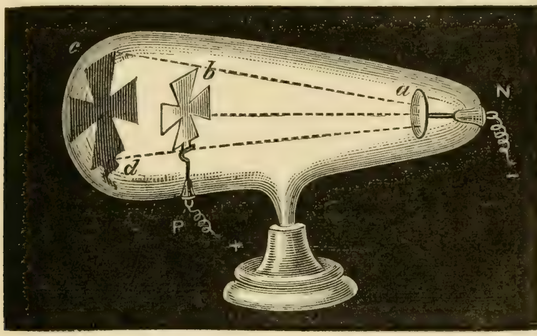
Fig. 8.



I must draw your attention to an important experiment connected with these molecular rays, but unfortunately it is a very delicate one, and very difficult to show to many at once; but I hope, if you know beforehand what to look for, you will all be able to see what I wish to show. In this pear-shaped bulb (Fig. 9 A) the negative pole (a) is at the pointed end. In the middle is a cross (b) cut out of sheet aluminium, so that the rays from the negative pole projected along the tube will be partly intercepted by the aluminium cross, and will project an image of it on the hemispherical end of the tube which is phosphorescent. I think you will all now see the shadow of the cross on the end of the bulb (c, d), and notice that the cross is black on a luminous ground. Now, the rays from the negative pole have been passing by the side of the aluminium cross to produce the shadow;

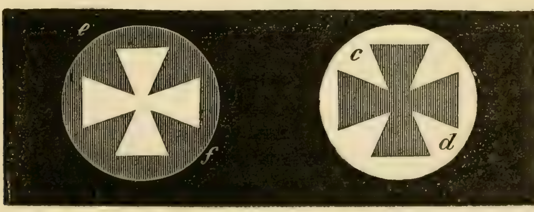
they have been hammering and bombarding the glass till it is appreciably warm, and at the same time they have been producing another effect on that glass—they have deadened its sensibility. The glass has got tired, if I may use the expression, by the enforced phosphorescence. Some change has been produced by this bombard-

FIG. 9 A.



ment which will prevent the glass from responding easily to additional excitement; but the part that the shadow has fallen on is not tired—it has not been phosphorescing at all and is perfectly fresh; therefore if I throw this star down,—I can easily do so by giving the apparatus a slight jerk, for it has been most ingeniously constructed with a hinge by Mr. Gimingham,—and so allow the rays from the negative pole to fall uninterruptedly on to the end of the bulb, you will suddenly see the black cross (*c*, *d*, Fig. 9 B) change to a luminous

FIG. 9 B.



one (*e*, *f*), because the background is only faintly phosphorescing, whilst the part which had the black shadow on it retains its full phosphorescent power. The luminous cross is now dying out. This is a most delicate and venturous experiment, and I am fortunate in having succeeded so well, for it is one that cannot be rehearsed. After resting for a time the glass seems to partly recover its power of phosphorescing, but it is never so good as it was at first.

We have, therefore, found an important fact connected with this

phosphorescence. Something is projected from the negative pole which has the power of hammering away at the glass in front of it, in such a way as to cause it not only to vibrate and become temporarily luminous while the discharge is going on, but to produce an impression upon the glass which is permanent. The explanation which has gradually evolved itself from this series of experiments is this :—The exhaustion in these tubes is so high that the dark space, as I showed you at the commencement of this Lecture, that extended around the negative pole, has widened out till it entirely fills the tube. By great rarefaction the mean free path has become so long that the hits in a given time may be disregarded in comparison to the misses, and the average molecule is now allowed to obey its own motions or laws without interference. The mean free path is in fact comparable to the dimensions of the vessel, and we have no longer to deal with a *continuous* portion of matter, as we should were the tubes less highly exhausted, but we must here contemplate the molecules *individually*. At first this was only a convenient working hypothesis. Long-continued experiment then raised this provisional hypothesis almost to the dignity of a theory, and now the general opinion is that this theory gives a fairly correct explanation of the facts. In these highly exhausted vessels the mean free path of the residual molecules of gas is so long that they are able to drive across from the pole to the other side of the tube with comparatively few collisions. The negatively electrified molecules of the gaseous residue in the tube therefore dash against anything that is in front, and cast shadows of obstacles just as if they were rays of light. Where they strike the glass they are stopped, and the production of light accompanies this sudden arrest of velocity.

Other substances besides English, German, and uranium glass, and Becquerel's luminous sulphides, are also phosphorescent. I think, without exception, the diamond is the most sensitive substance I have yet met for ready and brilliant phosphorescence. I have here a tube, similar to those already exhibited, containing a mica screen painted with powdered diamond, and when I turn on the coil, the brilliant blue phosphorescence of the diamond can be seen, quite overpowering the green phosphorescence of the glass. Here, again, is a very curious diamond, which I was fortunate enough to meet with a short time ago. By daylight it is green, produced, I fancy, by an internal fluorescence. The diamond is mounted in the centre of this exhausted bulb (Fig. 10), and the negative discharge will be directed on it from below upwards. On darkening the theatre you see the diamond shines with as much light as a candle, phosphorescing of a bright green.

In this other bulb is a remarkable collection of crystals of diamonds, which have been lent me by Professor Maskelyne. When I pass the discharge over them I am afraid you will only be able to see a few points of light, but if you will examine them after the Lecture, you will see them phosphoresce with a most brilliant

series of colours—blue, apricot, red, yellowish green, orange, and pale green.

Next to the diamond the ruby is one of the most remarkable

Fig. 10.

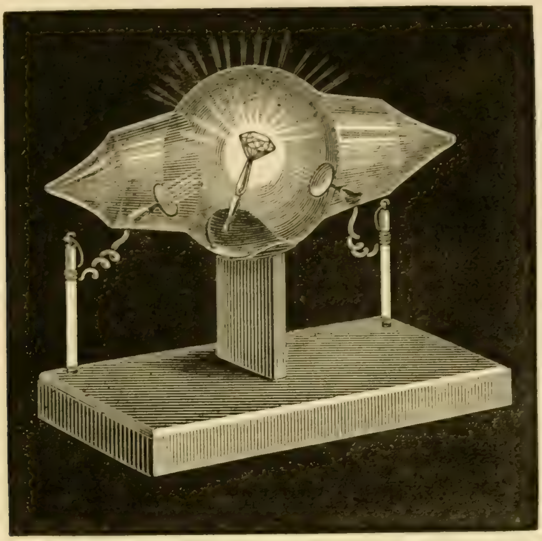
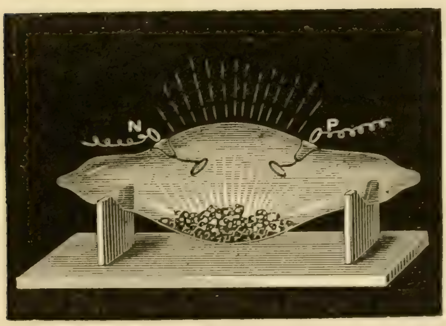


Fig. 11.



stones for phosphorescing. In this tube (Fig. 11) is a collection of ruby pebbles, for the loan of which I am indebted to my friend Mr. Blogg, of the firm of Blogg and Martin, who placed a small sackful at my disposal. As soon as I turn on the induction spark you will see these rubies shining with a brilliant rich red colour, as if they were glowing hot. Now the ruby is nothing but crystallised

alumina with a little colouring-matter, and it became of great interest to ascertain whether the artificial ruby made by M. Feil, of Paris, would glow in the same manner. I had simply to make my wants known to M. Feil, and he immediately sent me a box containing artificial rubies and crystals of alumina of all sizes, and from those I have selected the mass in this tube which I now place under the discharge: they phosphoresce of the same rich red colour as the natural ruby. It scarcely matters what colour the ruby is, to begin with. In this tube of natural rubies there are stones of all colours—the deep red ruby and the pale pink ruby. There are some so pale as to be almost colourless, and some of the highly-prized tint of pigeon's blood; but in the vacuum under the negative discharge they all phosphoresce with about the same colour.

As I have just mentioned, the ruby is crystallised alumina. In a paper published twenty years ago by Ed. Becquerel* I find that he describes the appearance of alumina as glowing with a rich red colour in the phosphoroscope (an instrument by which the duration of phosphorescence in the sunlight can be examined). Here is some chemically pure precipitated alumina which I have prepared in the most careful manner. It has been heated to whiteness, and you see it glows with the rich red colour which is supposed to be characteristic of alumina. The mineral known as corundum is a colourless variety of crystallised alumina. Under the negative discharge in a vacuum, corundum phosphoresces of a rose-pink colour. There is another curious fact in which I think chemists will feel interested. The sapphire is also crystallised alumina, just the same as the ruby. The ruby has a little colouring-matter in it, giving it a red colour; the sapphire has a colouring-matter which gives it a blue colour, whilst corundum is white. I have here in a tube a very fine crystal of sapphire, and, when I pass the discharge over it, it gives alternate bands of red and green. The red we can easily identify with the glow of alumina; but what is the green? If alumina is precipitated and purified as carefully as in the case I have just mentioned, but in a somewhat different manner, it is found to glow with a rich green colour. Here are the two specimens of alumina in tubes, side by side. Chemists would say that there was no difference between one and the other; but I connect them with the induction-coil, and you see that one glows with a bright green colour, whilst the other glows with a rich red colour. Here is a fine specimen of chemically pure alumina, lent me by Messrs. Hopkin and Williams; by ordinary light it is a perfectly white powder. It is just possible that the rich fire of the ruby, which has caused it to be so prized, may be due, not entirely to the colouring-matter, but to its wonderful power of phosphorescing with a deep red colour, not only under the electric discharge in a vacuum, but whenever exposed to a strong light.

The spectrum of the red light emitted by all these varieties of

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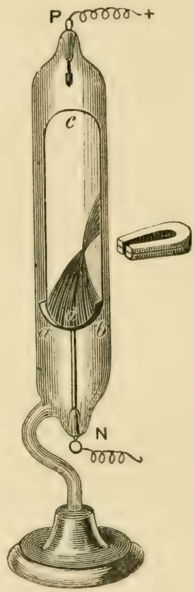
* Annales de Chimie et de Physique, 3rd series, vol. lvii. p. 50, 1859.

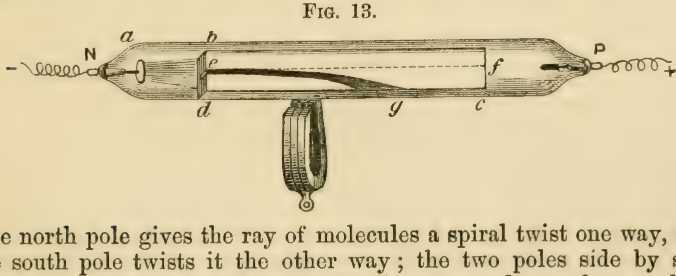
alumina—the ruby, corundum, or artificially precipitated alumina—is the same as described by Becquerel twenty years ago. There is one intense red line, a little below the fixed line B in the spectrum, having a wave-length of about 6895. There is a continuous spectrum beginning at about B, and a few fainter lines beyond it, but they are so faint in comparison with this red line that they may be neglected. This line may be called the characteristic line of alumina.

Fig. 12.



I now pass on to another fact connected with this negative discharge. Here is a tube (Fig. 12) with a negative pole (*a*, *b*) in the form of a hemi-cylinder, similar to the one you have already seen (Fig. 3), but in this case I receive the rays on a phosphorescent screen (*c*, *d*). See how brilliantly the lines of discharge shine out, and how intensely the focal point is illuminated; it lights the whole table. Now I bring a small magnet near, and move it to and fro; the rays obey the magnetic force, and the focus bends one way and the other as the magnet passes it. I can show this magnetic action a little more definitely. Here is a long glass tube (Fig. 13), very highly exhausted, with a negative pole at one end (*a*) and a long phosphorescent screen (*b*, *c*) down the centre of the tube. In front of the negative pole is a plate of mica (*b*, *d*) with a hole (*e*) in it, and the result is that when I turn on the current, a line of phosphorescent light (*e*, *f*) is projected along the whole length of the tube. I now place beneath the tube a powerful horse-shoe magnet: see how the line of light becomes curved under the magnetic influence (*e*, *g*), waving about like a flexible wand as I move the magnet up and down. The action of the magnet can be understood by reference to this diagram (Fig. 14).

Fig. 13.

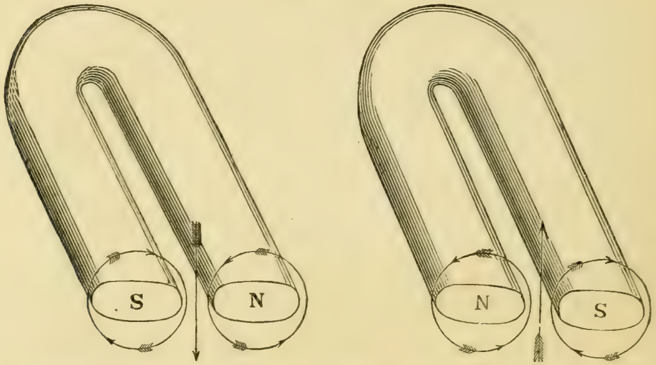


The north pole gives the ray of molecules a spiral twist one way, and the south pole twists it the other way; the two poles side by side compel the ray to move in a straight line up or down, along a plane at right angles to the plane of the magnet and a line joining its poles.

Now it is of great interest to ascertain whether the law governing

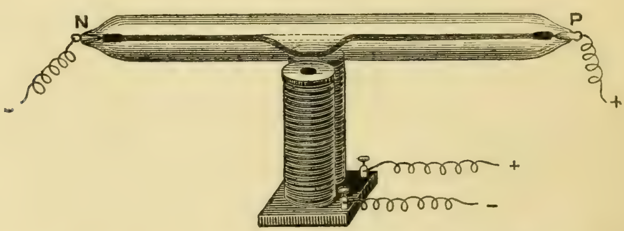
the magnetic deflection of the trajectory of the molecules is the same as has been found to hold good at a lower vacuum. The former experiment was with a very high vacuum. This is a tube with a low vacuum (Fig. 15). On passing the induction spark it passes as a

FIG. 14.



narrow line of violet light joining the two poles. Underneath I have a powerful electro-magnet. I make contact with the magnet, and the line of light dips in the centre towards the magnet. I reverse the poles, and the line is driven up to the top of the tube. Notice the difference between the two phenomena. Here the action is temporary. The dip takes place under the magnetic influence; the line of discharge then rises, and pursues its path to the positive pole. In the high exhaustion, however, after the ray of light had dipped to the magnet it did not recover itself, but continued its path in the altered direction.

FIG. 15.



During these experiments another property of this molecular discharge has made itself very evident, although I have not yet drawn attention to it. The glass gets very warm where the green phosphorescence is strongest. The molecular focus on the tube which we have just seen (Fig. 12) would be intensely hot, and I have prepared an apparatus by which this heat at the focus can be intensified and rendered visible to all present. This small tube (*a*)

(Fig. 16) is furnished with a negative pole in the form of a cup (*b*). The rays will therefore be projected to a focus in the middle of the tube (Fig. 17, *a*). At the side of the tube is a small electro-magnet,

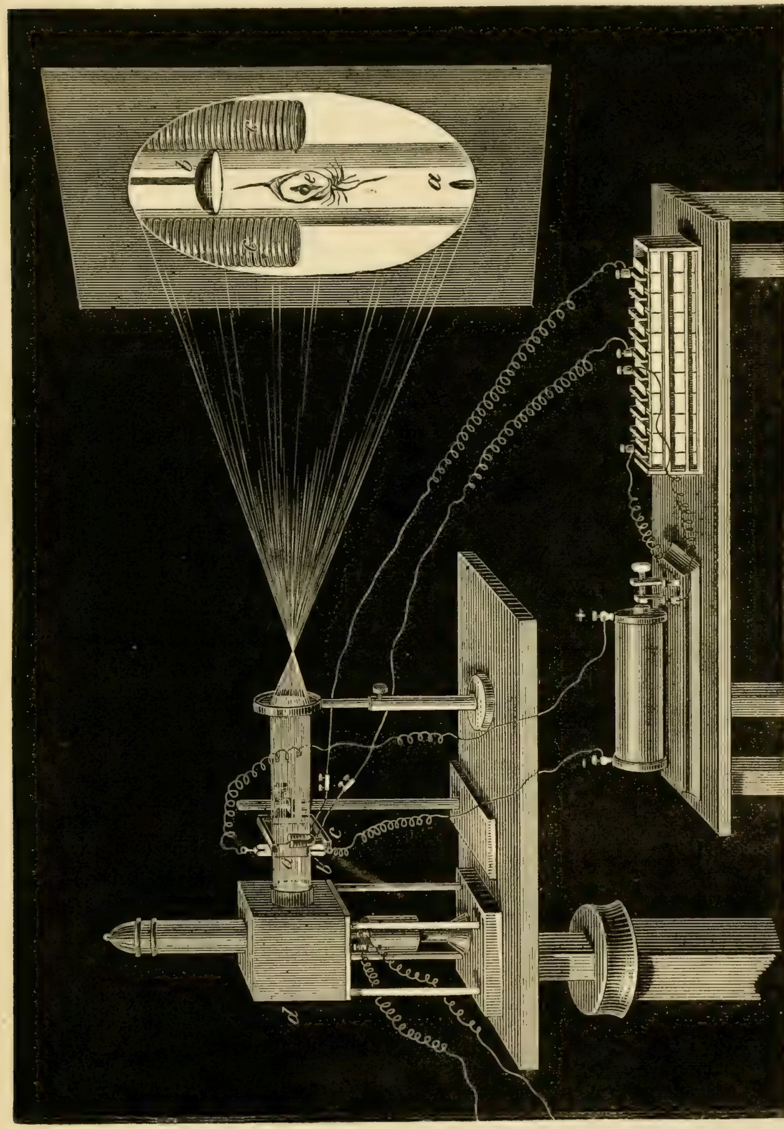


Fig. 16.

which I can set in action by touching a key, and the focus is then drawn to the side of the glass tube (Fig. 17, *b*). To show the first action of the heat I have coated the tube with wax. I will put the apparatus in front of the electric lantern (*d*), and throw a magnified image of the tube on the screen. The coil is now at work, and the focus of molecular rays is projected along the tube. I turn the magnetism on, and draw the focus on the side of the glass. The first thing you see is a small circular patch melted in the coating of wax. The glass soon begins to disintegrate, and cracks are shooting starwise from the centre of heat. The glass is softening. Now the atmospheric pressure forces it in, and now it melts. A hole (*e*) is perforated in the middle, the air rushes in, and the experiment is at an end.

Instead of drawing the focus to the side of the glass with a magnet, I will take another tube (Fig. 18), and allow the focus from

Fig. 17.

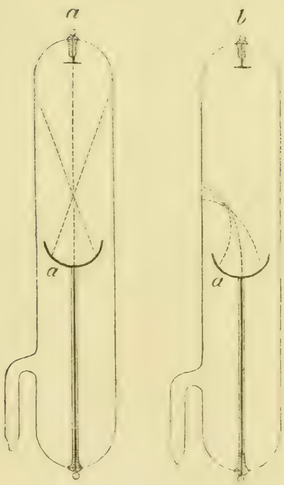


Fig. 18.

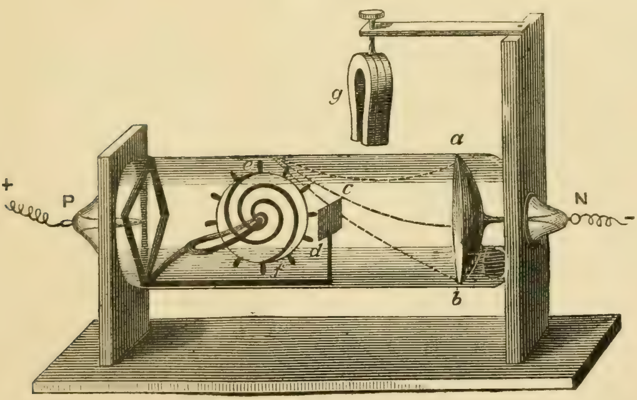
the cup-shaped negative pole (*a*) to play on a piece of platinum wire (*b*) which is supported in the centre of the bulb. The platinum wire not only gets white hot, but you can see sparks coming from it on all sides, showing that it is actually melting.

Here is another tube, but instead of platinum I have put in the focus that beautiful alloy of platinum and iridium which Mr. Matthey

has brought to such perfection, and I think that I shall succeed in even melting that. I first turn on the induction-coil slightly, so as not to bring out its full power. The focus is now playing on the iridio-platinum, raising it to a white heat. I bring a small magnet near, and you see I can deflect the focus of heat just as I did the luminous focus in the other tube. By shifting the magnet I can drive the focus up or down, or draw it completely away from the metal, and render it non-luminous. I withdraw the magnet, and let the molecules have full play again; the metal is now white-hot. I increase the intensity of the spark. The metal glows with almost insupportable brilliancy, and at last melts.

There is still another property of this molecular discharge, and it is this:—You have seen that the molecules are driven violently from the negative pole. If I place something in front of these molecules, they show the force of impact by the heat which is produced. Can I make this mechanical action evident in a more direct way? Nothing is simpler. I have only to put some easily moving object in the line of discharge in order to get a powerful mechanical action. Mr. Gimingham,

FIG. 19.



with great skill, has constructed a piece of apparatus which I will presently put in the electric lantern, so that all will be able to see its action. But first I will explain the construction by means of this diagram (Fig. 19). The negative pole (a, b) is in the form of a very shallow cup. In front of the cup is a mica screen (c, d), wide enough to intercept nearly all the molecular rays coming from the negative pole. Behind this screen is a mica wheel (e, f) with a series of vanes, making a sort of paddle-wheel of it. So arranged, the molecular stream from the pole a b will nearly all be cut off from the wheel, and what escapes over and under the screen will hit the vanes equally, and

will not produce any movement. I now put a magnet, g, over the tube, so as to deflect the stream over or under the obstacle $c\ d$, and the result will be rapid motion in one or the other direction, according to the way the magnet is turned. I now throw the image of the apparatus on the screen. The spiral lines painted on the wheel show which way it turns. I arrange the magnet to draw the molecular stream so as to beat against the upper vanes, and the wheel revolves rapidly, as if it were an over-shot water-wheel. I now turn the magnet so as to drive the molecular stream underneath; the wheel slackens speed, stops, and then begins to rotate the other way, as if it were an under-shot water-wheel. This can be repeated as often as I like to reverse the position of the magnet, the change of rotation of the wheel showing immediately the way the molecular stream is deflected.

This experiment illustrates the last of the phenomena which time allows me to bring before you, attending the passage of the induction spark through a highly exhausted atmosphere. It will now be naturally asked, What have we learned from the phenomena described and exhibited, and from the explanations that have been proposed? We find in these phenomena confirmation of the modern views of matter and energy. The facts elicited are in harmony with the theory that matter is not continuous but composed of a prodigious number of minute particles, not in mutual contact. The facts also are in full accordance with the kinetic theory of gases—to which I have already referred—and with the conception of heat as a particular kind of energy, expressing itself as a rapid vibratory motion of the particles of matter. This alone would be a lesson of no small value. In Science, every law, every generalisation, however well established, must constantly be submitted to the ordeal of a comparison with newly-discovered phenomena; and a theory may be pronounced triumphant when it is found to harmonise with and to account for facts which when it was propounded were still unrecognised or unexplained.

But the experiments have shown us more than this: we have been enabled to contemplate matter in a condition hitherto unknown,—in a fourth state,—as far removed from that of gas as gas is from liquid, where the well-known properties of gases and elastic fluids almost disappear, whilst in their stead are revealed attributes previously masked and unsuspected. In this ultra-gaseous state of matter phenomena are perceived which in the mere gaseous condition are as impossible as in liquids or solids.

I admit that between the gaseous and the ultra-gaseous state there can be traced no sharp boundary; the one merges imperceptibly into the other. It is true also that we cannot see or handle matter in this novel phase. Nor can human or any other kind of organic life conceivable to us penetrate into regions where such ultra-gaseous matter may be supposed to exist. Nevertheless, we are able to observe it and experiment on it, legitimately arguing from the seen to the unseen.

Of the practical applications that may arise out of these researches, it would now be premature to speak. It is rarely given to the dis-

coverer of new facts and new laws to witness their immediate utilisation. The ancients showed a perhaps unconscious sagacity when they selected the olive, one of the slowest growing trees, as the symbol of Minerva, the goddess of Arts and Industry. Nevertheless, I hold that all careful honest research will ultimately, even though in an indirect manner, draw after it, as Bacon said, "whole troops of practical applications."

[W. C.]

GENERAL MONTHLY MEETING,

Monday, April 7, 1879.

Sir W. Frederick Pollock, Bart. M.A. Vice-President, in the Chair.

John James Aubertin, Esq.
Mrs. Walter F. Ball,
Henry Bruce Boswell, Esq.
Fitzwilliam Comyn, Esq.
Claudius Shirley Harris, Esq.
Thomas Herbert Sowerby, Esq.

were *elected* Members of the Royal Institution.

The following Arrangements for the Lectures after Easter were announced:—

Ernst Pauer, Esq. — Three Lectures on Schubert, Mendelssohn, and Schumann (*with Musical Illustrations*); on Tuesdays, April 22 to May 6.

Professor Dewar, M.A. F.R.S. — Five Lectures on "Dissociation;" on Thursdays, April 24 to May 29.

H. H. Statham, Esq.—Four Lectures on the Leading Styles of Architecture Historically and Æsthetically Considered; on Saturdays, April 26 to May 17.

Professor Karl Hillebrand.—Six Lectures on the Intellectual Movement of Germany from the Middle of the Last to the Middle of the Present Century; on Tuesday, May 13; Mondays, May 19, 26, June 2; Tuesday, June 10; and Thursday, June 12.

John Robert Seeley, Esq. M.A. Professor of Modern History, Cambridge.—Four Lectures: Suggestions to Students and Readers of History. On Tuesdays, May 20, 27, June 3; and on Thursday, June 5.

Professor Henry Morley. — Three Lectures on Swift. On Saturdays, May 24 to June 7.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

Accademia dei Lincei, Rome—Atti, Serie III. Transunti, Vol. III. Fasc. 3. 4to. 1879.

Agricultural Society of England, Royal—Journal, Second Series, Vol. XV. Part 1. 8vo. 1879.

Astronomical Society, Royal—Monthly Notices, Vol. XXXI. No. 4. 8vo. 1879.
British Architects, Royal Institute of—1878–9: Proceedings, No. 9.
Transactions, No. 8. 4to.
British Association for the Advancement of Science—Report of the Forty-eighth Meeting at Dublin, August, 1878. 8vo. 1879.
Chemical Society—Journal for March, 1879. 8vo.
Civil Engineers' Institution—Minutes of Proceedings, Vol. LV. 8vo. 1879.
Cunningham, D. D. M.B. (the Author)—On Certain Effects of Starvation on Vegetable and Animal Tissues. 4to. 1879.
Editors—American Journal of Science for March, 1879. 8vo.
Analyst for March, 1879. 8vo.
Athenæum for March, 1879. 4to.
Chemical News for March, 1879. 4to.
Engineer for March, 1879. fol.
Horological Journal for March, 1879. 8vo.
Iron for March, 1879. 4to.
Journal for Applied Science for March, 1879. fol.
Monthly Journal of Science, March, 1879. 8vo.
Nature for March, 1879. 4to.
Telegraphic Journal for March, 1879. 8vo.
Franklin Institute—Journal, No. 639. 8vo. 1879.
Geographical Society, Royal—Proceedings, New Series. Vol. I. No. 3. 8vo. 1879.
Lewis, T. R. M.B. (the Author)—The Microscopic Organisms found in the Blood of Man and Animals and their Relation to Disease. 4to. 1879.
Liverpool Literary and Philosophical Society—Proceedings, No. 32. 8vo. 1877–8.
Madras Literary Society—Madras Journal of Literature and Science for 1878. 8vo. 1879.
Meteorological Society—Quarterly Journal, Nos. 28, 29. 8vo. 1878–9.
Photographic Society—Journal, New Series, Vol. III. No. 6. 8vo. 1879.
Physical Society of London—Proceedings, Vol. II. Part 5. 8vo. 1879.
Preussische Akademie der Wissenschaften—Monatsberichte: Dec. 1878. 8vo.
Royal Society of Edinburgh—Transactions, Vol. XXVIII. Part 2. 4to. 1877–8.
Proceedings, Nos. 100, 101, 102. 8vo. 1877–8.
Royal Society of London—Proceedings, No. 193. 1879.
Society of Arts—Journal for March, 1879.
Symons, G. J. Esq.—Monthly Meteorological Magazine, March, 1879. 8vo.
Tuson, Professor R. V. (the Editor)—Cooley's Cyclopædia of Practical Receipts. Part 11. 8vo. 1879.
Vereins zur Beförderung des Gewerbfleisses in Preussen—Verhandelungen, 1879. Heft 3. 4to.
Vincent, Charles W. F.R.S.E. (the Editor)—Dictionary of Chemistry applied to the Arts and Manufactures. Division VII. 4to. 1879.
Wild, Dr. H. (the Director)—Annalen des Physikalischen Central-observatoriums, Russia. Jahrgang, 1877. 4to. 1878.
Repertorium für Meteorologie: Band VI. Heft 1. 4to. St. Petersburg. 1878.

Warren De La Rue, Esq. M.A. D.C.L. F.R.S. Sec. R.I.—Professor Mascart's Insulating Stand on Sir William Thomson's System of Insulating by means of Sulphuric Acid.

WEEKLY EVENING MEETING,

Friday, April 25, 1879.

THE DUKE OF NORTHUMBERLAND, D.C.L. LL.D. the Lord Privy Seal, President, in the Chair.

FRANCIS GALTON, Esq. F.R.S. M.R.I.

*Generic Images.**

IN the pre-scientific stage of every branch of knowledge, the prevalent notions of phenomena are founded upon general impressions; but when that stage is passed and the phenomena are measured and numbered, many of those notions are found to be wrong, even absurdly so. This is the case not only in professional matters, but in those with which everyone has some opportunity of becoming acquainted. Think of the nonsense spoken every day about the signs of coming weather, in connection, for example, with the phases of the moon. Think of the ideas about chance, held by those who are unacquainted with the theory of probabilities; think of the notions on heredity, before the days of Darwin. It is unnecessary to multiply instances; the frequent incorrectness of notions derived from general impressions may be assumed, and the object of the following discourse is to point out a principal cause of it.

Attention will be called to a source of error that is inherent in our minds, that vitiates the truth of all our general impressions, and which we can never wholly eliminate except by separating the confused facts upon which our general impressions are founded, and treating them numerically by the regular methods of statistics. It is not sufficient to learn that an opinion has been long established or held by many, but we must collect a large number of instances to test that opinion, and numerically compare the successes and the failures.

Our general impressions are founded upon blended memories, and these latter will be the chief topic of the present discourse. An analogy will be pointed out between these and the blended portraits

* This memoir is in part an abstract, and in small part an extension of the discourse that was actually delivered. The greater part of the subject matter has been treated more fully in the July number of the 'Nineteenth Century,' but the autotype illustrations which are given here are not inserted there.

first described by myself a year ago under the name of 'Composite Portraits,' * and specimens of the latter will be exhibited. Then the cause will be explained that renders the mind incompetent to blend memories together in their just proportions.

The physiological basis of memory is simple enough in its broad outlines. Whenever any group of brain elements has been excited by a sense impression, it becomes, so to speak, tender, and liable to be easily thrown again into a similar state of excitement. If the new cause of excitement differs from the original one, a memory is the result. Whenever a single cause throws different groups of brain elements simultaneously into excitement, the result must be a blended memory.

We are familiar with the fact that faint memories are very apt to become confused. Thus some picture of mountain and lake in a country which we have never visited, often recalls a vague sense of identity with much we have seen elsewhere. Our recollections cannot be disentangled, though general resemblances are recognized. It is also a fact that the memories of persons who have great powers of visualising, that is, of seeing well-defined images in the mind's eye, are no less capable of being blended together. Artists are, as a class, possessed of the visualising power in a high degree, and they are at the same time pre-eminently distinguished by their gifts of generalization. They are of all men the most capable of producing forms that are not copies of any individual, but represent the characteristic features of classes.

There is then, no doubt, from whatever side the subject of memory is approached, whether from the material or from the mental, and, in the latter case, whether we examine the experiences of those in whom the visualising faculty is faint or in whom it is strong, that the brain has the capacity of blending memories together. Neither can there be any doubt that general impressions are faint and perhaps faulty editions of blended memories. They are subject to errors of their own, and they inherit all those to which the memories are themselves liable.

Specimens of blended portraits will now be exhibited; these might, with more propriety, be named, according to the happy phrase of Professor Huxley, "generic" portraits. The word generic presupposes a genus, that is to say, a collection of individuals who have much in common, and among whom medium characteristics are very much more frequent than extreme ones. The same idea is sometimes expressed by the word typical, which was much used by Quételet, who was the first to give it a rigorous interpretation, and whose idea of a type lies at the basis of his statistical views. No statistician dreams of combining objects into the same generic group that do not cluster towards a common centre, no more can we compose generic

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* 'Journal of the Anthropol. Institute' (Nov. 1878); or, 'Nature' (p. 97. 1878).

portraits out of heterogeneous elements, for if the attempt be made to do so the result is monstrous and meaningless.

It might be expected that when many different portraits are fused into a single one, the result would be a mere smudge. Such, however, is by no means the case, under the conditions just laid down, of a great prevalence of the mediocre characteristics over the extreme ones. There are then so many traits in common, to combine and to reinforce one another, that they prevail to the exclusion of the rest. All that is common remains, all that is individual tends to disappear.

The first of the composites exhibited on this occasion is made by conveying the images of three separate portraits by means of three separate magic lanterns upon the same screen. The stands on which the lanterns are mounted have been arranged to allow of nice adjustment. The composite about to be shown is one that strains the powers of the process somewhat too severely, the portraits combined being those of two brothers and their sister, who have not even been photographed in precisely the same attitudes. Nevertheless, the result is seen to be the production of a face, neither male nor female, but more regular and handsome than any of the component portraits, and in which the common family traits are clearly marked. Ghosts of portions of male and female attire, due to the peculiarities of the separate portraits, are seen about and around the composite, but they are not sufficiently vivid to distract the attention. [This effect is well seen in the composite of Napoleon in the autotype photographic plate here annexed.] If the number of combined portraits had been large, these ghostly accessories would have become too faint to be visible. [See the very faint indication of the various ears in the co-composite of the criminals.]

The next step is to compare this portrait of two brothers and their sister which has been composed by optical means before the eyes of the audience, and concerning the truthfulness of which there can be no doubt, with a photographic composite of the same group. This latter has been made by the process described in the memoir already referred to, and which is analogous to that by which memories are blended. The portraits to be combined are adjusted very carefully one in front of the other, so that the features shall be as exactly superimposed as is possible from the nature of the case. [This is done by making two pin-holes in the bottom of one of them, then placing it on each of the others in succession when held before a strong light, so that the two are seen in transparency, and pricking each through the same pin-holes. These pricks serve as fiducial marks for their subsequent arrangement.] The packet of adjusted portraits is next placed in front of the object-glass of a photographic camera, and the portraits are then removed one by one. Thus the impression left on the sensitised plate is that of a succession of different portraits thrown one on the top of another on the same part of it. The result is a composite portrait. A photographic composite prepared in this way from the portraits of the two brothers and sister is now placed in

a fourth magic lantern with a brighter light behind it, and its image is thrown on the screen by the side of the composite produced by direct optical superposition. It will be observed that the two processes lead to almost exactly the same result, and therefore the fairness of the photographic process may be taken for granted. However, two other comparisons will be made for the sake of verification, namely, between the optical and photographic composites of two children, and again between those of two Roman contadini.

The composite portraits that will next be exhibited are made by the photographic process, and it will now be understood that they are truly composite, notwithstanding their definition and apparent individuality. Attention is, however, first directed to a convenient instrument not more than 18 inches in length, which is, in fact, a photographic camera with six converging lenses and an attached screen, on which six pictures can be adjusted and brilliantly illuminated by artificial light. The effect of their optical combination can thus be easily studied; any errors of adjustment can be rectified and the composite may be photographed at once.

It must not be supposed that any one of the components fails to leave its due trace in the photographic composite, much less in the optical one. In order to allay misgivings on the subject, a small apparatus is laid on the table together with some of the results obtained by it. It is a cardboard frame, with a spring shutter closing an aperture of the size of a wafer, that springs open on the pressure of a finger, and shuts again as suddenly when the pressure is withdrawn. A chronograph is held in the other hand, whose index begins to travel the moment the finger presses a spring, and stops instantly on lifting the finger. The two instruments are worked simultaneously; the chronograph checking the time allowed for each exposure and summing all the times. It appears from several trials that the effect of 1000 brief exposures is practically identical with that of a single exposure of 1000 times the duration of any one of them. Therefore each of a thousand components leaves its due photographic trace on the composite, though it is far too faint to be visible unless reinforced by many similar traces.

The composites now to be exhibited are made from coins or medals, and in most instances the aim has been to obtain the best likeness attainable of historical personages, by combining various portraits of them taken at different periods of their lives and so to elicit the traits that are common to each series. A few of the individual portraits are placed in the same slide with each composite to give a better idea of the character of these blended representatives. Those that are shown are (1) Alexander the Great, from six components; (2) Antiochus, King of Syria, from six; (3) Demetrius Poliorcetes, from six; (4) Cleopatra, from five. Here the composite is as usual better looking than any of the components, none of which however give any indication of her reputed beauty; in fact, her features are not only plain but to an ordinary English taste are simply

hideous. (5) Nero, from eleven; (6) A combination of five different Greek female faces, and (7) A singularly beautiful combination of the faces of six different Roman ladies, forming a charming ideal profile.*

My cordial acknowledgment is due to Mr. R. Stuart Poole, the learned curator of the coins and gems in the British Museum, for his kind selection of the most suitable medals and for procuring casts of them for me for the present purpose. These casts were, with one exception, all photographed to a uniform size of four-tenths of an inch between the pupils of the eyes and the division between the lips, which experience shows to be the most convenient size on the whole to work with, regard being paid to many considerations not worth while to specify in detail. When it was necessary the photograph was reversed. These photographs were made by Mr. H. Reynolds; I then adjusted and prepared them for taking the photographic composite.

The next series to be exhibited consists of composites taken from the portraits of criminals convicted of murder, manslaughter, or crimes accompanied by violence. There is much interest in the fact that two types of features are found much more frequently among these than among the population at large. In one, the features are broad and massive, like those of Henry VIII., but with a much smaller brain. The other, of which five composites are exhibited, each deduced from a number of different individuals, varying four to nine, is a face that is weak and certainly not a common English face. Three of these composites, though taken from entirely different sets of individuals, are as alike as brothers, and it is found on optically combining any three out of the five composites, that is on combining almost any considerable number of the individuals, the result is closely the same. The combination of the three composites just alluded to will now be effected by means of the three converging magic lanterns, and the result may be accepted as generic in respect of this particular type of criminals.

The process of composite portraiture is one of pictorial statistics. It is a familiar fact that the average height of even a dozen men of the same race, taken at hazard, varies so little, that for ordinary statistical purposes it may be considered constant. The same may be said of the measurement of every separate feature and limb, and of every tint, whether of skin, hair, or eyes. Consequently a pictorial combination of any one of these separate traits would lead to results no less constant than the statistical averages. In a portrait, there is another factor to be considered besides the measurement of the separate traits, namely, their relative position; but this, too, in a sufficiently large group, would necessarily have a statistical constancy. As

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* The accompanying illustrations have been *photographically* transferred (on a reduced scale) to stone, and lithographed by the Autotype Company, 36, Rathbone Street. They are very successfully done, and are nearly equal in clearness to the originals. The composite of the Roman ladies comes out unfortunately a little too dark, and some of the beauty of the original is thereby lost.

a matter of observation, the resemblance between persons of the same "genus" (in the sense of "generic," as already explained) is sufficiently great to admit of making good pictorial composites out of even small groups, as has been abundantly shown.

Composite pictures are, however, much more than averages; they are rather the equivalents of those large statistical tables whose totals, divided by the number of cases, and entered in the bottom line, are the averages. They are real generalizations, because they include the whole of the material under consideration. The blur of their outlines, which is never great in truly generic composites, except in unimportant details, measures the tendency of individuals to deviate from the central type. My argument is, that the generic images that arise before the mind's eye, and the general impressions which are faint and faulty editions of them, are the analogues of these composite pictures which we have the advantage of examining at leisure, and whose peculiarities and character we can investigate, and from which we may draw conclusions that shall throw much light on the nature of certain mental processes which are too mobile and evanescent to be directly dealt with.

A generic mental image may be considered to be nothing more than a generic portrait stamped on the brain by the successive impressions made by its component images. Professor Huxley, from whom, as already mentioned, the apt phrase of "generic" has been borrowed, has expressed himself to a similar effect in his recent life of Hume (p. 95). I am rejoiced to find that, from a strictly physiological side, this explanation is considered to be the true one by so high an authority, and that he has, quite independently of myself, adopted a view which I also entertained, and had hinted at in my first description of composite portraiture, though there was not occasion at that time to write more explicitly about it.

In my original memoir on composite portraits a phrase was used which was written with some hesitation, and which I have since quoted, but which it will now be the object to examine and amend. The words were: "A composite portrait represents the picture that would rise before the mind's eye of an individual who had the gift of pictorial imagination in an exalted degree." The question to be considered is whether this is a strictly correct statement. If the eye of such a man were placed in the position of the object-glass of a camera when taking the composite portraits, and if we suppose him free from mental bias, would the resulting picture in his brain be identical with the composite? (Here again we are supposed to ignore such small differences as may exist between the photographic and optical composite.) The answer is distinctly, No. Suppose that one of the portraits has been exposed for a period fifty times as long as any of the rest, in the photographic composite the effect would be the same as that of fifty coats of transparent pigment, but in the mental composite it would have nothing like that importance; and therein lies the source of error in our mental impressions that it is the object of

this discourse to point out. Exceptional occurrences leave an impression on the brain of far greater strength, and conversely habitual occurrences leave one of far less strength, than their numbers warrant. The physiological effect of prolonged action, or of reiteration, is by no means in direct proportion to the length of the one or to the frequency of the other. The magnitude of the "subjective" effect never bears a simple, direct proportion to the magnitude of the "objective" cause. The relation between them, in a very wide circle of physiological phenomena, is expressed by the law of Weber or Fechner, which it is sufficient for our present purposes to state in its original form, because it is exceedingly simple, and is at the same time sufficiently correct for all except extreme cases, in which certain alien considerations begin to exert a sensible influence. According to this law (sensation = log. stimulus) the more the senses are stimulated, the more is their discriminative power blunted. If a room is lighted by only a single candle, and a second one is brought in, the eye feels a certain increase of light. Now, if 1000 candles had originally been in the room, it would require the addition, not of one candle, but of another 1000 candles, to produce the sense of a similar increase. In order that the magnitude of any sensation should increase by a series of equal steps, the magnitude of the stimulus that causes it must increase by successive multiples. The one follows an arithmetic progression, the other a geometric one.

A few simple experiments will illustrate this. Five perfectly black cards are taken, each of the size of half a sheet of note paper; also a sheet of perfectly white note-paper. The latter is torn in two, and one half is laid upon card No. 5, which it exactly covers. The remaining half is carefully folded down its middle, and torn in two, and one portion is laid on card No. 4, of which it exactly covers one half. The same process is continued, so that card 3 is covered to the extent of one quarter of its surface, 4 to one-eighth, and 5 to one-sixteenth, and there is a remnant of one-sixteenth, which may be thrown away. To avoid fractions, let us count the quantity of white on the black card No. 1 as one, then that on Nos. 2, 3, 4, 5 will be as two, four, eight, and sixteen respectively, the latter standing for pure white. The next step is to cut the portions of paper into shreds, and to scatter them uniformly over their respective cards. In the specimens now upon the table this has been already done, and the shreds are pasted down. The effect, when they are looked at from a little distance with the eye not focussed too sharply upon them, is that of a series of greys, which appear to be separated by equal intervals of tint from one another, although we know that the differences in the amount of white material is by no means uniform. The eye judges card No. 3, which contains four portions of white, to be of a medium tint between Nos. 1 and 5; but, as No. 1 contains one portion, and No. 5 contains sixteen portions, the medium quantity of white is really eight and a half (because $\frac{1+16}{2} = 8\frac{1}{2}$), and this

is somewhat lighter even than card No. 4, which contains eight portions.

The same relation is true as regards sound. The difference of noise made by the fall of one shilling or of two shillings is not readily perceived, unless we are specially attending to it. Neither is the difference readily perceived between firing a 38-ton gun or two such guns from the turret of an ironclad, as was proved by the evidence in the late terrible accident on board the 'Thunderer.' Here is an apparatus of eight arms that may be lifted in succession and then let drop by turning a cylinder like that of a musical snuff-box. Each arm as it falls makes the same amount of noise. The catches are so arranged on the cylinder that the effect of turning it is to lift and let drop first one arm, then two arms simultaneously, then four, then all the eight. It will be observed that the apparent loudness of sound increases by equal intervals, and not at all as the numbers 1, 2, 4, 8.

Finally, two large revolving discs are exhibited under illumination. They are painted black and white in five concentric rings, with a perfectly black centre. In the first of the two discs, counting pure white as 5, the proportions of white to black in the successive rings are as 1, 2, 3, 4, 5, thus forming an arithmetical series. On turning the wheel, the eye utterly repudiates the effect as being that of a series of equally gradated tints, and yet the actual quantities of white form such a series. In the second of the two discs, the proportions of white to black in the successive rings follow Weber's law, or rather, Delbœuf's modification of it; the disc is, indeed, a reproduction of that described in Delbœuf's memoir. On revolving it, the eye at once recognizes the effect of a beautifully exact gradation; but in order to show this properly, the illumination has to be very carefully adjusted.

These illustrations of Weber's law are submitted in order to make manifest the great difference between the progressive increase of objective causes and that of their corresponding subjective effects, and to afford a *primâ facie* evidence of the small influence likely to be exercised upon a generic mental image by a repetition of similar impressions. I do not venture as yet to assert that the law of Weber applies to this case, but the probability of its doing so is pointed out, and also the fact that the true law, whatever it may be, is certainly in some sense analogous to that of Weber. According to that law, if it required a tenfold experience or a tenfold period of exposure to produce a mental impression that should contribute to the composition of a blended image in twice as large a degree as a single experience or a single period of exposure, it would require a hundredfold experience or exposure to result in a threefold contribution.

The law of Weber has a further application to the topics under consideration. When the comparison was made a short time back between the blended image in the artist's brain and the photographic composite, it was stated that a fiftyfold period of exposure would produce in the latter case a fiftyfold effect, in the sense of being

6 LIKENESSES OF ALEXANDER THE GREAT,
AND THE COMPOSITE OF THEM IN THE CENTRE.

5 LIKENESSES OF NAPOLEON I TAKEN AT DIFFERENT PERIODS.
AND THE COMPOSITE OF THEM IN THE CENTRE.

LIKENESSES OF SIX DIFFERENT ROMAN LADIES
AND THE COMPOSITE OF THEM IN THE CENTRE.

3 COMPOSITES OF CRIMINALS TAKEN FROM
4, 9 & 5 DIFFERENT PERSONS RESPECTIVELY. AND THE
CO-COMPOSITE OF THE WHOLE 18 OF THEM IN THE CENTRE.

AUTOTYPE LONDON

equivalent to fifty layers of transparent colour. It was not intended to imply by this that the tint *as estimated by the eye* would be fifty times increased in depth. The law of Weber tells us that it would not be anything like so deep as that in appearance. Objectively speaking the tints of a photographic composite are correct, but subjectively speaking they are not. Hence there are three degrees of accuracy, respectively corresponding to the three processes of (1) numerical averages, (2) of optical or photographic composites, and (3) of mental images. Numerical averages are absolutely correct in every sense. Optical and photographic composites are objectively correct, but subjectively incorrect. Mental images are objectively incorrect, and they are subjectively incorrect in a double degree. Supposing Weber's law to be applicable throughout, a white mark in any one of the portraits would leave a mark on the optical or photographic composite whose *apparent* intensity would vary as the logarithm of the time of photographic exposure, but the intensity of the white mark that it would leave on the mental composite would be only as the logarithm of that logarithm.

Even this result is much too leniently calculated. It is based on the supposition that the visualising power is perfect, the memory absolutely retentive, and the attention perfectly free from bias. This is very far from being the case. Again, some of the images in every presumed generic group are sure to be aliens to the genus and to have become associated to the rest by superficial and fallacious resemblances, such as common minds are especially attentive to. Seeing, as we easily may, what monstrous composites result from ill-sorted combinations of portraits, and how much nicety of adjustment is required to produce the truest possible generic image, we cannot wonder at the absurd and frequent fallacies in our mental conceptions and general impressions.

Our mental generic composites are rarely defined ; they have that blur in excess which photographic composites have in a small degree, and their background is crowded with faint and incongruous imagery. The exceptional effects are not overmastered, as they are in the photographic composites, by the large bulk of ordinary effects. Hence, in our general impressions far too great weight is attached to what is strange and marvellous, and experience shows that the minds of children, savages, and uneducated persons have always had that tendency. Experience warns us against it, and the scientific man takes care to base his conclusions upon actual numbers.

The human mind is therefore a most imperfect apparatus for the elaboration of general ideas. Compared with those of brutes its powers are marvellous, but for all that they fall vastly short of perfection. The criterion of a perfect mind would lie in its capacity of always creating images of a truly generic kind, deduced from the whole range of its past experiences.

General impressions are never to be trusted. Unfortunately when they are of long standing they become fixed rules of life, and assume

a prescriptive right not to be questioned. Consequently, those who are not accustomed to original inquiry entertain a hatred and a horror of statistics. They cannot endure the idea of submitting their sacred impressions to cold-blooded verification. But it is the triumph of scientific men to rise superior to such superstitions, to devise tests by which the value of beliefs may be ascertained, and to feel sufficiently masters of themselves to discard contemptuously whatever may be found untrue.

[F. G.]

ANNUAL MEETING,

Thursday, May 1, 1879.

The Duke of Northumberland, D.C.L. LL.D. President, in the Chair.

The Annual Report of the Committee of Visitors for the year 1878, testifying to the continued prosperity and efficient management of the Institution, was read and adopted. The Real and Funded Property now amounts to nearly 85,000*l.*, entirely derived from the Contributions and Donations of the Members.

Sixty new Members paid their Admission Fees in 1878.

Sixty-four Lectures and Nineteen Friday Evening Discourses were delivered in 1878.

The Books and Pamphlets presented in 1878 amounted to about 344 volumes, making, with 281 volumes purchased by the Managers, a total of 562 volumes added to the Library in the year, exclusive of periodicals.

Thanks were voted to the President, Treasurer, and Secretary, to the Committees of Managers and Visitors, and to the Professors, for their services to the Institution during the past year.

The most cordial thanks of the Members were given to His Grace the President for his munificent gift of an Otto's Silent Gas-engine and a De Meritens Magnet-Electric Machine, the principles of which were explained and illustrated by Professor Tyndall.

The following Gentlemen were unanimously elected as Officers for the ensuing year:

President—The Duke of Northumberland, D.C.L. LL.D. the Lord Privy Seal.
Treasurer—George Busk, Esq. F.R.C.S. F.R.S.
Secretary—Warren De La Rue, Esq. M.A. D.C.L. F.R.S.

Managers.

Thomas Boycott, M.D. F.L.S.
Frederick Joseph Bramwell, Esq. F.R.S.
Joseph Brown, Esq. Q.C.
Edward Frankland, Esq. D.C.L. Ph.D. F.R.S. &c.
Sir Joseph D. Hooker, K.C.S.I. C.B. D.C.L. F.R.S. &c.
William Huggins, Esq. D.C.L. F.R.S.
William Watkiss Lloyd, Esq.
Sir W. Frederick Pollock, Bart. M.A.
John Rae, M.D. LL.D.
The Lord Rayleigh, M.A. F.R.S.
Robert P. Roupell, Esq. M.A. Q.C.
The Lord Arthur Russell, M.P.
C. William Siemens, Esq. D.C.L. F.R.S.
James Spedding, Esq.
William Spottiswoode, Esq. M.A. D.C.L. Pres. R.S.

Visitors.

Charles Brooke, Esq. M.A. F.R.S.
Warren William De La Rue, Esq.
William Henry Domville, Esq.
James N. Douglass, Esq.
Edward Enfield, Esq.
Right Hon. The Lord Claud Hamilton.
Francis Hird, Esq. F.G.S.
George Cargill Leighton, Esq.
John Fletcher Moulton, Esq.
Henry Pollock, Esq.
William Henry Preece, Esq. M.I.C.E.
Lachlan Mackintosh Rate, Esq.
Basil Woodd Smith, Esq. F.R.A.S.
George Andrew Spottiswoode, Esq.
Edward Woods, Esq.

WEEKLY EVENING MEETING,

Friday, May 2, 1879.

William Spottiswoode, Esq. M.A. D.C.L. LL.D. Pres. R.S. Vice-President, in the Chair.

Professor John G. McKendrick, M.D.

The Physiological Action of Anæsthetics.

[Abstract deferred.]

GENERAL MONTHLY MEETING,

Monday, May 5, 1879.

George Busk, Esq. F.R.S. Treasurer and Vice-President, in the Chair.

The following Vice-Presidents for the ensuing year were announced:

Sir W. Frederick Pollock, Bart. M.A.
C. William Siemens, Esq. D.C.L. F.R.S.
William Spottiswoode, Esq. D.C.L. LL.D. Pres. R.S.
George Busk, Esq. F.R.S. Treasurer,
Warren De La Rue, Esq. M.A. D.C.L. F.R.S. Secretary.

John Tyndall, Esq. D.C.L. LL.D. F.R.S. was re-elected Professor of Natural Philosophy.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Lords of the Admiralty—
Greenwich Observations for 1876. 4to. 1878.
Meteorological Observations, 1847–73. 4to. 1878.
Nine-year Catalogue of 2263 Stars for 1872. 4to. 1876.
Cape Astronomical Observations, 1859 and 1875. 2 vols. 8vo. 1874–77.
Accademia dei Lincei, Reale, Roma—Atti, Serie Terza, Transunti, Vol. III. Fasc. 4. 4to. 1879.
Actuaries, Institute of—Journal, No. 114. 8vo. 1879.
Antiquaries, Society of—Proceedings, Vol. VII. No. 5. 8vo. 1879.
Asiatic Society, Royal—Journal, New Series, Vol. XI. No. 2. 8vo. 1879.
Astronomical Society, Royal—Monthly Notices, Vol. XXXI. No. 5. 8vo. 1879.
British Architects, Royal Institute of—1878–9: Proceedings, Nos. 9, 10, 11. 4to. Transactions, No. 9. 4to.
British Museum Trustees—Catalogue of Birds. Vol. IV. 8vo. 1979.
Index of Minerals. 8vo. 1878.
Guide to first Vase-Rooms. 8vo. 1879.
Chemical Society—Journal for April, 1879. 8vo.
Editors—American Journal of Science for April, 1879. 8vo.
Analyst for April, 1879. 8vo.
Athenæum for April, 1879. 4to.
Chemical News for April, 1879. 4to.
Engineer for April, 1879. fol.
Horological Journal for April, 1879. 8vo.
Iron for April, 1879. 4to.
Journal for Applied Science for April, 1879. fol.
Monthly Journal of Science, April, 1879.
Nature for April, 1879. 4to.
Telegraphic Journal for April, 1879. 8vo.
Franklin Institute—Journal, No. 640. 8vo. 1879.
Geographical Society, Royal—Proceedings, New Series. Vol. I. No. 4. 8vo. 1879.
India Geological Survey—Records, Vol. XII. Part 1. 8vo. 1879.

Jenkins, Rev. Canon R. C. M.A. M.R.I. (the Author)—On some Popular Errors in regard to the Second General Council and its Additions to the Nicene Confession. (K 103) 8vo. 1879.

Linnean Society—Journal, No. 78. 8vo. 1879.

London Hospital—Reports for 1877. 8vo. 1878.

Mechanical Engineers' Institution—Proceedings, 1879. No. 1. 8vo.

Medical and Chirurgical Society—Proceedings, Part 48. 8vo. 1879.

Metorological Office—Report of Meeting of Meteorological Congress at Utrecht, October, 1878. 8vo. 1879.

Photographic Society—Journal, New Series, Vol. III. No. 7. 8vo. 1879.

Putnam, G. H. Esq. (the Author)—International Copyright considered (K 103) 8vo. New York, 1879.

Royal Society of London—Proceedings, No. 194. 1879.

Scottish Society of Arts, Royal—Transactions, Vol. IX. Part 5.; Vol. X. Part 1. 8vo. 1878.

Society of Arts—Journal for April, 1879. 8vo.

Statistical Society—Journal, Vol. XLII. Part 1. 8vo. 1879.

Symons, G. J.—Monthly Meteorological Magazine, April, 1879. 8vo.

Telegraph Engineers, Society of—Journal, Nos. 24, 25. 8vo. 1879.

Tuson, Professor R. V. (the Editor)—Cooley's Cyclopædia of Practical Receipts. Part 12. 8vo. 1879.

United States Naval Observatory—Astronomical and Meteorological Observations for 1875. 4to. Washington, 1878.

University of London—Calendar for 1879. 12mo.

Zoological Society of London—Transactions, Vol. X. Parts 10, 11. 4to. 1879. Proceedings, 1878, Part 4. 8vo.

WEEKLY EVENING MEETING,

Friday, May 9, 1879.

H.R.H. Prince Frederick Christian of Schleswig Holstein, Hon. M.R.I. in the Chair.

Sir John Lubbock, Bart. M.P. V.P.R.S. D.C.L. LL.D. *M.R.I.*

The Habits of Ants.

Mr. Grote, in his 'Fragments on Ethical Subjects,' regards it as an evident necessity that no society can exist without the sentiment of morality. "Everyone," he says, "who has either spoken or written on the subject, has agreed in considering this sentiment as absolutely indispensable to the very existence of society. Without the diffusion of a certain measure of this feeling throughout all the members of the social union, the caprices, the desires, and the passions of each separate individual would render the maintenance of any established communion impossible. Positive morality, under some form or other, has existed in every society of which the world has ever had experience."

If this be so, the question naturally arises whether ants also are moral and accountable beings. They have their desires, their passions, even their caprices. The young are absolutely helpless. Their communities are sometimes so numerous, that perhaps London and Pekin are almost the only human cities which can compare with them. Moreover their nests are no mere collections of independent individuals, nor even temporary associations like the flocks of migratory birds; but organized communities labouring with the utmost harmony for the common good. The remarkable analogies which they present in so many ways to our human societies render them peculiarly interesting to us, and one cannot but long to know more of their character, how the world appears to them, and to what extent they are conscious and reasonable beings.

For my own part I cannot make use of Mr. Grote's argument, because I have elsewhere attempted to show that, even as regards man, the case is not by any means clear. But, however, this may be, various observers have recorded in the case of ants instances of attachment and affection.

In various memoirs, published by the Linnean Society, I have discussed these instances, having reluctantly come to the conclusion that some of them at any rate rest on a very doubtful foundation.

Yet I am far from denying that such cases do exist. For instance, in one of my nests of *Formica fusca* was a poor ant, which had come into the world without antennæ. Not having previously met with such an individual, I watched her with great interest, but she never appeared to leave the nest. At length one day I found her wandering about outside in an aimless sort of manner, and apparently not knowing her way at all. After a while she fell in with some specimens of *Lasius flavus*, who directly attacked her. I at once set myself to separate them, but whether owing to the wounds she had received from her enemies, or my rough, though well-meant, handling, or both, she was evidently sorely wounded, and lay helplessly on the ground. After some time, another *F. fusca* from her nest came by. She examined the poor sufferer carefully, then picked her up tenderly and carried her away into the nest. It would have been difficult for anyone who witnessed this scene to have denied to this ant the possession of humane feelings.

Indeed, I have often been surprised that in certain emergencies, ants render one another so little assistance. The tenacity with which they retain their hold on an enemy they have once seized, is well known. M. Mocquerys even assures us that the Indians of Brazil made use of this quality in the case of wounds, causing an ant to bite the two lips of the cut, and this bring them close, after which the ant's head is cut off, and thus holds the lips of the wound together. He asserts that he has often seen natives, with wounds in course of healing, by the assistance of a row of seven or eight ants' heads! Now I have often observed that some of my ants had the heads of others hanging on to their legs for a considerable time, and as this must certainly be very inconvenient, it seems remarkable that their friends should not relieve them of such an awkward encumbrance.

As mentioned in the previous lecture, one of my queen ants (*Formica fusca*) had a large mite on the under side of her head. She could not remove it, and not one of her companions, for more than three months, performed this kind office for her. Being a queen, she never left the nest, and I therefore had no opportunity of helping her. Since then I have met with several similar cases. Moreover, I have often put ants, which had become smeared with a sticky substance, on the boards close to my nests, and it was the exception when their companions took any notice of or sought to disentangle them; though sooner or later this was generally done.

I have on a previous occasion endeavoured to give you a general account of the habits of ants, and I will therefore now confine myself to certain points.

Before going further, I think it will perhaps interest you to see two or three species of ants, and with the kind help of Professor Tyndall, Mr. Cottrell, and the electric lamp, I shall hope to be able to show you some on the screen. At the same time I must ask your indulgence, if they do not behave as could be wished, for it is a nervous matter to appear before a Royal Institution audience.

The first to which I will call your attention are the common little meadow ants—*Lasius flavus.* I shall hope to show you the young or larvæ, and the queen; at this time of year there are no pupæ or males. This queen was killed by an accident, but the ants appear to have not yet realized their loss. When I prepared this nest for them, and they took possession of it, it was touching to see them carry in the poor dead queen. The queens and males have wings, but after one flight they themselves strip them off.

The next species is one which I shall have occasion to mention very often—*Formica fusca.* Here also you will see that the queen has stripped off her wings.

In these species, though the workers differ more or less in size, they are all alike in other respects; but in the next, which belongs to the genus Pheidole, there are, as you will see, two distinct kinds of workers. These two sorts, different as they are, are of the same age and sex, all being females, and moreover sisters, daughters of the same mother. The large-headed individuals are generally called soldiers; but I am not myself satisfied as to their functions. This differentiation of certain individuals is a very remarkable phenomenon.

I now come to the slave-making ants. The mistresses in this species are entirely dependent on their slaves, and have even lost the instinct of feeding. If placed by themselves, they starve even in the midst of plenty; but, as I mentioned in my previous lecture, I kept a couple alive and well for months by giving them a slave for an hour every morning to clean and feed them.

None of our northern ants store up grain, and hence there has been much discussion as to the well-known passage of Solomon. It is, however, now a well-established fact that more than one species of South European ants do collect seeds of various kinds. I have kept several, and have now a nest of one of these species under observation. The quantity of grain thus stored up is sometimes so considerable that in the Mischna rules are laid down with reference to it; and various commentators, including the celebrated Maimonides, have discussed at length the question whether such grain belonged to the owner of the land, or might be taken by gleaners—giving the latter the benefit of the doubt: they do not appear to have considered the rights of the ants.

A Texan ant, *Pogonomymex barbatus*, is also a harvesting species, storing up especially the grains of *Aristida oligantha*, the so-called "ant rice," and of a grass, *Buchlœ dactyloides.* These ants clear disks, 10 or 12 feet in diameter, round the entrance to their nest, a work of no small labour in the rich soil, and under the hot sun, of Texas. I say "clear disks," but some, though not all, of these disks are occupied, especially round the edge, by a growth of ant rice. Dr. Lincecum, who first gave an account of these insects, maintained not only that the ground was carefully cleared of all other species of plants, but that this grass was intentionally cultivated by the ants. Mr. McCook, by whom the subject has been recently studied, fully confirms Dr.

Lincecum that the disks are kept carefully clean, that the ant rice alone is permitted to grow on them, and that the produce of this crop is carefully harvested; but he thinks that the ant rice sows itself, and is not actually cultivated by the ants. I have myself observed in Algeria, that some species of plants are allowed by the ants to grow on their nests.

But though our English ants do not actually store up provisions, they show a considerable amount of prudence and foresight in other ways. If you examine almost any ants' nest, you will find, besides the ants, a larger or smaller number of other insects; the quantity varying according to the season, the species of ant, and other circumstances. Of course association in some cases is purely accidental and without significance. In a large number of instances, however, this is by no means the case, and Mr. André gives a list of no less than 584 species of insects which are habitually found in association with ants.

In some of them no doubt the bond of union is merely the selection of similar places of abode; in some few others the ants are victimized by parasites of which they cannot rid themselves. There is for instance a small black fly, belonging to the genus Phora, which lays her eggs on ants. Again I have already mentioned the mites which live parasitically on ants. Then there are some insects, such as the caterpillar of that beautiful beetle, the rosechafer, which find a congenial place of residence among the collection of bits of stick, &c., of which certain species of ants make their nests.

Another class of ant guests are those which reside actually in the galleries and chambers of, and with, the ants, but which the latter never touch. Of these the commonest in England are a species allied to Podura, for which I have proposed the name Beckia. It is an active bustling little being, and I have kept hundreds, I may say thousands, in my nests. They run about in and out among the ants, keeping their antennæ in a perpetual state of vibration. Another very common species is a sort of white woodlouse, which enjoys the rather long name of *Platyarthrus Hoffmanseggii.* It also runs about, and is evidently at home, among the ants. Both of these species from living constantly in the dark have become blind; I say "have become," because their ancestors no doubt had eyes. Now in neither of these cases have I ever seen an ant take the slightest notice of either of these insects. One might almost imagine they had the cap of invisibility.

It is, however, certain that the ants intentionally (if I may so say) sanction the residence of these insects in their nests. An unauthorized interloper would be at once killed. I have therefore ventured to suggest that these insects may perhaps act as scavengers.

A still more interesting case is afforded by the relations existing between the ants and aphides, which have been justly called the cows of the ants. The latter secrete a sweet juice of which the ants are very fond, and the ants may constantly be seen running up trees and plants, on which the aphides live, in search of this nutritious food.

The ant may be seen to tap the aphis gently with its antenna, when the latter squeezes out a drop of sweet syrup, which the ant greedily sucks up. Sometimes the ants carry earth up low plants, and build a sort of shed over the aphides, which they also defend from the attacks of other insects. The Baltic amber contains among the remains of many other insects a species of ant intermediate between our small brown garden ants, and the little yellow meadow ants, and which is possibly the stock from which these and other allied species are descended. One is even tempted to suggest that the brown species which live so much in the open air, and climb up trees and bushes, have retained and even deepened their dark colour; while others, such as *Lasius flavus*, the yellow meadow ant, which lives almost entirely below ground, has become much paler. But though this *Lasius flavus* has thus ceased to visit the aphides above ground, it has perhaps acquired its subterranean habit from having discovered certain species of underground aphides, which live on the roots of grass, and which the ants collect into their nests, where these aphides abound, while they are comparatively rare in the surrounding soil. These subterranean aphides, like those which live above ground, secrete a sweet juice which forms no small part of the sustenance of the ants. There are several species of them in my neighbourhood; five are very common. The ants take great care of them, and if the nest is disturbed hasten to carry them away to a place of safety.

Various other species of insects are utilized by ants very much in the same manner as aphides, namely, some species of coccus allied to the cochineal insect, and the "scale" of our greenhouses, and a number of beetles, some of which, like the Beckia, are perfectly blind.

Perhaps the most remarkable case of all still remains to be mentioned. Insects of this group are known to be utilized by ants in tropical countries; but no such relation was known to exist between our English species. I found, however, in nests of *L. flavus* some black eggs which were watched over by the ants with great care, and which eventually turned out to be those of an aphis. Now these eggs are laid in the autumn; they do not hatch till the end of February, or even till May, and yet during the whole of that time they are tended by the ants. This is perhaps, taking all in all, the most striking case of prudence recorded in the animal kingdom.

I would by no means intend to imply that the relations between ants and the other insects which live with them are exhausted by the above suggestions. On the contrary, various other reasons may be imagined which may render the presence of these insects useful, or agreeable, to the ants. For instance, they may emit an odour which is pleasant to the ants. Again, Mr. Francis Galton has, I think, rendered it very probable that most of our domestic animals were kept as pets before they were made of any use. Unlikely as this may appear in some cases, for instance in the pig, we know as a fact that pigs are often kept by savages as pets. I would not put it forward as a suggestion which can be supported by any solid reasoning, but it

seems not altogether impossible that some of these tame insects may be kept as pets.

I have already mentioned that while *Lasius niger*, the brown garden ant, habitually makes use of the out-of-doors aphides, the yellow meadow ant keeps the underground kind; but within the limits of the same species there appear to be considerable differences. M. Lespés even considered that some communities of *L. niger* were more advanced in civilization than others of the same species. He assures us that if he took specimens of their domestic beetles from one nest and placed them in another, always be it understood of the same species, the beetles were attacked and eaten. I have not had the opportunity of repeating these experiments, but I have moved specimens of the blind woodlouse, Platyarthrus, from one nest to another, and even from nests of one species to those of another, and they were always amicably received. But whether there are differences in advancement within the limits of the same species or not, there are certainly considerable differences between the different species, and one may almost fancy that we can trace stages corresponding to the principal steps in the history of human development.

I do not now refer to slave-making ants, which represent an abnormal, or perhaps only a temporary state of things, for slavery seems to tend in ants as in men to the degradation of those by whom it is adopted, and it is not impossible that the slave-making species will eventually find themselves unable to compete with those which are more self-dependent, and have reached a higher phase of civilization. But putting these slave-making ants on one side, we find in the different species of ants different conditions of life, curiously answering to the earlier stages of human progress. For instance, some species, such as *Formica fusca*, live principally on the produce of the chase; for though they feed partly on the honey-dew of aphides, they have not domesticated their insects. These ants probably retain the habits once common to all ants. They resemble the lower races of men, who subsist mainly by hunting. Like them they frequent woods and wilds, live in comparatively small communities, and the instincts of collective action are but little developed among them. They hunt singly, and their battles are single combats, like those of Homeric heroes. Such species as *Lasius flavus* represent a distinctly higher type of social life; they show more skill in architecture, may literally be said to have domesticated certain species of aphides, and may be compared to the pastoral stage of human progress—to the races which live on the produce of their flocks and herds. Their communities are more numerous; they act much more in concert; their battles are not mere single combats, but they know how to act in combination. I am disposed to hazard the conjecture that they will gradually exterminate the mere hunting species, just as savages disappear before more advanced races. Lastly, the agricultural nations may be compared with the harvesting ants.

Thus there seem to be three principal types, offering a curious

analogy to the three great phases—the hunting, pastoral, and agricultural stages—in the history of human development.

When I first began keeping ants, I surrounded the nests by moats of water. This acted well, but the water required continually renewing, especially, of course, in summer, just when the ants were most active. At length, however, in considering the habits of ants and their relation to flowers, another plan suggested itself to me. The hairs by which plants are clothed are of various forms, and fulfil various functions; one is, I believe, to prevent ants and other creeping insects from climbing up the plants so as to obtain access to the flowers, and thus rob them of their honey; for though ants are in some respects very useful to plants, they are not wanted in the flowers. The great object of the beauty, scent, and honey of flowers, is to secure cross fertilization; but for this purpose winged insects are almost necessary, because they fly readily from one plant to another, and generally confine themselves for a certain time to the same species. Creeping insects, on the other hand, naturally would pass from one flower to another on the same plant; and as Mr. Darwin has shown, it is desirable that the pollen should be brought from a different plant altogether. Moreover, when ants quit a plant, they naturally creep up another close by, without any regard to species. Hence, even to small flowers, such as many cruciferæ, compositæ, saxifrages, &c., which, as far as size is concerned, might well be fertilized by ants, the visits of flying insects are much more advantageous. Moreover, if larger flowers were visited by ants, not only would they deprive the flowers of their honey without fulfilling any useful function in return, but they would probably prevent the really useful visits of bees. If you touch an ant with a needle or a bristle, she is almost sure to seize it in her jaws; and if bees, when visiting any particular plant, were liable to have the delicate tip of their proboscis seized on by the honey jaws of an ant, we may be sure that such a species of plant would soon cease to be visited. On the other hand, we know how fond ants are of honey, and how zealously and unremittingly they search for food. How is it then that they do not anticipate the bees, and secure the honey for themselves? This is guarded against in several ways. Some plants are covered with glandular hairs, which make them so sticky that ants do not attempt to walk up them. Some are said to be so slippery that ants cannot do so. Some flowers are closed so that ants cannot get into them. But the commonest protection, perhaps, of all is provided, as I have mentioned, by a clothing of downward-pointing hairs, making a sort of *cheveux de frise* which effectually stops the ants. It occurred to me, therefore, that instead of water I might use fur, arranged so that the hairs pointed downwards. This I have found to answer perfectly, and I mention it specially because the same arrangement may perhaps be found practically useful in hot climates.

When I last had the honour of addressing you, I mentioned various experiments which proved that ants remembered their friends

for more than a year. Having separated a nest of *F. fusca* into two halves, I put from time to time one of the ants from one half into the other, and in every case she was amicably received; while strangers from another nest, although of the same species, were invariably attacked.

It is clear, then, that the ants recognize all their fellows in the same nest, but it is very difficult to understand how this can be effected. The nests vary very much in size, but in many species 100,000 individuals is by no means an unusual number, and in some instances, even this is largely exceeded. Now, it seems almost incredible that in such cases every ant should know every other one by sight.

It has been suggested, in the case of bees, that each nest might have some sign or password.

The whole subject is full of difficulty. It occurred to me, however, that experiments with pupæ might throw some light upon it. Although the ants of every nest, say of *Formica fusca*, are deadly enemies, still, if larvæ or pupæ from one nest are transferred to another, they are kindly received, and tended with, apparently, as much care as if they really belonged to the nest. In ant warfare, though sex is no protection, the young are spared—at least, when they belong to the same species. Moreover, though the habits and disposition of ants are greatly changed if they are taken away from their nest and kept in solitary confinement, or only with a few friends, still, under such circumstances, they will carefully tend any young which may be confided to them. Now, if the recognition were effected by means of some signal or password, then, as it can hardly be supposed that the larvæ or pupæ would be sufficiently intelligent to appreciate, still less to remember it, the pupæ which were entrusted to ants from another nest, would have the password, if any, of that nest, and not of the one from which they had been taken. Hence, if the recognition were effected by some password or sign with the antennæ, they would be amicably received in the nest from which their nurses had been taken, not in their own.

I took, therefore, a number of pupæ out of some of the nests of *Formica fusca* and *Lasius niger*, and put them in small glasses, some with ants from their own nest, some with ants from another nest of the same species.

The result of my observations was that thirty-two ants belonging to *Formica fusca* and *Lasius niger*, removed from their nest as pupæ, attended by friends, and restored to their own nest, were all amicably received. What is still more remarkable, of twenty-two ants belonging to *Formica fusca*, removed as pupæ, attended by strangers, and returned to their own nest, twenty were amicably received, though, in several cases, after some hesitation.

Of the same number of *Lasius niger*, developed in the same manner, from pupæ tended by strangers belonging to the same species, and then returned into their own nest, seventeen were amicably received, three were attacked, and about two I felt doubtful. On the

other hand, fifteen specimens belonging to the same species, removed as pupæ, tended by nurses belonging to the same species, and then put into the nest of these nurses, were all attacked. The results may be tabulated as follows:—

	Pupæ brought up by friends, and replaced in their own nest.	Pupæ brought up by strangers.	
		Put in own nest.	Put in strangers' nest.
Attacked	0	7*	15
Received amicably ..	33	37	0

* About three of these I did not feel sure.

I hope to make further experiments in this direction, but the above results seem very interesting. They appear to indicate that ants of the same nest do not recognize one another by any password. On the other hand, if ants are removed from a nest in the pupæ state, tended by strangers, and then restored, some at least of their relatives are certainly puzzled, and in many cases doubt their claim to consanguinity. Strangers, under the same circumstances, would be immediately attacked: these ants, on the contrary, were in every case,—sometimes, however, only after examination,—amicably received by the majority of the colony, though it even then seemed as if there were still a few ants who did not recognize them.

I had hoped to have been able to keep various species of ants together, trusting that if they were well supplied with food and water they would not attack one another. In this expectation I have been disappointed. My ants quite appreciate the importance of rectifying their frontier, and in their case, as in others nearer home, it is especially the strong communities which feel the need of a scientific frontier to enable them to defend themselves from the attacks of the weak.

In the construction of their nests ants manifest much ingenuity. Thus, in one case I established some ants between two plates of glass, $\frac{1}{4}$ inch apart, and with three sides closed, but the fourth open. This suited them very well, but they did not like being so much exposed; accordingly they had recourse to a heap of earth, which was about three feet from the nest, and brought enough of it to close up the open side, leaving only a small door. In winter they generally fasten up their doors. The majority of our species live in the ground, some construct mounds, some burrow in wood. In parts of South America liable to floods, the ants have learnt to construct their nests in trees above the reach of the water.

Until recently we had no knowledge as to how long ants lived, but the general impression was that the workers lived one year, the queens somewhat longer. The results of my observations have proved that this was a mistake, and I have been much surprised at the longevity of my ants. I have still two queens which I have kept

under observation since the year 1874, and they were at that time probably at least a year old. They must, however, at any rate be now at least five years old, and may be more. As regards workers, I have also many belonging to several species which I have kept since 1875, and which must be at least four years old.

But though they are thus long lived, and proved very healthy in my nests, still sometimes, and especially with new nests, there was a good deal of mortality among them. They generally come out of the nest to die; but if they are from any reason unable to do so, their companions bring the corpses out of the nest and carry them off to some little distance. Nay, I have even found that they are generally placed more or less together, so as to constitute a sort of burial-ground.

It is remarkable that notwithstanding the labours of so many excellent observers, and though ants swarm in every field and every wood, we did not till lately know how their nests commence.

Three principal modes have been suggested: after the marriage flight, the young queen may either—

1. Join her own, or some other old nest;
2. Associate herself with a certain number of workers, and with their assistance commence a new nest; or
3. Found a new nest by herself.

As some nests continue to flourish for many years, the first case must be frequent, though I am not aware that any observations with reference to it are on record. Whether the other two occur, can, of course, only be settled by observation, and the experiments made to determine it have hitherto been indecisive. Blanchard, indeed, in his work on the 'Metamorphoses of Insects' (I quote from Dr. Duncan's translation, p. 205), says, "Huber observed a solitary female go down into a small underground hole, take off her own wings, and become, as it were, a worker: then she constructed a small nest, laid a few eggs, and brought up the larvæ by acting as mother and nurse at the same time."

This, however, is not quite a correct version of what Huber says. His words are: "I enclosed several females in a nest of light humid earth, with which they constructed lodges, where they resided: some singly, others in common. They laid their eggs and took great care of them: and notwithstanding the inconvenience of not being able to vary the temperature of their habitation, they reared some, which became larvæ of a tolerable size, but which soon perished from the effects of my own negligence."

It will be observed that it was the eggs—not the larvæ—which, according to Huber, these isolated females reared. It is true that he attributes the early and uniform death of the larvæ to his own negligence; but the fact remains, that in none of his observations did an isolated female bring her offspring to maturity. Forel even thought himself justified in concluding, from his own observations and those of Ebrard, that such a fact could not occur. Lepeletier de St. Fargeau

was of opinion that ants' nests originate in the second mode indicated above, and it is indeed far from improbable that this may occur. No clear case has, however, yet been observed.

Under these circumstances, I made various experiments, in order, if possible, to solve the question. For instance, I took an old fertile queen from a nest of *Lasius flavus*, and put her to another nest of the same species. The workers became very excited, and killed her. I repeated the experiment, with the same result, more than once.

I concluded then that, at any rate in the case of *Lasius flavus*, the workers would not adopt an old queen from another nest.

The following instance, however, shows that whether or not ants' nests sometimes originate in the two former modes or not, at any rate in some cases isolated queen ants are capable of giving origin to a new community. On the 14th August, 1876, I isolated two pairs of *Myrmica ruginodis*, which I found flying in my garden. I placed them with damp earth, food and water, and they continued perfectly healthy through the winter. The first eggs were laid between the 12th and 23rd of April.

They began to hatch the first week in June; the first turned to a chrysalis on the 27th, and emerged as a perfect insect on the 22nd July. Others followed shortly afterwards, and this experiment proves therefore that the queens of this species, at any rate, have the instinct of bringing up larvæ, and consequently the power of founding new communities.

Amongst other experiments to test the affection of ants for one another, I tried the following. I took six ants from a nest of *Formica fusca*, imprisoned them in a small bottle, one end of which was left open, but covered by a layer of muslin. I then put the bottle close to the door of the nest. The muslin was of open texture, the meshes, however, being sufficiently fine to prevent the ants from escaping. They could not only see one another, but communicate freely with their antennæ. We now watched to see whether the prisoners would be tended or fed by their friends. We could not, however, observe that the least notice was taken of them. The experiment, nevertheless, was less conclusive than could be wished, because they might have been fed at night, or at some time when we were not looking. It struck me, therefore, that it would be interesting to treat some strangers also in the same manner.

Now some critics have objected to my experiments (always, I must admit, in the fairest and most friendly spirit) that my ants may have been stupid ants, and that the experiments being in many cases (though by no means in all) made on captive nests, were on that account also scarcely fair to the ants. Indeed, I have myself anticipated and pointed out this objection. I am disposed to believe that in warmer countries the ants are more highly developed, as everyone knows is the case with reference to numbers, than in our comparatively cold regions. Again, much allowance must certainly be made for the fact of the ants being in captivity. However, I have always en-

deavoured so to devise my experiments that they might be tested and repeated under other conditions and with other species. Now as regards the one just mentioned, it may be said, "Oh, but these ants were under very unnatural conditions. In their native haunts they would never find their friends imprisoned in a glass bottle fastened up with muslin." That is of course true; but then it occurred to me to try the experiment with strangers.

I put, therefore, two ants from one of my nests of *F. fusca* into a bottle, the end of which was tied up with muslin as described, and laid it down close to the nest. In a second bottle I put two other ants from another nest of the same species. The ants which were at liberty took no notice of the bottle containing their imprisoned friends. The strangers in the other bottle, on the contrary, excited them considerably. The whole day one, two, or more ants stood sentry, as it were, over the bottle. In the evening no less than twelve were collected round it, a larger number than usually came out of the nest at any one time. The whole of the next two days, in the same way, there were more or less ants round the bottle containing the strangers, while, as far as we could see, no notice whatever was taken of the friends. On the 9th the ants had eaten through the muslin and effected an entrance, when the strangers were at once attacked, while, on the other hand, the friends throughout were quite neglected.

It would appear, therefore, that in these curious little creatures hatred is stronger than love.

These observations seemed to me sufficient to test the behaviour of the ants belonging to this nest under these circumstances. I thought it desirable, however, to try other communities. I selected, therefore, two other nests. One behaved just like the preceding. The other was a community of *Polyergus rufescens*, with numerous slaves. Close to where the ants of this nest came to feed I placed as before two small bottles, closed in the same way, one containing two slave ants from the nest, the other two strangers. These ants, however, behaved quite unlike the preceding, for they took no notice of either bottle, and showed no sign either of affection or hatred. Is not one tempted to surmise that the warlike spirit of these ants was broken by slavery?

In the previous lecture I mentioned that I was never able to satisfy myself that ants heard any sounds which I could produce. I would not, however, by any means infer from this that they are incapable of hearing.

Micromegas, indeed, the gigantic inhabitant of Sirius, concluded that as he heard no sound, men did not speak; moreover, Voltaire makes him ask, "How is it possible that such infinitesimal atoms as men should have organs of voice? and what could they have to say? To speak," he continues, "it is necessary to think, or nearly so: now to think requires a mind, and to attribute a mind to these little creatures would be absurd." We must be careful not to fall into a

similar series of errors. It is far from improbable that ants may produce sounds entirely beyond our range of hearing. Indeed, it is not impossible that insects may possess a sense, or rather, perhaps, sensations, of which we can no more form an idea than we should have been able to conceive red or green had the human race been blind. Helmholtz and Depretz have shown that the human ear is sensitive to vibrations reaching to 38,000 in a second. The sensation of red is produced when 470 millions of millions of vibrations of ether enter the eye in a similar time; but between 38,000 and 470 millions of millions, vibrations produce on us the sensation of heat only. We have no special organs of sense adapted to them, but there is no reason in the nature of things why this should be the case with other animals, and the problematical organs possessed by many of the lower forms favour the suggestion. If any apparatus could be devised by which the number of vibrations produced by any given cause could be lowered so as to be brought within the range of our ears, it is probable that the result would be most interesting.

I have tried unsuccessfully various experiments in order to ascertain whether the ants themselves produced any sounds for the purpose of conveying signs or ideas.

Professor Tyndall was good enough to arrange for me one of his sensitive flames, but I could not perceive that it responded in any way to my ants. The experiment was not, however, very satisfactory, as I was not able to try the flame with a very active nest. Professor Bell was also good enough to set up for me an extremely sensitive microphone; it was attached to the under side of one of my nests, but though we could distinctly hear the ants walking about, we could not perceive any other sound.

It is, however, of course possible, as I have already suggested, that ants may be sensitive to, and also themselves produce, sounds which, from the rapidity of their vibrations, or from some other cause, are beyond our range of hearing.

Having failed, however, in hearing them or making them hear me, I endeavoured to ascertain whether they could hear one another. To determine, if possible whether they have the power of summoning one another by sound, I tried the following experiments. I put out on the board where one of my nests of *Lasius flavus* was usually fed, six small pillars of wood about an inch and a half high, and on one of them I put some honey. A number of ants were wandering about on the board in search of food, and the nest itself was about 12 inches from the board. I then put three ants to the honey, and when each had sufficiently fed I imprisoned her and put another; thus always keeping three ants at the honey, but not allowing them to go home. If, then, they could summon their friends by sound, there ought soon to have been many ants at the honey. The results were as follows:—

We began to watch at 11, and up to 3 in the afternoon only seven ants had found their way to the honey, which was about as many as ran up the other pillars. The arrival of these seven, therefore, was

not more than would naturally have resulted from the number of ants running about. We then, at 3, allowed the ants which were feeding to return to the nest. In less than half an hour after this eleven came, and in the following half hour no less than forty-three. So that in the first four hours only seven came, while in less than an hour after the first was allowed to return to the nest, no less than fifty-four came.

Again, on September 30th, I tried the same arrangement, beginning at 11; up to 3.30, seven ants came. We then let them go. From 3.30 to 4.30, twenty-eight came. From 4.30 to 5, fifty-one came. Thus, in four hours and a half only seven came; while when they were allowed to return, no less than seventy-nine came in an hour and a half.

I tried this experiment several times more, and always with similar results. It seems obvious, therefore, that, in three cases at least, no communication was transmitted by sound.

I will now endeavour to show you one or two microscopical preparations, merely to give you a very slight idea how beautiful and complex the anatomical structure of an ant is. Here is a longitudinal section of a queen ant.

Sir John then proceeded to describe the principal points :—

The organs of vision of ants are generally well developed and conspicuous. There are usually three simple eyes, or ocelli, arranged in a triangle on top of the head, and on each side a large compound eye. These compound eyes are very complex organs, but the mode in which they act is by no means understood. They consist of a number of facets, varying from 1–5 in *Ponera contracta*, to more than 1000 in each eye—as, for instance, in the males of *F. pratensis*. In fact, these, so far fortunate, insects realize the wish of the poet—

> "Thou lookest on the stars, my love,
> Ah, would that I could be
> Yon starry skies, with thousand eyes
> That I might look on thee."

But if the male of *F. pratensis* sees 1000 queens at once, even when only one is really present, this would seem to be a bewildering privilege, and the prevailing opinion among entomologists is that each facet only takes in a portion of the object.

From the observations of Sprengel, there could, of course, be little, if any, doubt, that bees are capable of distinguishing colours: but I have, in my previous lecture, recorded some experiments which put the matter beyond a doubt. Under these circumstances, I was naturally anxious to ascertain if possible, whether the same is the case with ants. I have, however, experienced more difficulty in doing so, because ants find their food so much more by smell than by sight.

I tried, for instance, placing food at the bottom of a pillar of coloured paper, and then moving both the pillar and the food. The pillar, however, did not seem to help the ant at all to find her way to

the food. I then placed the food on top of a rod of wood 8 inches high, in fact a pencil, and when the ant knew her way perfectly well to the food, so that she went quite straight backwards and forwards to the nest, I found that if I moved the pillar of wood only 6 inches, the ant was quite bewildered, and wandered about backwards and forwards, round and round, and at last only found the pillar accidentally as it were.

Under these circumstances, I could not apply to ants those tests which had been used in the case of bees. At length, however, it occurred to me that I might utilize the dislike which ants, when in their nests, have to light. Of course, they have no such feeling when they are out in search of food: but if light be let in upon their nests, they at once hurry about in search of the darkest corners, and there they all congregate. If, for instance, I uncovered one of my nests, and then placed an opaque substance over one portion, the ants invariably collected in the shaded part.

I procured, therefore, four strips of glass, similar, but coloured respectively green, yellow, red, and blue, or, rather, violet. The yellow was somewhat paler in shade, and that glass consequently more transparent than the green, which, again, was rather more transparent than the red or violet. I then laid the strips of glass on one of my nests of *Formica fusca*, containing about 170 ants. These ants, as I knew by previous observations, seek darkness, and would certainly collect under any opaque substance. I then, after counting the ants under each strip, moved the colours gradually at intervals of about half an hour, so that each should by turns cover the same portion of the nest. The results were as follows—the numbers indicating the approximate numbers of ants under each glass (there were sometimes a few not under any of the strips of glass):—

1.	Green	Yellow	Red	Violet
	50	40	80	0
2.	Violet	Green	Yellow	Red
	0	20	40	100
3.	Red	Violet	Green	Yellow
	60	0	50	50
4.	Yellow	Red	Violet	Green
	50		1	40
5.	Green	Yellow	Red	Violet
	30	30	100	1
6.	Violet	Green	Yellow	Red
	0	14	5	140
7.	Red	Violet	Green	Yellow
	50	0	40	70
8.	Yellow	Red	Violet	Green
	40	50	1	70

9.	Green	Yellow	Red	Violet
	60	35	65	0
10.	Violet	Green	Yellow	Red
	1	50	40	70
11.	Red	Violet	Green	Yellow
	50	2	50	60
12.	Yellow	Red	Violet	Green
	35	55	0	70

Average, Red over 70, Green 48, Yellow 45, Violet ½.

Adding these numbers together, there were, in the twelve observations, under the red 890, under the green 544, under the yellow 495, and under the violet only 5. The case of the violet glass is most marked. To our eyes the violet was as opaque as the red, more so than the green, and much more so than the yellow, yet, as the numbers show, the ants had scarcely any tendency to congregate under it. There were nearly as many under the same area of the uncovered portion of the nest as under that shaded by the violet glass.

I also experimented in the same way with a nest of *Formica fusca*, in which there were some chrysalises. These chrysalises were generally collected into a single heap. I used glasses coloured dark yellow, dark green, light yellow, light green, red, violet, and dark purple. The colours were always in the same order, but the places were shifted after each observation. To my eye the purple was almost black, the violet and dark green very dark and almost opaque; the purple could be dimly seen through the red, rather more clearly through the dark yellow, while the light yellow and light green were almost transparent. The purple were in fifteen observations six times placed under the dark green, three under dark yellow, four under dark red, once each under light yellow and light green, but not once under the violet or purple. In another experiment the purple were placed seven times under the red, six under the dark yellow, never under any of the other colours. The same experiment tried with another species—*Lasius niger*—gave very similar results; the purple being placed in forty experiments, nineteen times under the dark yellow, sixteen under the red, five under the green. In some subsequent experiments the green and yellow seemed to be decidedly preferred to the red.

It is curious that the coloured glasses appear to act on the ants (speaking roughly) as they would, or, I should rather say, inversely as they would on a photographic plate. It might even be alleged that the avoidance of the violet glass by the ants was due to the chemical rays which are transmitted. From the habits of these insects such an explanation was very improbable. If, however, the preference for the other coloured glasses to the violet was due to the transmission, and not to the absorption of rays, that is to say, if the ants went under the green and red rather than the violet, because

the green and red transmitted rays which were agreeable to the ants, and which the violet glass, on the contrary, stopped, then, if the violet was placed over the other colours, they would become as distasteful to the ants as the violet itself. On the contrary, however, whether the violet glass was placed over the others or not, the ants equally readily took shelter under them. Obviously, therefore, the ants avoid the violet glass because they dislike the rays which it transmits.

Mr. Busk suggested that as the red glass stops the chemical rays more effectually than the yellow or green, while the violet is most transparent to them, and as the ants appear to prefer the red glass to the yellow or green, and these, again, to the violet, possibly the explanation might be that the chemical rays were peculiarly distasteful to them. To test this, therefore, I made some experiments with fluorescent liquids, which Mr. Hanbury was kind enough to procure for me. I poured them into shallow glass cells, about half an inch deep, which I put, as before, over the ants. If now they were affected mainly by the chemical rays, it must appear to them to be dark under these solutions. This, however, was not the case. The solutions seemed to make no difference to them. I also tried quinine and uranium glass with the same effect. I conclude therefore that the ants are affected by the true light rays.

It is obvious that these facts suggest a number of interesting inferences. I must, however, repeat the observations and make others; but we may at least, I think, conclude from the preceding that: (1) ants have the power of distinguishing colour; (2) that they are very sensitive to violet light; and it would also seem (3) that their sensations of colour must be very different from those produced upon us.

But though it is thus, I think, sufficiently evident that ants are differently affected by different colours, it by no means follows that they should see them as we do. It is, indeed, most remarkable how little we yet know with reference to their real nature, or how nature herself appears to them. What actual impressions do colours give them? What are the limits of their vision, how far, and how distinctly can they see; can they hear any sounds, or do they live in everlasting silence? Have they senses with reference to which we have as yet no knowledge? Last, but not least, how far are they mere exquisite automatons; how far are they conscious beings? When we see an ant-hill, tenanted by thousands of industrious inhabitants, excavating chambers, forming tunnels, making roads, guarding their home, gathering food, feeding the young, tending their domestic animals, each one fulfilling its duties industriously, and without confusion, it is difficult altogether to deny to them the gift of reason; and yet it is perhaps wiser to admit that the whole question is still a mystery.

[J. L.]

WEEKLY EVENING MEETING,

Friday, May 16, 1879.

William Spottiswoode, Esq. M.A. D.C.L. LL.D. President R.S. Vice-President, in the Chair.

Professor A. Cornu,

PROFESSOR AT THE POLYTECHNIC SCHOOL, MEMBER OF INSTITUTE OF FRANCE.

The Optical Study of the Elasticity of Solid Bodies.

Preliminary Remarks.

All solid bodies utilized in scientific and industrial applications are more or less elastic: and it is very important, in a practical as well as a theoretical point of view, to be able to predict the deformations due to given forces, or, conversely, to know the forces which correspond to given deformations.

Mathematical Calculation enables us to solve both problems for every description of form and of force in all their details, provided that it borrows from experience certain results obtained in very simple cases.

Homogeneous and Isotropic Bodies.

An elastic bar urged by external traction forces extends itself along its largest dimensions (longitudinal extension): at the same time, by the natural play of internal forces, its transversal dimensions diminish (transversal contraction).

[Illustration of this general fact with an indiarubber bar.]

In order to calculate all the circumstances of deformation of an elastic isotropic body, whatever may be its shape and acting forces, it is sufficient to know the rate of longitudinal extension (modulus of elasticity), and its ratio to the transversal contraction.

Various opinions amongst the Physicists upon the value of this Ratio.

A considerable number of physicists (Cagniard-Latour, Wertheim, Prof. Kirchhoff, Dr. Everett, &c.) have made a series of experiments on various supposed isotropic bodies.

The question, a very important one in a theoretical point of view, is to know if this ratio is a variable one according to the nature of

the substance, or an invariable one, and equal to $\frac{1}{4}$ as given by the Navier's and Green's theories.

Double difficulty. 1. Is the body really homogeneous and isotropic ?

The metals are always annealed or crystallized: homogeneous glass is one of the bodies approaching nearest to theoretical isotropy.

2. The transversal contraction is extremely small.

Necessity of using any indirect mode of deformation to determine accurately the transversal contraction.

Mode of Experiment: Circular Flexion of a Rectangular Rod.

The upper surface, primitively plane, becoming curved with two opposite curvatures (and not cylindrical, as commonly supposed), like a *horse saddle.* The ratio of the main radii of curvature is, according to a theorem due to Mr. De St. Venant, precisely the ratio in question.

[Illustration of this general fact with an indiarubber plate.]

Optical Methods for Testing the Deformation of the Surface of Elastic Bodies.

1. *Variation of Focus of a Beam of Light Reflected from the Polished Surface of the Elastic Body.*

2. *Use of the Newton's Coloured Rings.*

Newton's rings are produced by illuminating with white or with monochromatic light the thin film of air comprised between a fixed surface and the exterior surface of the elastic body.

Extreme sensitiveness of this method, according to the small difference of thickness, corresponding to the successive rings.

The lines of equal intensity of the rings correspond to the lines of equal thickness of the film of air. The successive rings correspond to a difference of thickness of about $\frac{1}{4}$ of a thousandth of a millimeter (a hundred thousandth of an inch).

If the fixed surface is a plane one, the appearance of the rings is exactly the *topographic map* of the deformed surface, of which the *scale of elevation* is the small length above defined.

In a small part of the field, the rings coincide, in form, with conic sections, concentric with the *indicatrix curve* of Ch. Dupin.

Illustration of various forms of Newton's rings—circular, elliptic, hyperbolic—with monochromatic light (sodium vapour in electrical arc).

Optical Method of Testing the Circular Flexion.

A piece of plate-glass is used. The Newton's rings, before flexion, more or less regular according to the perfection of polish,

become, by increasing forces, more and more regular, and take the form of conjugate hyperbolas, the axes of which being parallel and perpendicular to the main dimension of the rod.

The trigonometrical tangent of the semi angle of the common asymptote converges towards the value $\frac{1}{2}$; therefore, the ratio of the curvatures, and consequently the ratio in question, is $\frac{1}{4}$ with the best isotropic body.

The theoretical solution of the problem seems to be in favour of Navier's and Green's theories.

Generality of the Optical Method.

Application to the torsion of a rectangular plate.

The shape of the deformed surface becomes a hyperbolic paraboloid; but the asymptotes of the hyperbolæ (and not the axis, as before) are parallel and perpendicular to the main dimension of the rod.

Fixing of the Newton's Rings by Photography.

In order to study at leisure, and with accuracy, the *topographic surfaces* of elastic deformation, it is very convenient to keep an exact and fixed image of the field.

The induction spark between two poles of magnesium supplies a source of light which fulfils the three necessary conditions—to be intense, photographic, and monochromatic.

Amongst the bright lines of the magnesium spectrum none is useful for that purpose; the radiation utilized as a source of photographic light is invisible, but becomes visible when projected on Prof. Stokes's fluorescent screen.

Though the photography of the Newton's rings be a delicate operation, the experiment will be tried before the audience.

Newton's rings were photographed for the first time by the illustrious Dr. Young at the Royal Institution in the year 1803.

WEEKLY EVENING MEETING,

Friday, May 23, 1879.

William Spottiswoode, Esq. D.C.L. L.L.D. President R.S. Vice-President, in the Chair.

W. H. Preece, Esq. M. Inst. C.E. *M.R.I.*

Multiple Telegraphy.

Many of you, in your rural rambles, may, during a moment of reflection or thoughtlessness, while standing on the parapet of a rustic bridge, have dropped a stone into the river flowing gently below, and have noticed the rings of waves projecting outwards in ever increasing circles. If at the same moment a hungry fish, in his desire to satisfy a natural craving, should dart or snatch at a fly on the surface of the water, a second series of rings of waves would be produced, and in such a case a careful observer would notice that where crest of wave meets crest of wave there is a higher crest formed, and where hollow meets hollow a deeper hollow is formed. This super-position of wave on wave is called interference, and the interference of undulations plays a most important part in the phenomena of sound, of light, and of electricity.

In electricity, wave upon wave can be super-imposed, either in waves flowing in the same direction or in waves flowing in opposite directions. The usual indication of the presence of electric waves is either by the attraction of a magnet or by the deflection of a needle.

Here is a needle that is subjected to deflection by the passage of such waves of electricity. I send a current of positive electricity through the wire surrounding the magnet to which this needle is attached, and you see I get a certain deflection; I increase the strength of that wave by super-imposing another wave, and you see I increase the amount of the deflection. In the same way, if I reverse the direction of the current and send a negative current, I get a wave of a certain strength, and if I double the strength of the current I get a stronger deflection. But if, instead of super-imposing one wave on the other in the same direction, I send one wave in one direction and the other wave in the opposite direction, we get neutrality. This neutrality is a phenomenon corresponding very much to silence in sound or to darkness in light.

Advantage is taken of this neutrality in duplex and quadruplex

telegraphy, in a way which I will now show to you. It is usually imagined by those who think of duplex telegraphy that something crosses or passes in the same way that railway trains pass or cross each other. It is not at all necessary to conceive that anything whatever passes. For instance, here I have some glass balls placed in a row between two parallel wooden rods. If I take the ball at one end, say No. 1, a short distance from its normal position, and drive it along the rods back again, you will notice that a something, a form of force, passes through the other balls, and the one at the far end is by this developed motion forced a distance away from its neighbours. The same thing occurs if I take No. 12, as No. 1 is then displaced by the concussion. This resultant motion represents, and may be called a telegraphic signal. If I am skilful enough to take No. 1 and No. 12, and let them return together, we shall see that they are both driven back simultaneously, the intervening balls remain undisturbed, and two such signals will be made. It is this latter phenomenon that I wish you to bear in mind as being analogous to the principle used in duplex telegraphy.

The next fact that I wish to impress upon you is that if a current of electricity have many paths open to it, it will always separate or spread itself among those paths in inverse proportion to the resistance which each opposes to its progress. The greater the resistance, the smaller the current; the less the resistance, the greater the current.

If we have two paths or lines open to a current, and these two lines be of exactly equal resistance, the current will divide itself between them in exactly equal proportions.

The next point, almost as self-evident as the previous one, but which time will not allow me to illustrate by experiment, is that the magnitude of the magnetism produced in the electro-magnet is simply proportional to the strength of current passed through that magnet.

The last elementary fact I have to bring before you is that the polarity or direction of the magnetism simply depends upon the direction of the current.

On the wall I have two electro-magnets connected with each other by means of a wire. Around each are coiled two wires of exactly equal length and equal resistance. If I send a current through one of the wires I produce a deflection in one direction; if I send a current through the other wire I get a deflection in the opposite direction. You can imagine that one of these magnets is in Brighton and the other in London, with the connecting line wire between them. I want to arrange matters so that when I make a signal at Brighton I shall not in any way affect my own instrument, and to do so I divide my current in halves; one half goes through one wire of the magnet, and tends to deflect the needle in one direction, and the other half goes through the other wire of the magnet, with a tendency to throw the needle in the opposite direction. If by an arrangement, such as an artificial line, I make these two halves exactly equal, then no deflection of my needle takes place; though at

the same time I influence Brighton's needle. This gives us the first principle of duplex telegraphy. Of course, with a similar arrangement at Brighton, on the key at that end being depressed the needle there remains unaffected, while at the same time the needle at this end is deflected. When, however, under these conditions, currents are sent from both instruments, signals or deflections are noticed at each station. By using resistance we are able to make an artificial line attached to the second wire of the electro-magnet exactly equal to the line wire attached to the other wire of the electro-magnet. I have in a box here resistance equal to three or four hundred miles of line. Resistance is measured in ohms, an ohm being a unit used by electricians, and which represents about a yard of fine platinum wire, or about one-tenth of a mile of ordinary iron wire. The resistance contained in this box is divided into quantities ranging from one to two thousand ohms, any portion of which can be brought into use by simply taking out pegs representing the amount required. With this ready means at hand you will easily comprehend the facility with which a balance can be adjusted, and the opposition to the two halves of the current passing through the electro-magnet be made equal. Whatever the distance of the line, whether between here and the Central Telegraph Station, or between here and Calcutta, or any distant place to which you choose to exert your imagination, the effect is just the same. To show you this system in actual working I have here instruments joined to a wire which passes through the streets to the Central Telegraph Station in the city. It is just as easy for us to connect a wire up in actual operation as to have assistants secreted in the adjoining room, which is sometimes supposed to be the case. I must first of all explain to you how telegraphic signals are interpreted into ordinary language. It is by means of what is known as the Morse alphabet, which consists of combinations of dots and dashes used to represent the ordinary letters. A dot and a dash, for instance, represents *a*, a dash and three dots represents *b*, dash dot dash dot *c*, and so on throughout the alphabet. A dot itself is represented on the instrument before us by a short sound or beat, and a dash by a longer one. The sounds caused by the signals appeal to the consciousness through the ear, and are translated into the proper language. (The central station was called up, and a message was sent to him at the same time that one was being received from him, in illustration of duplex working.)

The operation which you have just seen going on exactly corresponds with that which I just described to you. The division of the current is arranged at each end by the use of a resistance box or "rheostat," and the signals are sent without any interruption between the one and the other.

The duplex system of telegraphy is employed to a very large extent in this country, no less than 320 circuits being so fitted. The system is applicable to long and short lines, and even long cables in the Red Sea, Indian Ocean, and across the Atlantic (in one case 2400 miles long), have been successfully so fitted.

Its application enables really more than double the amount of work being done to that which the wire (if an ordinary land line) would perform when working singly. This arises from the fact that no interruption ensues from repetitions being required or questions asked, but the messages pass in a continuous stream in opposite directions almost without let or hindrance.

I would just mention another fact in answer to any who are surprised at errors taking place in the transmission of messages. The signals representing certain letters are very similar, and it is very easy for a faint dot to be missed or a short dash to be misinterpreted for a dot, and as the difference between the letter *t* (represented by a dash) and *r* (represented by a dot, dash, dot) is simply two dots, you will not be surprised that a message, informing some friends of the arrival of a party of ladies, "all right," was delivered as, "all tight." And, again, a friend of mine in Manchester, whose wife wished to inform him that the "rash was all gone," was astonished to receive an intimation that the "cash was all gone." Many of these errors are simply due to the failure of a dot or the shortness or breaking of a dash, and the wonder is that with more than one hundred million messages which are despatched every year in this country the percentage of errors is not greater than the small amount it is. I hope that, after the practical illustration you have just had of the extreme care and exactitude which is attendant on the accurate receipt of signals, you will deal lightly with errors of conversion of words such as "rash" into "cash," "right" into "tight," &c.

So far as regards duplex telegraphy.

We have now to deal with another class, and that is called diplex telegraphy. Duplex telegraphy means sending two messages in opposite directions at the same time; diplex means sending two messages at the same time in the same direction. I have here two little instruments which give out their signals in musical tones, and I have also two keys attached to a battery in connection with them. There is only one wire between them. If I press down, say, key No. 1 I call up the sounder which emits a deep tone; if I press down key No. 2 I obtain a response from the sounder with a tone an octave higher. If I press both keys down at the same time both sounders answer, and whichever key is depressed it is always answered by its proper sounder. This power of sending two signals or messages at the same time in the same direction is called diplex telegraphy.

It is difficult to explain how this is performed without the aid of a diagram, and it really requires some amount of courage to attempt the feat. Currents of electricity are developed in two ways; they differ in the direction in which they flow and also in their strength.

The key on my right hand simply reverses the direction in which the currents flow every time it is depressed; the other key simply increases the strength of the current flowing, whatever may be its direction. The relay here employed, which is connected to the deep-toned sounder, responds to a reversal of the current whatever its

strength may be, and the other relay, which is connected to the light sounder, responds to the increased strength of the current whatever its direction may be; so that on depressing the key which reverses the current the deep-toned sounder responds; and if I touch the other key I simply make the light sounder speak by increasing the strength of that current. (Mr. Preece here very minutely traced the different currents from the battery through key and relay to sounder, so as to produce sounds by currents, varying in either direction or strength, and acting upon one or both sounders at the same time, or separately at will.) The increase of current is brought into action by means of the one key having attached to it a greater proportion of cells of the battery.

That is diplex telegraphy.

How is this diplex used for quadruplex working? I showed you in duplex working that we simply split the current in two paths made exactly equal to each other. It matters not whether we have one or any number of relays in. Theoretically, it is possible to insert a great number of relays, but practically only two are used; and in quadruplex working we simply duplex what I have called the diplex arrangement. On one end of the table before you we have two sides of the quadruplex, or one diplex; and on the other end of the table we have the other side of the quadruplex which also works diplex, and by this apparatus we are able to send four messages at the same time on the single wire, which you see goes from the table to the wall, and so proceeds to the Central Telegraph Station. (The various courses of the currents and adjustments of balance necessary, chiefly owing to variations of temperature, &c., were here described, but without the aid of a diagram it would be next to impossible to follow them.) The system, although difficult in description, is really wonderfully and beautifully simple, and if it were only requisite to follow out what I have just said, quadruplex working would be a very easy matter indeed. But there is such a thing in England as a climate, and there also is unfortunately such a thing as rain, and rain interferes very considerably with the action of our duplex and quadruplex working. In a word, the effect of rain is precisely the same as reducing the length of our line. Supposing we have a wire between Liverpool and London, about 200 miles long, then when we have rain it covers the insulators with moisture, and the moistened insulators allow the current to escape to earth, and the result is just the same as though the line itself were reduced in length; and to compensate for this it is necessary to take out resistance equal to the loss. But by means of the rheostat it is not a very difficult matter to adjust a balance, and by carefully noting and watching storms of rain, snow, sleet, or fogs and mist, which are so troublesome to the working, a clerk has simply to vary his rheostat, and can so maintain working on wires of ordinary length.

When we come to very long lengths of wire the weather disturbance interferes very much.

There is another disturbance besides that produced by rain or weather, and that is one due to the existence in a telegraphic line of what is called electrostatic induction. This is a very hard word, but it really means something similar to the effect of friction in pipes on the flow of water or gas. When gas or water is forced through pipes, a quantity of it adheres to the sides, and produces a diminution of pressure due to friction. So, when we try to force electricity through a wire, a quantity of it adheres, as it were, to the sides of the wire, remains there as a charge, and diminishes the action of the current, and produces what is called retardation in signals. Retardation means really reducing the rate at which we work. If it is possible, say, between London and Aberdeen to work as fast as ever we like, then if we attempt to work over a submarine cable of equal length the speed diminishes very greatly indeed. Between London and Cork, and between London and Aberdeen we are at the present moment working practically as fast as the instruments will run, at the rate of about 150 words a minute, while through the Atlantic cable, with all their skill, they cannot work at more than twenty-five words a minute, and this is due to the disturbing element of electrostatic induction. To compensate for this it is necessary to make your artificial line exactly similar in all respects to your real line, and, with duplex circuits, this is done by inserting, in addition to the rheostat, a condenser. A condenser is simply a series of leaves of tinfoil separated by paraffined paper. A series of alternate paraffined papers and tinfoil really comprise a Leyden jar, and such a Leyden jar would have the capacity of retaining a charge similar to that retained by the line; and it is only necessary to increase the size of the condenser until the same electrostatic capacity in it is obtained as is experienced in the line itself. Here is a condenser which has not the appearance of being a very formidable instrument, but it has sufficient electrostatic capacity to compensate for a line about 200 miles long.

We will now work the quadruplex apparatus before you. I do not hope to have made you comprehend the action of the system; I only hope that I have given you an idea which you can work out for yourselves, and which will give you more interest in seeing the system in actual operation.

(Communication was then opened with the Central Telegraph Station, and several messages were sent to and from the Institution on the quadruplex apparatus.)

Many people are suspicious of operations of this kind going on at lectures. I may tell you that a few years ago I gave a lecture at Southampton, and took special care to have a wire joined through London to the Continent (we only had one cable at that time), and at the proper period of the evening spoke to London, Vienna, Berlin, and Amsterdam, and received answers. I then asked what time it was in Vienna (it was nine o'clock in Southampton), and received answer, "Twenty minutes past eight." This was wrong, and it turned out that a clerk in London had been personating the

Continental places, and knowing that there was forty minutes' difference in the time he put it on the wrong side!

We have in England at the present moment six circuits worked with the quadruplex system. In America the system is carried out to a much larger extent, and sixty-three wires are fitted with it, and over these sixty-three circuits no less than eight million messages are transmitted annually.

The question may arise in your minds as to why the quadruplex apparatus is not used as extensively in this country as in America, and the answer would be that in this country we have not the same necessity for it. We have apparatus in use superior to the quadruplex. I mean the Wheatstone automatic, an instrument which was in its early stage brought before an audience of this Institution. It enables us to transmit messages and news with enormous rapidity, and makes us quite independent of any of those supposed advanced inventions.

What has transpired before you this evening is simply one of the innumerable applications of electricity that are now daily in use, and it really makes us regard with wonder what science is doing for us. What you have just seen far exceeds the dream of the wildest alchemist, and the most imaginative necromancer never could have conceived the possibility of four persons talking to each other at the same time separated by a distance of 200 miles; but when we assail Nature in her strongholds it is astonishing to find how easily she is mastered, how simple are the means by which she veils her secrets, and how rude are the weapons she places in our hands to produce before you these wonders.

[W. H. P.]

WEEKLY EVENING MEETING,

Friday, May 30, 1879.

George Busk, Esq. F.R.S. Treasurer and Vice-President, in the Chair.

Grant Allen, Esq.

*The Colour-Sense in Insects: its Development and Reaction.**

The lecturer began by pointing out the probable absence of all bright-coloured flowers and insects in the world whose fauna and flora have been preserved to us by the primary rocks. Hence it might be inferred that no animals then possessed a colour-sense, because there were few or no coloured objects upon which it could be exercised.

He traced the development of colour-perception in insects to the gradual growth of entomophilous flowers. All parts of plants in which oxidation is taking place are liable to display brilliant pigments other than green; and this is especially the case in the neighbourhood of the floral organs. Flowers which exhibited this tendency in a high degree would attract the eyes of insects, and so gain easier fertilization. While, conversely, insects which were able to discriminate such patches of colour to the greatest extent, would best discover the pollen and honey. Thus nascent colour in flowers and the nascent colour-sense in insects would develope side by side, till they reached their present high point of perfection.

But not only would a power to discriminate different hues arise in the process of evolution: a taste for bright tints would also spring up in the insect consciousness. This taste exerts itself actively in the preference for beautiful mates, which is especially visible amongst flower-haunting insects. The lepidoptera exhibit the brightest hues of all, while the rose-chafers, the anthophilous diptera, and the other tribes of like habit, rank next to them in beauty of colouration.

The lecturer combated the idea that such selective preference transcends the faculties of insects, and showed that various other facts lead up to a similar conclusion. Certain species and genera were proved by Müller's observations to possess greater æsthetic sensibility

* See the Lecturer's work 'The Colour-Sense: its Origin and Development. An Essay in Comparative Psychology.' Trübner & Co. 1879.

than others; and the facts of mimicry give good evidence that insects notice comparatively minute distinctions of colour, form, and ornamental markings. The spots and lines on entomophilous flowers, which act as honey-guides to bees, were further adduced as showing that insects pay great attention to varieties of colouration.

Finally, the lecturer pointed out that an immensely large proportion of what we consider beauty in the external world is due to the colour-sense in insects. [G. A.]

GENERAL MONTHLY MEETING,

Monday, June 2, 1879.

George Busk, Esq. F.R.S. Treasurer and Vice-President, in the Chair.

Miss Lucy Bligh,
Fung-Yee, Esq. Interpreter to the Chinese Legation,
Lionel Gye, Esq.
Lieut.-Col. Charles Alexander McMahon,
James Mason, Esq. F.C.S.

were *elected* Members of the Royal Institution.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

New Zealand Government—Statistics of New Zealand for 1877. fol. 1878.
Accademia dei Lincei, Reale, Roma—Atti, Serie Terza, Transunti, Vol. III. Fasc. 5. 4to. 1879.
Astronomical Society, Royal—Monthly Notices, Vol. XXXI. No. 6. 8vo. 1879.
Aubertin, J. J. Esq. M.R.I. (the Translator)—The Lusiads of Camoens. 2 vols. 8vo. 1878.
British Architects, Royal Institute of—1878–9: Proceedings, No. 12. 4to. Transactions, No. 10. 4to.
Chemical Society—Journal for May, 1879. 8vo.
Coutts, John, Esq. (the Author)—What is Truth? 16to. 1879.
Dax: Société de Borda—Bulletins, 2e Série, Quatrième Année; Trimestre 1. 8vo. Dax, 1879.
Editors—American Journal of Science for May, 1879. 8vo.
Analyst for May, 1879. 8vo.
Athenæum for May, 1879. 4to.
Chemical News for May, 1879. 4to.
Engineer for May, 1879. fol.
Horological Journal for May, 1879. 8vo.

Editors—Iron for May, 1879. 4to.
Journal for Applied Science for May, 1879. fol.
Monthly Journal of Science, May, 1879.
Nature for May, 1879. 4to.
Telegraphic Journal for May, 1879. 8vo.
Ferguson, Professor J. (the Author)—Sir Humphry Davy. (L 17) 8vo. 1879.
Franklin Institute—Journal, No. 641. 8vo. 1879.
Geographical Society, Royal—Proceedings, New Series. Vol. I. No. 5. 8vo. 1879.
Geological Society—Quarterly Journal, No. 138. 8vo. 1878.
Geological Society of Ireland—Journal, Vol. XV. Part 1. 8vo. 1878.
Linnean Society—Journal, Nos. 79, 101. 8vo. 1879.
Mann, R. J. M.D. M.R.I. (the Author)—The Zulus and Boers of South Africa. (2 copies) 12mo. 1879.
Contributions to the Meteorology of Natal. (Met. Soc. Jour. 1878.) 8vo.
Henry Reeve, M.D.—Journal of a Residence at Vienna and Berlin in the Eventful Winter, 1805–6. 12mo. 1877.
Modern Meteorology. Six Lectures. By Dr. R. J. Mann and Others. 16to. 1879.
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WEEKLY EVENING MEETING,

Friday, June 6, 1879.

George Busk, Esq. F.R.S. Treasurer and Vice-President, in the Chair.

James Dewar, M.A. F.R.S.

FULLERIAN PROFESSOR OF CHEMISTRY, ROYAL INSTITUTION, ETC.

Spectroscopic Investigation.

In Kirchhoff's celebrated paper "On the Relation between the Radiating and Absorbing Powers of different Bodies for Light and Heat," the remarkable experiments of reversing the bright lines of lithium and sodium by causing sunlight to pass through the vapours of those metals, volatilized in the flame of a Bunsen's burner, are described. Bunsen and Kirchhoff reversed the stronger lines of potassium, calcium, strontium, and barium by deflagrating their chlorates with milk-sugar, before the slit of the solar spectroscope. Recent researches on the artificial formation of Fraunhofer lines have been made by Cornu, Lockyer, and Roberts.

Cornu improved upon a method previously used by Foucault. It depends upon so arranging the electric arc that the continuous spectrum of the intensely heated poles is examined through an atmosphere of the metallic vapours volatilized around them. By this means Cornu succeeded in reversing several lines in the spectra of the following metals, in addition to those above mentioned, viz. thallium, lead, silver, aluminium, magnesium, cadmium, zinc, and copper. He observed that, in general, the reversal began with the least refrangible of a group of lines, and gradually extended to the more refrangible lines of the group, and drew the conclusion that a very thin layer of vapour was sufficient for the reversal. In almost every case the lines reversed are the more highly refrangible of the lines characteristic of each metal.

Lockyer's plan was to view the electric arc through the vapours of the metals volatilized in a horizontal iron tube. The iron tube had its ends covered with glass plates, and was heated in a furnace, a current of hydrogen passing during the experiment. He did not succeed in observing any new reversal of bright lines, with the exception of an unknown absorption line which sometimes appeared when zinc was experimented on. He confirmed, however, the channelled-space absorption spectra observed by Roscoe and Schuster in the cases of potassium and sodium, and recorded channelled-space

spectra in the case of antimony, phosphorus (?), sulphur, and arsenic (probably). "As the temperature employed for the volatilization of the metals did not exceed bright redness, or that at which cast iron readily melts, the range of metals examined was necessarily limited; and in order to extend these observations to the less fusible metals, as well as to ascertain whether the spectra of those volatilized at the lower temperature would be modified by the application of a greater degree of heat," a new series of experiments were undertaken by Lockyer and Roberts, in which the combined action of a charcoal furnace and the oxy-hydrogen blowpipe was employed. A lime crucible after the form of Stas was used to replace the iron tube. By this means they obtained still no new reversal of a metallic line, but they observed channelled-space spectra in the cases of silver, manganese, chromium, and bismuth. They observed, however, that the metal thallium gave the characteristic *bright* green line, the light of the arc not being reversed.

In the above-mentioned experiments, the coolness of the ends of the tube, which acted as condensers of the metallic vapours, and the continual change of density and temperature necessarily produced by the maintenance of a current of hydrogen through the tube, appear to account for the failure in observing reversals.

The following facts have been acquired during the course of a long series of conjoint experiments with my distinguished colleague, Professor Liveing, of Cambridge.*

In order to examine the reversal of the spectra of metallic vapours, it is more satisfactory to observe the absorptive effect produced on the continuous spectrum emitted by the sides and end of the tube in which the volatilization takes place. For this purpose it is convenient to use iron tubes about half an inch in internal diameter, and about 27 inches long, closed at one end, thoroughly cleaned inside, and coated on the outside with borax, or with a mixture of plumbago and fireclay. These tubes are inserted in a nearly vertical position in a furnace fed with Welsh coal, which will heat about 10 inches of the tube to about a welding heat, and observations are made through the upper open end of the tube, either with or without a cover of glass or mica. To exclude oxygen, and avoid as much as possible variations of temperature, hydrogen is introduced in a gentle stream by a narrow tube into the upper part only of the iron tube, so that the hydrogen floats on the surface of the metallic vapour without producing convection currents in it. By varying the length of the small tube conveying the hydrogen, the height in the tube to which the metallic vapour reaches may be regulated. Thus different depths of metallic vapour may be maintained at a comparatively constant temperature for considerable periods of time. The general plan of the apparatus is given in Plate I. (at the end of the paper).

"On the Reversal of the Lines of Metallic Vapours," Nos. I., II., III., IV., V., VI., 'Proc. Roy. Soc.,' 1878–1879.

By this means the characteristic lines of the volatile metals thallium and indium may be easily reversed.

Metallic lithium, alone or mixed with sodium, gave no results. Similarly, chloride of lithium and metallic sodium, introduced together, gave no better results. To a tube containing mixed potassium and sodium vapour, lithium chloride was added. Now the bright-red lithium line was sharply reversed, and remained well defined for a long time. The lithium line was only reversed in a mixture of the vapours of potassium and sodium, and it seems highly probable that a very slightly volatile vapour may be diffused in an atmosphere of a more volatile metal, so as to secure a sufficient depth of vapour to produce a sensible absorption. This would be analogous to well-known actions which take place in the attempt to separate organic bodies of very different boiling points by distillation, where a substance of high boiling point is always carried over, in considerable quantity, with the vapour of a body boiling at a much lower temperature.

Sodium and potassium, when observed in such tubes, give none of the appearances noted by Lockyer, "On a New Class of Absorption Phenomena," in the 'Proceedings of the Royal Society,' vol. xxii., but the channelled-space spectrum of sodium described by Roscoe and Schuster in the same volume of the 'Proceedings' was often seen. Potassium gives no channelled-space absorption, but continuous absorption in the red, and one narrow absorption band, with a wave-length of about 5,730, not corresponding with any bright line of that metal.

The absorption spectrum of sodium vapour is by no means so simple as has been generally represented. The fact that the vapour of sodium in a flame shows only the reversal of the D lines, while the vapour, volatilized in tubes, shows a channelled-space absorption corresponding to no known emission spectrum, appears to be a part of a gradational variation of the absorption spectrum, which may be induced with perfect regularity. Experiments with sodium exhibit the following succession of appearances, as the amount of vapour is gradually diminished, commencing from the appearance when the tube is full of the vapour of sodium, part of it condensing in the cooler portion of the tube, and some being carried out by the slow current of hydrogen. During this stage, although the lower part of the tube is at a white heat, as long as the cool current of hydrogen displaced metallic vapour, on looking down the tube it appeared perfectly dark. The first appearance of luminosity is of a purple tint, and, with the spectroscope, appears as a faint blue band, commencing with a wave-length of about 4,500, and fading away into the violet. Next appears a narrow band in the green, with a maximum of light, with a wave-length of about 5,420, diminishing in brightness so rapidly on either side as to appear like a bright line. This green band gradually widens, and is then seen to be divided by a dark band with a wave-length of about 5,510. Red light next appears, and

between the red and green light is an enormous extension of the D absorption line, while a still broader dark space intervenes between the green and the blue light. The dark line in the green (wave-length about 5,510) now becomes more sharply defined. This line appears to have been observed by Roscoe and Schuster, and regarded by them as coinciding with the double sodium line next in strength to the D lines, but it is considerably more refrangible than that double line. In the next stage, the channelled-space spectrum comes out in the dark space between the green and blue, and finally in the red. Gradually the light extends, the channels disappear, the D line absorption narrows, but still the dark line in the green is plainly discernible. Lastly, there is only D lines absorption.

The method of observation described may be used to observe emission as well as absorption spectra; for if the closed end of the tube be placed against the bars of the furnace so as to be relatively cooler than the middle of the tube, the light emitted by the vapours in the hottest part is more intense than that emitted by the bottom of the tube. This succeeds admirably with sodium.

The volatility of rubidium and cæsium rendered it advisable to try the effects first in glass tubes. For this purpose a piece of combustion tubing had one end drawn out and the end turned up sharply, and sealed off (like an ill-made combustion tube of the usual form), so as to produce an approximately plane face at the end of the tube; a small bulb was then blown at about an inch from the end, and the tube drawn out at about an inch from the bulb on the other side, so as to form a long narrower tube. Some dry rubidium or cæsium chloride was introduced into the bulb, and a fragment of fresh cut sodium, and the narrow part of the tube turned up, so as to allow the tube and bulb to be seen through in the direction of the axis of the tube. The shape of tube is given in Plate I. The open end was then attached to a Sprengel pump, and the air exhausted; the sodium was then melted, and afterwards either dry hydrogen or dry nitrogen admitted, and the end of the tube sealed off at nearly the atmospheric pressure. It is necessary to have this pressure of gas inside the tube, otherwise the metal distilled so fast on heating that the ends were speedily obscured by condensed drops of metal. Through these tubes placed lengthways in front of a spectroscope, a lime light was viewed. On warming the bulb of a tube in which rubidium chloride had been sealed up with sodium, the D lines were of course very soon seen; and very soon there appeared two dark lines near the extremity of the violet light, which, on measurement, were found to be identical in position with the well-known violet lines of rubidium. Next appeared faintly the channelled spectrum of sodium in the green, and then a dark line in the blue, very sharp and decided, in the place of the more refrangible of the characteristic lines of cæsium in the flame spectrum. As the temperature rose, these dark lines, especially those in the violet, became sensibly broader; and then another fine dark line appeared in the blue, in the

place of the less refrangible of the cæsium blue lines. During this time no dark line could be observed in the red; but as the temperature rose, a broad absorption band appeared in the red, with its centre about midway between B and C, ill defined at the edges, and though plainly visible not very dark. The lines in the violet had now become so broad as to touch each other and form one dark band. On cooling, the absorption band in the red became gradually lighter without becoming defined, and was finally overpowered by the channelled spectrum of sodium in that region. The double dark line in the violet became sharply defined again as the temperature fell. There are two blue lines in the spectrum of rubidium taken with an induction-coil very near the two blue lines of cæsium; but they are comparatively feeble, and the two dark lines in the blue observed in the places of the characteristic blue lines of cæsium must have been due to a small quantity of cæsium chloride in the sample of rubidium chloride. These blue lines were not, however, visible when some of the rubidium chloride was held in the flame of a Bunsen's burner, nor when a spark was taken from a solution of the chloride; but the more refrangible of them ($Cs\alpha$) was visible in the spark of an induction-coil, without a Leyden jar, taken between beads of the rubidium chloride fused on platinum wires.

When a tube containing cæsium chloride and sodium was observed, in the same way, the two dark lines in the blue were seen very soon after the heating began, and the more refrangible of them broadened out very sensibly as the temperature increased. The usual channelled spectrum of sodium was seen in the green, and an additional channelling appeared in the yellow, which may be due to cæsium or to the mixture of the two metals. Indeed the cæsium chloride was not free from rubidium, and the dark lines of rubidium were distinctly seen in the violet. Metallic lithium acts on the chlorides of cæsium and rubidium, giving the same results as sodium.

It is remarkable that these absorption lines of cæsium coincide with the blue lines of cæsium as seen in the flame, not with the green line which that metal shows when heated in an electric spark of high density. It is to be observed, however, that when sparks from an induction-coil without a jar are taken between beads of cæsium chloride fused on platinum wires, a spectrum similar to the flame spectrum is seen, and it is only when a Leyden jar is used that the spectrum is reduced to a green line. In like manner both the violet lines of rubidium are reversed, and both these violet lines are seen when the spark of an induction-coil, without jar, is passed between beads of rubidium chloride fused on platinum wire, though only one of them appears when a Leyden jar is used.

Mixtures of carbonate of cæsium with carbon, and of carbonate of rubidium with carbon, prepared by charring the tartrates, heated in narrow porcelain tubes, placed vertically in a furnace, gave sharp results. A small quantity of the cæsium mixture, introduced into a tube at a bright red heat, showed instantly the two blue lines reversed

and so much expanded as to be almost in contact. The width of the dark lines decreased as the cæsium evaporated, but they remained quite distinct for a long time. A similar effect was produced by the rubidium mixture, only it was necessary to have the tube very much hotter, in order to get enough of violet light to see the reversal of the rubidium lines. In this case the two lines were so much expanded as to form one broad dark band, which gradually resolved itself into two as the rubidium evaporated. The reversal of these lines of cæsium and rubidium seems to take place almost or quite as readily as that of the D lines by sodium, and the vapours of those metals must be extremely opaque to the light of the refrangibility absorbed, for the absorption was conspicuous when only very minute quantities of the metals were present. The red, yellow, and green parts of the spectrum were carefully searched for absorption lines, but none due to cæsium or rubidium could be detected in any case. It is perhaps worthy of remark that the liberation of such extremely electro-positive elements as cæsium and rubidium from their chlorides by sodium and by lithium, though it is probably only partial, is a proof, if proof were wanting, that so-called chemical affinity only takes a part in determining the grouping of the elements in such mixtures; and it is probable that the equilibrium arrived at in any such case is a dynamical or mobile equilibrium, continually varying with change of temperature.

It is difficult to prevent the oxidation of magnesium in the iron tubes, and tubes wider than half an inch did not give satisfactory results. With half-inch tubes, the lines in the green were clearly and sharply reversed, also some dark lines, not measured, were seen in the blue. The sharpness of these lines depended on the regulation of the hydrogen current, by which the upper stratum of vapour could be cooled at will.

(1) The absorption spectrum of magnesium consists of two sharp lines in the green, of which one, which is broader than the other, and appears to broaden as the temperature increases, coincides in position with the least refrangible of the *b* group, while the other is less refrangible, and has a wave-length very nearly 5,210. These lines are the first and the last to be seen, and were first taken for the extreme lines of the *b* group.

(2) A dark line in the blue, always more or less broad, difficult to measure exactly, but very near the place of the brightest blue line of magnesium. This line was not always visible, indeed rarely when magnesium alone was placed in the tube. It was better seen when a small quantity of potassium or sodium was added. The measure of the less refrangible edge of this band gave a wave-length of very nearly 4,615.

(3) A third line or band in the green rather more refrangible than the *b* group. This is best seen when potassium and magnesium are introduced into the tube, but it may also be seen with sodium and magnesium. The less refrangible edge of this band is sharply

defined, and has a wave-length about 5,140, and it fades away towards the blue.

These absorptions are all seen both when potassium and sodium are used along with mgnesium, and may be fairly ascribed to magnesium, or to magnesium together with hydrogen.

But besides these, other absorptions are seen which appear to be due to mixed vapours.

(4) Thus when sodium and magnesium are used together a dark line, with ill-defined edges, is seen in the green, with a wave-length about 5,300. This is the characteristic absorption of the mixed vapours of sodium and magnesium; it is not seen with either vapour separately, nor is it seen when potassium is used instead of sodium.

(5) When potassium and magnesium are used together, a pair of dark lines are seen in the red. The less refrangible of these sometimes broadens into a band with ill-defined edges, and has a mean wave-length of about 6,580. The other is always a fine sharp line, with a wave-length about 6,475. These lines are as regularly seen with the mixture of potassium and magnesium as the above-mentioned line (5,300) is seen with the mixture of sodium and magnesium, but are not seen except with that mixture.

There is a certain resemblance between the absorptions above ascribed to magnesium, and the emission spectrum seen when the sparks of a small induction-coil, without Leyden jar, are taken between electrodes of magnesium.

The coincidences of the series of the solar spectrum hitherto observed have, for the most part, been with lines given by dense electric sparks; while it is not improbable that the conditions of temperature, and the admixtures of vapours in the upper part of the solar atmosphere, may resemble much more nearly those in our tubes.

It became a question of interest to find the conditions under which the same mixtures would give luminous spectra, consisting of the lines which had been seen reversed. On observing sparks from an induction-coil taken between magnesium points in an atmosphere of hydrogen, a bright line regularly appeared, with a wave-length about 5,210, in the same position as one of the most conspicuous of the dark lines observed to be produced by vapour of magnesium with hydrogen in our iron tubes. This line is best seen, i. e. is most steady, when no Leyden jar is used, and the rheotome is screwed back, so that it will but just work. It may, however, be seen when the coil is in its ordinary state, and when a small Leyden jar is interposed; but it disappears (except in flashes) when a larger Leyden jar is used, if the hydrogen be at the atmospheric pressure. This line does not usually extend across the whole interval between the electrodes, and is sometimes only seen near the negative electrode. Its presence seems to depend on the temperature, as it is not seen continuously when a large Leyden jar is employed, until the pressure of the hydrogen and its resistance is very much reduced. When well-dried nitrogen or carbonic oxide is substituted for hydrogen, this line disappears en-

tirely; but if any hydrogen or traces of moisture be present it comes out when the pressure is much reduced. In such cases the hydrogen lines C and F are always visible as well. Sometimes several fine lines appear on the more refrangible side of this line, between it and the *b* group, which give it the appearance of being a narrow band, shaded on that side. Various samples of magnesium used as electrodes, and hydrogens prepared and purified in different ways, gave the same results.

In addition to the above-mentioned line, there is also produced a series of fine lines, commencing close to the most refrangible line of the *b* group, and extending with gradually diminishing intensity towards the blue. These lines are so close to one another, that in a small spectroscope they appear like a broad shaded band. We have little doubt that the dark absorption line, with wave-length about 5,140, shading towards the blue, observed in our iron tubes, was a reversal of part of these lines, though the latter extend much further towards the blue than the observed absorption extends.

Charred cream of tartar in iron tubes, arranged as before, gave a broad absorption band extending over the space from about wave-length 5,700 to 5,775, and in some cases still wider, with edges ill-defined, especially the more refrangible edge. By placing the charred cream of tartar in the tube before it was introduced into the furnace, and watching the increase of light as the tube got hot, this band was at first seen bright on a less bright background, it gradually faded, and then came out again reversed, and remained so. No very high temperature was required for this, but a rise of temperature had the effect of widening the band. Besides this absorption, there appeared a very indefinite faint absorption in the red, with the centre at a wave-length of about 6,100, and a dark band, with a tolerably well-defined edge on the less refrangible side, at about a wave-length of 4,850, shading away towards the violet. A fainter dark band was sometimes seen beyond, with a wave-length of about 4,645; but sometimes the light seemed abruptly terminated at about wave-length 4,850. It will be noticed that these absorptions are not the same as those seen when potassium is heated in hydrogen, nor do they correspond with known emission lines of potassium, although the first, which is also the most conspicuous and regularly visible of these absorptions, is very near a group of three bright lines of potassium. It seemed probable that they might be due to a combination of potassium with carbonic oxide. Potassium heated in carbonic oxide in glass tubes, united readily with the gas, but the compound did not appear to volatilize at a dull red heat, and no absorption, not even that which potassium gives when heated in nitrogen under similar circumstances, could be seen. Induction sparks between an electrode of potassium and one of platinum in an atmosphere of carbonic oxide, gave the usual bright lines of potassium, and also a bright band, identical in position with the above-

* With greater dispersion this line is seen as the sharp edge of a series of very fine lines shading off towards the blue like the ordinary hydrocarbon spectrum.

mentioned band, between wave-lengths about 5,700 and 5,775. This band could not be seen when hydrogen was substituted for carbonic oxide. A mixture of sodium carbonate and charred sugar, heated in an iron tube, gave only the same absorption as sodium in hydrogen. There were also no indications of any absorption due to a compound of rubidium or of cæsium with carbonic oxide.

A mixture of barium carbonate, aluminium filings, and lamp-black, heated in a porcelain tube, gave two absorption lines in the green, corresponding in position to bright lines seen when sparks are taken from a solution of barium chloride, at wave-lengths 5,242 and 5,136, marked α and β by Lecoq de Boisbaudran. These two absorptions were very persistent, and were produced on several occasions. A third absorption line, corresponding to line δ of Boisbaudran, was sometimes seen; and on one occasion, when the temperature was as high as could be obtained in the furnace fed with Welsh coal, and a mixture of charred barium tartrate with aluminium was used, a fourth dark line was seen with wave-length 5,535. This line was very fine and sharply defined, whereas the other three lines were ill-defined at the edges; it is, moreover, the only one of the four which corresponds to a bright line of metallic barium.

Repeated experiments with charred tartrates of calcium and of strontium mixed with aluminium gave no results; but on one occasion, when sodium carbonate was used along with the charred tartrate of strontium and aluminium, the blue line of strontium was seen reversed: and on another occasion, when a mixture of charred potassium, calcium, and strontium tartrates, and aluminium was used, the calcium line, with wave-length 4,226, was seen reversed.

In order to command higher temperatures, experiments were made with the electric arc enclosed in lime, magnesia, or carbon crucibles. The different forms used are represented in Plate II. Figs. 1, 2, 3, 4, and 5; and the plan for projecting reversals in Plate III.

In the first experiments thirty cells of Grove were employed; in the later ones the Siemens arc from the powerful dynamo-machine belonging to the Royal Institution.

The electric arc in lime crucibles gives a very brilliant spectrum of bright lines, a copious stream of vapours ascending the tube. On drawing apart the poles, which could be done for nearly an inch without stopping the current, the calcium line with wave-length 4,226 almost always appears more or less expanded with a dark line in the middle, both in the lime crucibles and in carbon crucibles into which some lime has been introduced; the remaining bright lines of calcium are also frequently seen in the like condition, but sometimes the dark line appears in the middle of K (the more refrangible of Fraunhofer's lines H), when there is none in the middle of H. On throwing some aluminium filings into the crucible, the line 4,226 appears as a broad dark band, and both H and K, as well as the two aluminium lines between them, appear for a second as dark bands on a continuous background. Soon they appear as bright bands with dark middles;

gradually the dark line disappears from H, and afterwards from K, while the aluminium lines remain with dark middles for a long time. When a mixture of lime and potassium carbonate was introduced into a carbon crucible, the group of three lines with wave-lengths 4,425, 4,434, and 4,454 were all reversed, the least refrangible being the most strongly reversed, and remaining so longest, while the most refrangible was least strongly reversed, and for the shortest time.

When aluminium was put into the crucible, only the two lines of that metal between H and K were seen reversed. The lines at the red end remained steadily bright.

When magnesium was put into a lime crucible, the *b* group expanded a little without appearing reversed, but when some aluminium was added, the least refrangible of the three lines appeared with a dark middle, and on adding more magnesium the second line put on the same appearance; and lastly, the most refrangible was reversed in like manner. The least refrangible of the three remained reversed for some time; and the order of reversibility of the group is that of refrangibility. Of the other magnesium lines, that in the yellowish-green (wave-length 5,527) was much expanded, while the blue line (wave-length 4,703), and a line still more refrangible than the hitherto recorded lines, with wave-length 4,354, were still more expanded each time that magnesium was added.

The following experiments were made in carbon crucibles :—

With strontia the lines with wave-lengths 4,607, 4,215 and 4,079 were all seen with dark lines in the middle, but no reversal of any strontium line less refrangible could be seen.

A mixture of barium and potassium carbonates produced the reversal of the lines with wave-lengths 5,535 and 4,933. When barium chlorate was dropped into a crucible, the four lines with wave-lengths 4,553, 4,933, 5,545, and 5,518 were reversed.

To observe particularly the effects of potassium a mixture of lime and potassium carbonate previously ignited was thrown in. The violet lines of potassium, wave-length 4,044, came out immediately as a broad black band, which soon resolved into *two* narrower dark bands having wave-lengths nearly 4,042 and 4,045. On turning to the red end the two extreme red lines were both seen reversed. No lines of potassium between the two extremes could be seen reversed, but the group of three yellow lines were all expanded, though not nebulous, and other lines in the green were seen much expanded.

Sodium carbonate gave only the D lines reversed, though the other lines were expanded, and the pairs in the green had each become a very broad nebulous band, and D almost as broad a black band. When sodium chlorate was dropped into a crucible, the pair of lines with wave-lengths 5,681, 5,687, were both momentarily reversed, the latter much more strongly than the former.

When a very little charred rubidium tartrate was put in, the two violet lines were sharply reversed, appearing only as black lines on a continuous light background. Turning to the red end, the more

refrangible of the two lines in the extreme red (wave-length 7,800) was seen to have a decided dark line in the middle, and it continued so for some time. The addition of more rubidium failed to cause any reversal of the extreme red line, or of any but the three lines already mentioned.

On putting lithium carbonate into the crucible, the violet line of lithium appeared as a nebulous band, and on adding some aluminium this violet band became enormously expanded, but showed no reversal. The blue lithium line (wave-length 4,604) was well reversed, as was also the red line, while a fine dark line passed through the middle of the orange line. On adding a mixture of aluminium filings and the carbonates of lithium and potassium, the red line became a broad black band, and the orange line was well reversed. The green line was exceedingly bright, but not nebulous or reversed, and the violet line still remained much expanded, but unreversed.

Metallic indium placed in the crucible gave the lines with wave-lengths 4,101 and 4,509, and both were seen strongly reversed. No other absorption line of indium could be detected.

In some cases a current of hydrogen or of coal-gas was introduced into the crucibles by means of a small lateral opening, or by a perforation through one of the carbon electrodes, as is shown in Plate II. Fig. 4; sometimes the perforated carbon was placed vertically, and we examined the light through the perforations. When no such current of gas is introduced, there is frequently a flame of carbonic oxide burning at the mouth of the tube. The current of hydrogen produces very marked effects. As a rule, it increases the brilliance of the continuous spectrum, and diminishes relatively the apparent intensity of the bright lines, or makes them altogether disappear with the exception of the carbon lines. When this last is the case, the reversed lines are seen simply as black lines on a continuous background. The calcium line with wave-length 4,226 is always seen under these circumstances as a more or less broad black band on a continuous background, and when the temperature of the crucible has risen sufficiently, the lines with wave-lengths 4,434 and 4,454, and next that with wave-length 4,425, appear as simple black lines. So, too, do the blue and red lines of lithium, and the barium line of wave-length 5,535, appear steadily as sharp black lines, when no trace of the other lines of these metals, either dark or bright, can be detected. Dark bands also frequently appear, with ill-defined edges, in the positions of the well-known bright green and orange bands of lime.

With sodium chloride, the pair of lines (5,687, 5,681) next more refrangible than the D group were repeatedly reversed. In every case the less refrangible of the two was the first to be seen reversed, and was the more strongly reversed, as has also been observed by Mr. Lockyer. But our observations on this pair of lines differ from his in so far as he says that "the double green line of sodium shows scarcely any trace of absorption when the lines are visible," while we have repeatedly seen the reversal as dark lines appearing on the

expanded bright lines; a second pair of faint bright lines, like ghosts of the first, usually coming out at the same time on the more refrangible side.

Potassium carbonate gave, besides the violet and red lines which had been reversed before, the group, wave-lengths 5,831, 5,802, and 5,872, all reversed, the middle line of the three being the first to show reversal. Also the lines wave-lengths 6,913, 6,946, well reversed, the less refrangible remaining reversed the longer. Also the group, wave-lengths 5,353, 5,338, 5,319 reversed, the most refrangible not being reversed until after the others. Also the line wave-length 5,112 reversed, while two other lines of this group, wave-lengths 5,095 and 5,081, were not seen reversed.

Using lithium chloride, not only were the red and blue lines, as usual, easily reversed, and the orange line well reversed for a long time, but also the green line was distinctly reversed; the violet line still unreversed, though broad and expanded. Had this green line belonged to cæsium, the two blue lines of that metal which are so easily reversed could not have failed to appear; but there was no trace of them.

In the case of rubidium, the less refrangible of the red lines was well reversed as a black line on a continuous background, but it is not easy to get, even from the arc in one of our crucibles, sufficient light in the low red to show the reversal of the extreme ray of this metal.

With charred barium tartrate, and also with baryta and aluminium together, the reversal of the line with wave-length 6,496 was observed, in addition to the reversals previously described. The less refrangible line, wave-length 6,677, was not reversed.

With charred strontium tartrate, the lines with wave-lengths 4,812, 4,831, and 4,873, were reversed, and by the addition of aluminium, the line wave-length 4,962 was reversed for a long time, and also the lines wave-lengths 4,895, 4,868.

On putting calcium chloride into the crucible, the line wave-length 4,302 was reversed, this being the only one of the well-marked group to which it belongs which appeared reversed. On another occasion, when charred strontium tartrate was used, the line wave-length 4,877 was seen reversed, as well as the strontium line near it. The lines wave-lengths 6,161, 6,121, have been seen momentarily reversed.

With magnesium, when a stream of hydrogen or of coal-gas was led into the crucible, the line wave-length 5,210, previously seen in iron tubes, and ascribed to a combination of magnesium with hydrogen, was regularly seen, usually as a dark line, sometimes with a tail of fine dark lines on the more refrangible side similar to the tail of bright lines seen in the sparks taken in hydrogen between magnesium points. Sometimes, however, this line (5,210) was seen bright. It always disappeared when the gas was discontinued, and appeared again sharply on readmitting hydrogen. These effects were

however, only well defined in crucibles having a height of at least 3 inches above the arc.

On putting a fragment of metallic gallium into a crucible, the less refrangible line, wave-length 4,170, came out bright, and soon a dark line appeared in the middle of it. The other line, wave-length 4,031, showed the same effect, but less strongly.

Reviewing the series of reversals which have been observed, in many cases the least refrangible of binary groups is the most easily reversed, as has been previously remarked by Cornu.

Making a general summation of the results respecting the alkaline earth metals, potassium and sodium, having regard only to the most characteristic rays, which for barium may be taken as 21, for strontium 34, for calcium 37, for potassium 31, and for sodium 12, the reversals number respectively 6, 10, 11, 13, and 4. That is in the case of the alkaline earth metals about one-third, and these chiefly in the more refrangible third of the visible spectrum, the characteristic rays remaining unreversed in the more refrangible part of the spectrum being respectively 2, 5, and 4.

The curious behaviour of the lines of different spectra with regard to reversal induced a comparison with the bright lines of the chromosphere of the sun, as observed by Young. It is well known that some of the principal lines of the metals giving comparatively simple spectra, such as lithium, aluminium, strontium, and potassium, are not represented amongst the dark lines of Fraunhofer, while other lines of those metals are seen: and an examination of the bright chromospheric lines shows that special rays highly characteristic of bodies which appear from other rays to be present in the chromosphere are absent, or are less frequent in their occurrence than others.

In the following table the relation between the observations on reversals and Young's on the chromospheric lines is shown.

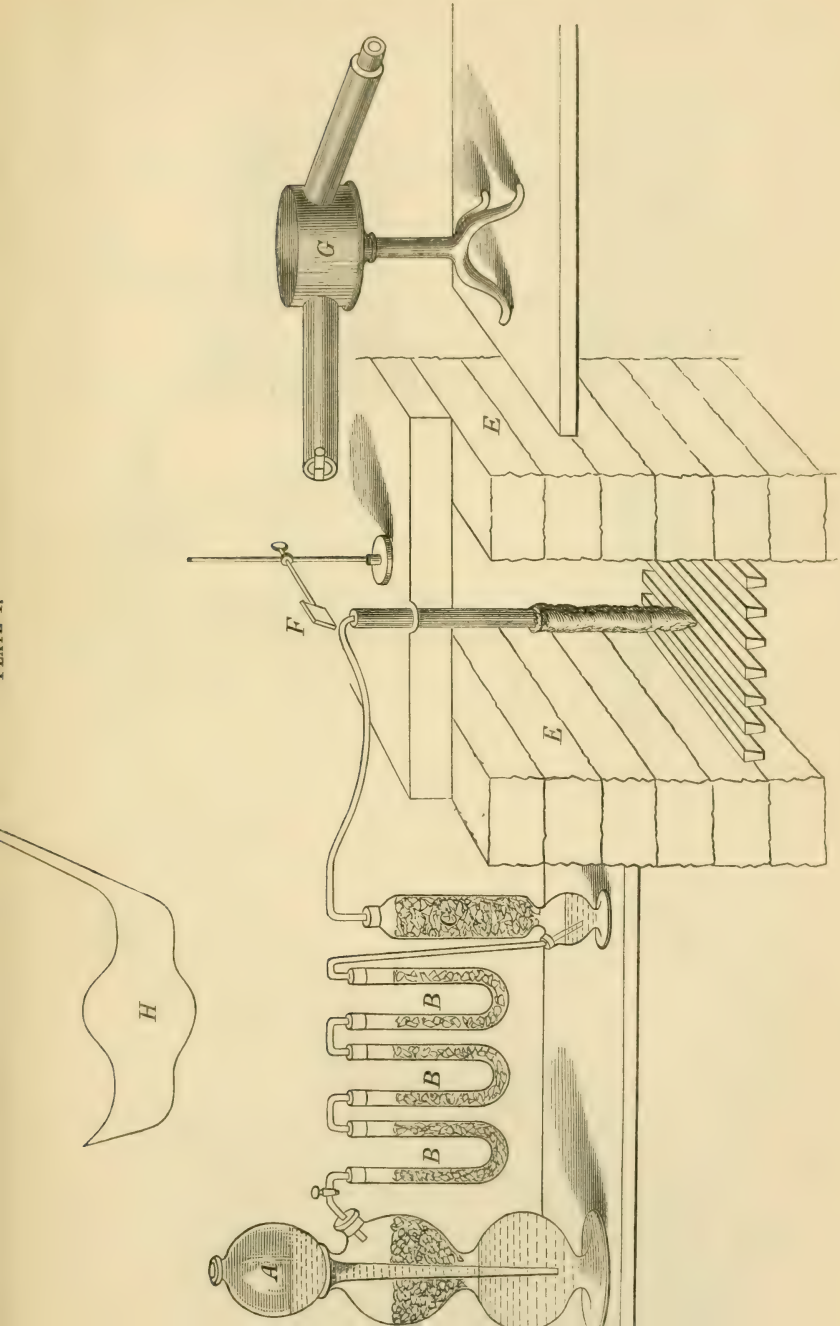
Lines in Wave-Lengths.	Frequency in Chromosphere.	Behaviour. Reversal in our Tubes.	Remarks.
Sodium .. 6,160 / 6,154	0	Expanded.	
D	50	Most easy	Principal ray.
5,687 / 5,681	2	Difficultly reversed.	
5,155 / 5,152	2	Very diffused.	
4,983 / 4,982	0	" "	
Lithium .. 6,705	0	Readily reversed ..	Most characteristic, at low temperature and low density.
6,101	3	Difficultly reversed.	
4,972	0	" "	
4,603	0	Readily reversed.	
4,130	0	Very diffused ..	Described by Boisbaudran.

Lines in Wave-Lengths.		Frequency in Chromosphere.	Behaviour. Reversal in our Tubes.	Remarks.
Magnesium	5,527	40	Expanded.	
b_1	5,183	50	Reversed	Most characteristic.
b_2	5,172	50	,, ...	Most characteristic.
b_4	5,167	30	Difficultly reversed	Most characteristic.
	4,703	0	Much expanded.	
?	4,586	0	,, ,, ..	Doubtful whether due to magnesium.
	4,481	0	Not seen either bright or reversed.	Characteristic of spark absent in arc.
Barium ..	6,677	25	0	May be either Ba or Sr.
	6,496	18	Reversed.	,, ,, ,,
	6,140	25	0	
	5,534	50	Readily reversed ..	Most persistent.
	5,518	15	Reversed.	
	4,933	30	,,	Well-marked ray.
	4,899	30	0	
	4,553	10	Pretty readily reversed.	
Strontium	6,677	25	0	May be Sr or Ba.
	6,496	18	0	,, ,, ,,
	4,902	..	Reversed.	
	4,895	..	Reversed.	
	4,873	..	Reversed.	
	4,868	..	Reversed.	
	4,812	..	Reversed.	
	4,831	..	Reversed.	
	4,607	0	Readily and strongly reversed.	Most characteristic.
	4,215	40	Readily reversed ..	Well marked.
	4,077	25	,, ,, ..	,, ,,
Calcium ..	6,161	8	Reversed difficultly	Very bright.
	6,121	5	,, ,,	
	5,587	2	Doubtful reversal.	
	5,188	10	Reversed.	
	4,877			
	4,587	2	0	
	4,576	4	0	
	4,453	0	Readily reversed.	
	4,435	1	,, ,,	
	4,425	2	,, ,,	
	4,302			
	4,226	3	Most easily reversed	Very characteristic.
	4,095 (?)	0	Strongly reversed.	
	3,968	75	Well reversed.	
	3,933	50	Rather more readily than the last.	
Aluminium	6,245	8	0	Strong lines
	6,237	8	0	
	3,961	0	Strongly reversed ..	Very marked.
	3,943	0	Strongly reversed ..	Very marked.

Lines in Wave-lengths.		Frequency in Chromosphere.	Behaviour. Reversal in our Tubes.	Remarks.
Potassium	7,670 } 7,700 }	0	Strongly reversed ..	Chief rays.
	6,946 } 6,913 }	..	Reversed.	
	5,872 } 5,831 } 5,802 }	..	”	
	5,353 } 5,338 } 5,319 }	..	”	
	5,112	..	”	
	4,044 } 4,042 }	3	.. ..	Well marked.
Cæsium ..	5,990	10	0	
	4,555	10	Strongly reversed ..	Most marked.

The group calcium, barium, and strontium on the one hand, and sodium, lithium, magnesium, and hydrogen, on the other, seem to behave in a similar way in the chromosphere of the sun; but before definite conclusions can be reached regarding the sequence of the reversals, a further series of long and laborious experiments must be executed.

[J. D.]


A
B
B
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C
E
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F
G
H

PLATE II.

FIG. 1

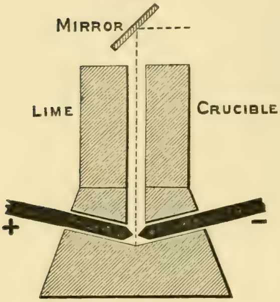
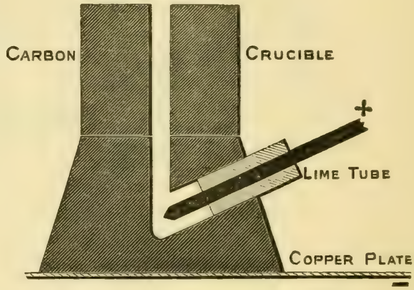


FIG. 2



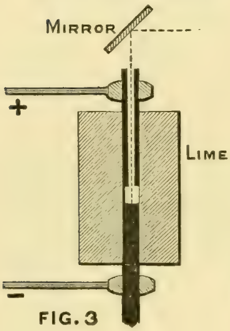


FIG. 3

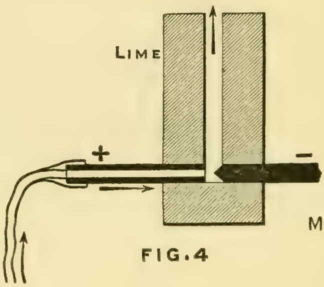


FIG. 4

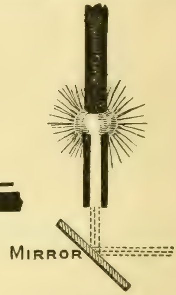
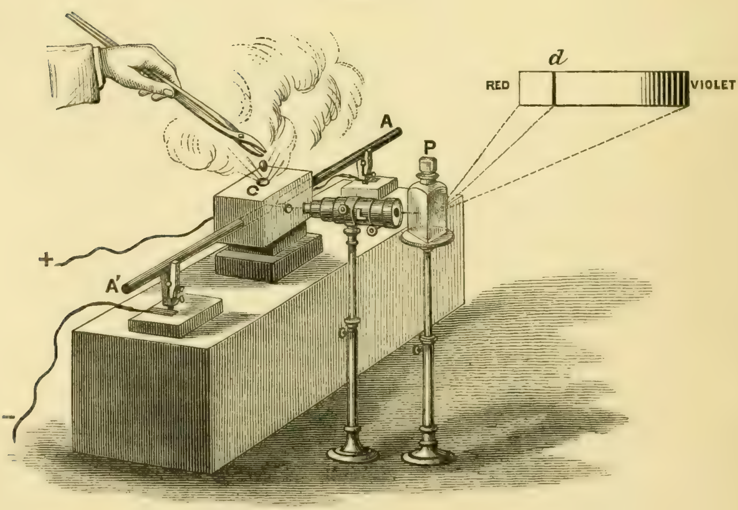


FIG. 5

PLATE III.



WEEKLY EVENING MEETING,

Friday, June 13, 1879.

WARREN DE LA RUE, Esq. M.A. D.C.L. F.R.S. Secretary and Vice-President, in the Chair.

F. J. BRAMWELL, Esq. F.R.S. M. Inst.C.E. *M.R.I.*

PAST-PRESIDENT OF THE INSTITUTE OF MECHANICAL ENGINEERS.

The 'Thunderer' Gun Explosion.

THE explosion of the gun on board the 'Thunderer'—such, you will see, on referring to the notice paper, is the subject of my lecture to-night, and I commence by reading that title to you to introduce the statement I desire to make, that the subject of this lecture is a very limited one. It is not artillery generally, nor "big guns" generally, nor muzzle-loaders compared with breech-loaders, nor the consideration of what is the best material to employ in the construction of big guns; and it is not any or all of these, for one very obvious reason. The least of the subjects I have mentioned would require an evening to itself to deal with it in the merest outline, while those of more importance would each demand a long course of lectures. My subject is, as I have said, the limited one of the "Explosion of the gun on board the 'Thunderer';" but limited as it is, I feel the difficulty of bringing all that I think should be laid before you within the compass of the hour allotted to me.

I am aware the remark may be made, What more can you tell us than we can discover for ourselves by reading the report of the Committee? and I am also aware it is just possible there may be some who will urge that the event is now five months old, and the interest in it has therefore died out. These, however, were not the feelings of the Managers of the Institution (of whom, let me say, I was not one at the time it was suggested I should deliver this lecture); they do not believe, and I do not believe, that the interest which attached to the subject has died out, and that the nation is no longer concerned in the investigation of an occurrence attended with such grievous results to so many of the faithful servants of our Queen, and the cause of a deeply rooted apprehension that the armaments in which we trust are unworthy of that trust, and might not only fail us in the hour of need by refusing to act against the enemy, but worse still, might side with that enemy by inflicting wounds and death upon ourselves. Further, Parliamentary papers have not an inviting appearance, and I think it not improbable that but few of those who

honour me with their presence to-night are in the habit of reading such documents, or if they are, that they find them, in the absence of models and of familiar explanation, readily intelligible. Bearing all these considerations in mind, it appeared to me that a plain, untechnical account of the investigation into the causes of the explosion by one who took part in it from first to last, more especially if that account were aided by diagrams and models, would not be uninteresting to the members of the Royal Institution, and might not be without its use in assisting to dissipate any feeling of doubt as to the safety of our artillery which still lingered in the minds of some who knew that a gun had exploded on board the 'Thunderer,' but who did not know under what circumstances that explosion had occurred.

Let me here say, although I think it barely necessary to do so, that having been placed by the Admiralty in the position of Assessor to the Committee which made the investigation, I should never have thought for one moment (notwithstanding that our report was in the hands of the public) of giving this lecture unless I had ascertained that my doing so would meet with the full approval, as I am happy to say it does, of the authorities of the Admiralty.

I will now ask you to direct your attention to Diagram 1, representing a longitudinal section, of the 38-ton gun which exploded. This gun you will see was composed of six pieces, namely, the steel tube 12 inches in bore, and 16 feet 6 inches long internally, upon which at the rear end is shrunk the wrought-iron coiled breech piece; the 1 B coil shrunk on to the tube in advance of the breech piece; the B tube shrunk on to the steel tube in front of all, and forming the chase or muzzle of the gun; the C coil shrunk outside the breech piece; and the cascable screw in rear of all. The trunnions by which the gun is supported on its carriage are forged in one with the C coil. It will be observed that the ends of the different parts overlap, and that at these overlapping places there are appropriate recesses, and projections to hook into those recesses, by which endway connection is made among the various parts. Similarly, there are certain projections on the exterior of the steel tube itself, which prevent its endway motion. The tube is made from a solid steel forged ingot, which is bored out to nearly the desired size, and after having been toughened in oil (and about this toughening I shall have something to say presently), has shrunk upon it the breech piece and 1 B coil and the B tube in succession.

You will observe I frequently use the word "coil" when speaking of the envelopes by which the tube is surrounded. They have this name because they are made by winding bars of wrought iron of a suitable transverse section round and round until they are made into gigantic ringlets, ringlets very closely twisted, as you will see on reference to Diagram 2. This represents a coil lying on its side; it is turned up on end and placed in a furnace to be heated. The doorway of this furnace, I may say in passing, is large enough to be traversed by a full-sized coach. Here the heating is continued until the whole

body of the iron is raised to the full welding point, and then the door being opened a pair of tongs nearly 60 feet long and weighing in themselves as much as 22 tons, grasp the coil, withdraw it from the furnace, and place it on the anvil of a 40-ton steam hammer which, beating on the top of the coil, welds each convolution to its neighbour, and makes that which was before but a ringlet, into a tube. It will be seen from the foregoing description that the fibre of the wrought bar goes round about the gun. After the forging, the coils are turned in a lathe and are bored to such dimension as will not quite allow of their being placed when they are cold outside the steel tube, but on being suitably heated they expand sufficiently to admit of the tube being inserted within them, and then in cooling they shrink upon the tube and embrace it firmly. Similarly the C coil is shrunk upon the breech piece.

This making of the rear end of the gun in two layers outside the steel tube by shrinking the C coil upon the breech piece, is not done with the mere object of reducing the weights of the pieces to be handled, but with the further and more important object of obtaining greater assistance from the metal in the gun to resist explosion. I regret very much that time will not permit of our investigating this interesting subject; I will content myself by saying that if there were no countervailing considerations, the gun which would give the greatest strength for the weight of metal employed in it would be one made up of a large number of very thin coils shrunk one upon another. To put it popularly, if the outside coil, the C coil, is in a state of tension owing to shrinkage, then it is on its guard and is prepared to assist the breech piece and the tubes within it in resisting the strain produced by the explosion; while if the C coil were not in that state of tension, the tube and the breech piece might be split by the pressure before the strength of the outer coil was brought to their aid. You will see from the diagram that the extreme diameter of the gun is as much as $57\frac{1}{2}$ inches; while its extreme length is as much as 19 feet 2 inches; its weight, as we all know, is 38 tons; and, as I have said, the bore of this gun is 12 inches, which is the bore also of its fellow, the bore of the other 38-ton guns in the service being as much as $12\frac{1}{2}$ inches.

You are all aware that the modern projectile is not a sphere, but a cylinder with a pointed end, and that these projectiles are from $2\frac{1}{2}$ to 3 diameters in length. The cannon ball is a thing of the past. You are also probably aware that it would be impossible to shoot cylindrical projectiles of these proportions for even a short distance without the risk of their turning sideways and of going anywhere but where they were wanted to go, unless a rotary motion were given by the rifling. You will observe in Diagram 1 the rifle grooves are shown commencing at $27\frac{1}{2}$ inches from the rear end of the bore and reaching to the muzzle. There are nine of these grooves, each of them $1\frac{1}{2}$ inch wide by $\frac{1}{5}$ of an inch deep.

Diagram 3 represents that which is called a "development" of

the interior of the bore, and shows the nature of the rifling. To account to yourselves for the appearance presented by this diagram, I will ask you to imagine that the bore of the gun had been lined with a paper tube on which the rifling has been drawn, and that then the tube had been removed from the gun and slit by a straight longitudinal cut extending from end to end, and opened out flat, and suspended on the wall before you. This would exhibit, as Diagram 3 does, the rifling round the tube when developed on to a flat surface. You will observe that the grooves, where they commence at the rear end, are parallel with the bore of the gun, but that they at once begin to depart from this parallelism, the inclination of departure gradually increasing to the very muzzle, at which point each groove is a portion of a helix of such an inclination as would make one complete turn round about the barrel, if that barrel were prolonged for 35 feet beyond the muzzle. It is technically called 1 turn in 35 calibres; and the calibre being in this instance exactly 1 foot, the inclination is equal, as I have said, to one turn in 35 feet. Assuming, therefore, that the projectile were moving with a velocity of say 1400 feet per second, which is 40 times 35 feet, that projectile would be spinning on its axis at the rate of 40 revolutions in a second.

This kind of rifling is called the increasing twist. Diagram 4 shows rifling of a uniform twist—that is to say, the departure from parallelism is as great at the very commencement of the rifling as at its termination.

In the gun under consideration, the vent through which the charge is ignited is at 12 inches forward of the rear end of the bore.

I need hardly tell you that all the materials used in our guns are most carefully tested. These tests are directed to ascertain not only that the metal will support a certain load, carefully and gradually applied to a specimen of a standard size, before rupture ensues, but also to ascertain that the metal is elastic and tough, and competent, therefore, to support shocks. The elasticity is judged of by the extension which a sample under strain will afford with a given load, such extension disappearing on the removal of that load. The toughness is judged of by the total extension occurring before rupture, and by the ability to support bending without fracture. I have already said that the steel tube is toughened in oil; this toughening, which is performed by heating the steel and then cooling it in oil, has a very marked effect in increasing the resistance to strain, and also in increasing the elastic limit. Speaking generally, when the steel is reduced to the size of the test sample before the toughening, a specimen which would require, when untoughened, 30 tons per square inch to break it, will, when toughened, require 45 tons, while the elastic limit, which in the untoughened state would be attained by a strain of about 15 tons per square inch, will in the toughened state not be reached under 30 tons to the square inch. Samples of iron and steel are on the table, and an examination will show how, before rupture,

they became diminished in sectional area as they were pulled out under the strain.

Having now described the *gun*, I will ask your attention to the subject of the *powder* and of the *projectiles*.

I do not propose to say anything about the chemistry of gunpowder, but I wish to make a few observations about the differences which arise in the use of gunpowder from variation in the size of the particles or grains, and to explain why it is that as the particles enlarge the combustion is slower, and as it may be said takes place by degrees.

With respect to this suggestion that powder may burn by degrees, I remember the amusement that was caused by Tom Hood's 'Report from Below,' where Mrs. Round, the washerwoman, has occasioned great alarm to Mr. and Mrs. B. by emptying the whole contents of Mr. B.'s powder-horn into the fireplace of the washing copper, in order to clear out the flue and stop it from smoking. Mrs. Round is rendered insensible, but the servant who escapes and rushes upstairs to give an account of the occurrence, does so in the following words :—

"As Mrs. Round and I, marm, was a-standing at our tubs,
And Mrs. Round was seconding what little things I rubs,
'Mary,' says she to me, 'I say,' and there she stops for coughin',
'That dratted copper flue has took to smoking very often;
But, please the pigs,' for that's her way of talking in a passion,
'I'll blow it up, and not be set a-coughin' in this fashion.'
So down she takes my master's horn (I mean his horn for loading),
And empties every grain alive for to set the flue exploding.
'Lawks! Mrs. Round,' says I, and stares, 'that quantum can't be proper;
I'm sartin sure it ne'er can take a pound to sky a copper.'
Them words sets up her back, so, with her hands upon her knees,
'Afore ever you was born,' says she, 'I was used to things like these;
I'll put it in the fire and let it burn up by degrees.'"

At the time this poem was written, everyone was prepared for the catastrophe which, as on reading it, you will find did occur; and even now, if one were to speak of powder burning up by degrees, it would excite a smile; and still better ground was there for amusement when the 'Report from Below' was written, for then the largest-grained cannon powder was but dust as compared with the cannon powder of the present day. I have before me samples of these powders, the very smallest of which, the R.L.G., or Rifle Large Grain, is of goodly dimensions, while the P or Pebble powder is, except for its colour, fit to form a gravel path, and the P^2 powder is, as you will see, composed of "grains"! which measure about $1\frac{1}{2}$ inch in each direction, and weigh some three to three and a half ounces.

It is tolerably easy to see why the combustion of powder in large "grains" should be slower than that of powder when the particles are small. Although powder contains within itself the elements necessary for its combustion, and does not need therefore the presence of air to burn it, nevertheless, as combustion does not occur until a

certain temperature is reached, and as that temperature when a light is applied to powder proceeds from the outside towards the centre of each grain, it is, as I have said, easy to see why, if a pound of powder be made into a single grain of about $3\frac{1}{8}$ inches cube, and heat be applied to the outside of such a grain so as to cause ignition to take place, more time will be required for the combustion of such a pound of powder, owing to the slowness with which the heat would travel from the outside to the centre of a grain of these dimensions as the powder burnt away, than would be required if the pound of powder were made into 16 grains of 1 inch cube each, and these were simultaneously heated on their exteriors to the temperature of ignition. And following this up, one understands readily how much more rapid would be the burning of the powder if the same weight, instead of being in the form of 16 cubes of an inch each, were in the state of thousands of small grains. In short, one can readily see why it is that the larger the particles, the slower the combustion.

I need hardly say that the expressions quick and slow are but comparative terms, and that even the slowest of gunpowders finishes its combustion in what is popularly called "no time." But "no time" though it be, the unassisted eye can readily detect the difference in the rate of burning due to variations in the size of the powder.

I will now ask Professor Abel, who, I am glad to say, is with us to-night, to show you by an experiment which he has kindly prepared, that there is this marked difference in the rate of combustion in powder of varying sizes.

I will next endeavour to explain in what way it is that the slower combustion, while equally efficacious in propelling the shot, acts less severely on the gun.

We will take it, that the object to be obtained by the explosion of powder behind a projectile in a cannon, is to cause that projectile to issue from the muzzle with a certain velocity. About a quarter of a mile, that is, 1320 feet in a second, is now-a-days a low velocity. I have already, when speaking of the rifling, suggested, by way of illustration a speed of 1400 feet a second; but, although it does not immediately concern this lecture, it may be of interest to remark, that by special arrangements as much as 2265 feet per second have with a projectile of 160 lbs. been obtained. In the present instance, to illustrate that which I wish to convey to you, I will again assume a speed of 1400 feet. Such a velocity would be produced in a falling body by a descent (through a vacuous space, so as to be unresisted by air) of about 30,000 feet, or $5\frac{3}{4}$ miles, and the stored-up energy in a projectile moving at 1400 feet would be equal to its weight multiplied into the height through which it must fall to attain the velocity, that is to say, in the case of the 1400 feet supposed, if the projectile weighed 5 cwt. or $\frac{1}{4}$ of a ton, then 30,000 feet multiplied by a $\frac{1}{4}$ would give 7500 foot tons as the stored-up energy. Now, if this velocity were got by the action of gravity, and if one be allowed to leave out of consideration atmospheric resistance and the

resistance of friction, it would be a matter of indifference as regards the ultimate velocity whether the shot fell perpendicularly 30,000 feet or slid down a (frictionless) slope of no matter how gradual an inclination, or whether it descended by a curved path, so long as it did, between the commencement and the end of its journey, traverse 30,000 feet measured perpendicularly.

To illustrate my meaning, I will ask your attention to Diagram 5. Here the straight vertical line, Fig. 1, represents 30,000 feet; if the projectile were to fall from A to B, it would at B attain a velocity of 1400 feet in a second. The inclined line, Fig. 2, represents a (frictionless) slope of an angle of 30°, but having the same vertical height of 30,000 feet. If the shot were to slide down this, its velocity at the bottom would still be 1400 feet a second; but it would have required double the time to attain this velocity, because it would during its passage through any unit of length in its descent have been subjected but to one-half of the downward impulse that it would have received when falling vertically. Fig. 3 shows a frictionless concave curve having a vertical height as before of 30,000 feet. Again the final velocity would be the 1400 feet a second; but the time occupied, while greater than that required for the vertical fall, would be less than that needed to pass down the slope, because, as will be seen, the early part of the downward journey is made nearly vertically, and thus the shot has already attained a high speed before it reaches the lower and flatter parts of the course where the downward impulse of gravity and the acceleration are but small. Fig. 4 shows a convex frictionless curve of the height of 30,000 feet. In this case also, when the shot had reached the bottom, the velocity would be the 1400 feet a second, but the time required would be more than in any of the preceding cases, because the first part of the journey is made upon a path which departs but gradually from the horizontal, and therefore the motion of the shot is but slow while traversing this first part.

A consideration of these four figures will show, that so long as a certain total impulse is applied, it is a matter of unimportance as regards the velocity when produced, whether that impulse be large and uniform and needing therefore to act but through a small space and for a short time as in Fig. 1, whether it be less and uniform, and needing therefore to act through a greater space and a longer time as in Fig. 2, whether it be variable as in Fig. 3, where it is great to begin with, and becomes less towards the end, a condition of things requiring comparatively a short time, or whether it be variable as in Fig. 4, where it is small to begin with, and becomes greater towards the end, a condition demanding the longest time of all.

Now these propositions, which are true when a body is caused by the action of gravity to attain a velocity of 1400 feet a second, are equally true when that velocity arises from the body being impelled by a force vastly superior to that due to the action of gravity, and needing therefore to be exercised through a correspondingly

diminished distance, and for a correspondingly diminished time. Take, for example, the force arising from the combustion of gunpowder, and applied to the projectile through the space which that projectile traverses in the bore of a cannon, and applied during the short time in which that traverse is made.

I have already told you that the length of the 38-ton bore is 16 feet 6 inches, but from this must be deducted, say, 2 feet for the length of the cartridge, leaving about $14\frac{1}{2}$ feet as the distance through which the shot moves under the influence of the powder pressure. Now $14\frac{1}{2}$ feet is the $\frac{1}{2069}$th part of 30,000 feet; therefore the average pressure on a projectile while traversing this $14\frac{1}{2}$ feet must be 2069 times as great as the weight of the shot, in order to give it as great a velocity, 1400 feet in a second, as it would have attained by falling 30,000 feet. I have said that this must be the average pressure. Obviously as regards strain tending to burst the gun, the most favourable condition of things would be that this average pressure should be exercised, but, equally obviously, the explosion of a charge of powder is not a means by which such an average can be attained. To put it popularly, one feels that the explosion of the charge in the small space which it occupies between the rear end of the tube and the base of the projectile before that projectile begins to move, must give rise to an intense pressure which will gradually die out as, owing to the progression of the shot, the space becomes enlarged, and as the gases are cooled. This being so, and 2069 times the weight of the shot being needed as the average pressure, it follows that as the final pressure falls much below the average, the commencing pressure must be greatly above it; and it is this great commencing pressure which strains the gun and demands the enormous thicknesses of metal which you see surrounding the powder chamber.

I believe I have now, by what I fear has been too long an introduction, prepared the way to show you why it is that the large-grain slow-burning powders tax the resisting power of the gun less than it is taxed by the small-grain and quick-burning, for it will readily occur to you that if the chamber be occupied by large cubes of powder which begin burning from the outside, and in this beginning of burning generate a certain pressure, the shot will commence to move, and the space in which the powder gases are confined will commence to increase, so that by the time the whole of the powder is ignited the chamber will have become larger, and thereby the intensity of the pressure will be reduced; while if the powder had been composed of small grains, like those used in a rifle, the explosion of the charge would take place in so short a time that the shot would not have appreciably moved before the whole of the charge was in combustion, and in this way a very high pressure would be produced. I may mention that the French apply to this small-sized powder when used in cannon the expressive title "poudre brutale."

Diagram 6 is a "curve of pressures." Imagine that the horizontal

line A B represents the length of the bore traversed by the shot, and that the vertical lines represent tons pressure per square inch; and assume that the black vertical dotted line A to D represents 24 tons and is the maximum pressure arising from burning the charge of pebble powder, and that this pressure is maintained for a very short time, and therefore through a very small distance as represented by the summit C of the hillock to which I now point, and that then by the increase of space arising from the moving of the shot and by the cooling of the gases the pressure diminishes until when the shot is leaving the muzzle of the gun, the pressure has fallen to $2\frac{1}{4}$ tons on the square inch, as represented by the height B E. The average of these varying pressures will be represented by the dotted line *x y*, and will be found to be about 5 tons per square inch; which pressure when applied on the area of the base of a 12-inch projectile, equals 568 tons, or 2069 times the weight (as I shall presently have occasion to tell you) of the common shell, empty, but with its gas check, for this gun. The maximum strain you will see to attain this average pressure has been only 24 tons to the square inch.

Assume next that a quick-burning powder had been used, and that this had given a maximum pressure, as shown by the dotted line A F, of 30 tons on the inch, but that this pressure had continued for a still shorter time and for a less distance, as shown by the position of the hillock G, and had then fallen until at the muzzle it retained only the pressure expressed by B N. The average would still be represented by the line *x y*, and the effect in propelling the shot would be just the same as before, but the gun would have been strained by a maximum pressure of 30 tons per inch, instead of by a maximum pressure of only 24 tons.

Although Diagram 6 represents the varying pressures which propel the projectile, it does not, until after the maximum pressure is reached, that is to say it does not, except in front of the point of maximum pressure, represent the extent to which the gun is subjected to these pressures. In the absence of wave action (to which action I shall have to refer hereafter), if a pressure exists at the base of the shot, that same pressure must prevail throughout the bore of the gun to the rear of the shot. Diagram 6*a* correctly shows the strains which come upon the gun under the conditions of propelling pressure represented in Diagram 6.

From that which I have just been saying, you will be prepared to hear that, other things being equal, the intensity of the explosion is increased when the space occupied by the powder is diminished. In practice the cartridges are made of such length, that every pound of powder as it lies in the gun between the base of the projectile and the rear end of the bore, reposes in a space the cubic contents of which are 30 inches, or rather more than the contents ($27\frac{3}{4}$ inches) of a pound of water, and if the cartridge were made of double the length and of correspondingly attenuated girth, so that the powder lay with practical uniformity in the cartridge along the bottom of the bore, and thus each

pound had devoted to it 60 cubic inches of space, the intensity of the explosion would be greatly reduced. While on the other hand, if the powder were made into a solid block occupying the space demanded by its specific gravity of 1·76, namely only 16 cubic inches, and if that block could by some means, say by threading it in all directions with platinum wires, be made red hot by electricity, and be ignited throughout simultaneously, the intensity of the explosion would be at a maximum.

There is no need for me to tell you that it is in the highest degree important in designing a gun, that the designer should know with a considerable approach to accuracy, what are the *pressures* the gun may be called upon to sustain, and what are the varying pressures to be expected in the different parts of the same gun. Now there are two modes by which the pressures may be ascertained. The first mode is by calculation from the velocity of the projectile or from the recoil of the gun, if it could be left free to recoil; the second mode is by the introduction of pressure gauges called from the mode in which they are acted upon "crusher" gauges placed within the gun itself. I have already shown you how if the velocity of the shot when it leaves the muzzle is known, and the weight of that shot, and of the powder which is also in motion is known, and the distance from the part of the bore where the base of the shot was situated before it started to the muzzle is known, it is comparatively easy to calculate, after making due allowance for the pressures required to set up rotation, the mean pressure that has been exerted in the gun, but (as I have stated) the information thus arrived at does not tell the inquirer what the varying pressures have been, nor what the maximum pressure has been. But if it were possible to ascertain what was the velocity of the projectile, say when it had traversed half-way along the bore, it would then be easy to compute the average pressure which had prevailed from the commencement of the journey to this half-way point, and then the average pressure from this point to the muzzle; and this clearly would be a step gained in the solution of the question, what are the variations of pressure during the passage of the shot? Similarly, if the length of the traverse of the shot in the gun were divided into say ten parts, and the velocity as the shot passed each of these imaginary points of division could be known, the average pressure in each section could be calculated, and thus the maximum pressure and the variations would be ascertained. You will, I think, agree with me that it is a difficult task to detect at what speed a projectile is travelling when ten or twelve feet within the bore of a gun. The way in which up to a certain date this information was obtained with small arms, was to cut off successive lengths from the barrel of a rifle so that each part of the barrel in turn became the muzzle, and thus the velocity of the bullet as it issued could be recorded in the usual manner. But this mode would be a costly one to pursue with large cannon, and, moreover, it is open to the great objection that a record

of velocity is obtained at only one place for each discharge, and thus if there were any accidental variation in the weight, or in the dryness, or in the circumstances attending the ignition of the charge, discordant results would ensue.

We owe to the study and to the inventive genius of Captain Andrew Noble, F.R.S., an apparatus, the *chronoscope*, which enables the inquirer to determine the velocity of the projectile within the gun at as many points as he may deem necessary. Diagram 7 represents the chronoscope; you will see that it consists of a train of wheelwork by which the smooth circumference of the final wheels lettered A can be made to move with great velocity. As much as 100 feet per second are reached.

Assume now that the circumferences of the wheels be clothed with properly prepared paper bands, and that a wire be brought in proximity to the paper on each wheel, and be so coupled up to an electrical apparatus, that on the wire being broken in some part of its length, electricity shall pass from the point of the wire through the paper to the wheel, making a mark on the paper. Next assume that the wire, properly insulated, is applied to a suitable trigger-like apparatus, such as is shown on the diagram, in the side of the bore of the gun to be experimented upon; then if the shot be fired, it will as it passes each apparatus push in the trigger, and will thereby break the wire, and will cause a record to be left on the paper; and if, as suggested, several wires be applied to the gun, say at ten points, these will be broken in succession as the shot passes them, each rupture will make its own mark on its band of paper on the edge of its wheel, and the velocity of that band being known, the varying distance of the marks from a common datum will be a measure of time, and therefore of the velocity which the shot had in passing each wire in the gun, and thus the varying pressures in the gun can be calculated. I have said that a velocity as high as 100 feet or 1200 inches per second has been given to the circumference of the wheels, and as by proper appliance the $\frac{1}{1000}$ of an inch can be read off, it is possible to detect intervals of time of less than one-millionth of a second (actually $\frac{1}{1,200,000}$) in duration.

The second mode of ascertaining pressure is shown on Diagram 8, which exhibits a crusher gauge drawn one-half full size; the actual gauges both before and after use are on the table. You will see the gauge is composed of a little solid cylinder of copper, $\frac{1}{10}$th of an inch in area by $\frac{1}{2}$ inch long; this is held loosely by a spring in the centre of a hollow steel cylinder, which cylinder is screwed into the end of the bore or the side of the bore, or the base of the projectile as the case may be, and is provided with an accurate but easily fitting steel piston which bears upon one end of the copper cylinder, the other end being in contact with the base of the hollow cylinder. When the gauge is applied to the side of the bore the outer end of the steel piston is fair with the surface of the bore, and for the time forms a portion of that surface, and thus it receives on its end as much

pressure per inch as the bore of the gun is receiving at the part where the gauge is inserted, and in this way the piston is subjected to an outward pressure equal to the maximum pressure per square inch prevailing in that part of the gun multiplied by the area of the end of the piston. You will see that the only thing which prevents the piston from being driven outwards under this pressure is the copper cylinder; but this cylinder is designedly made too small in area to support the pressure without being shortened and thickened out. By trial in a proper machine, where the pressures are known, the behaviour of similar copper cylinders under varying loads is ascertained, and in this manner an examination of the extent to which the cylinder has been shortened by the pressure in the gun gives at once the information needed, namely, what that pressure had been.

Reverting to Diagram 6*a*, I will now state that this shows by the full lines the maximum and varying pressures which have been ascertained by experiment to prevail in such a gun as that which burst on board the 'Thunderer' when using 85 lbs. of pebble powder, occupying 30 cubic inches to the pound. You will remember that the maximum is 24 tons on the square inch, that this pressure prevails over the length of the powder chamber, and for a short distance beyond, and that then the pressure drops in the manner indicated by the curved line, until at the muzzle it is only 2¼ tons per inch, as shown by the vertical line B E. With such a curve of pressures it is easy to ascertain what is the strain tending to burst the gun at any part of the bore; for example, at the point *h* the pressure would be represented by the vertical line *h i*, and would be found to be 10 tons, while at the point *m* it would be represented by the line *m n*, and would be found to be 5 tons.

I will now ask your attention to the *projectiles* used in these guns. Two kinds are employed. One is known as a common shell; it is a hollow cylinder of ordinary cast iron, terminating in a conoidal point, and containing within it a very considerable bursting charge. This shell weighs, when fitted with its gas check, but empty, 590 lbs., and to propel it that which is known as the full charge, namely 85 lbs. of pebble powder, is employed. Such a shell would be unfit to penetrate armour plating, as the point of the shell would fail on striking the plate; but if proper iron (that which is known as mottled pig-iron) be employed, and if when fluid it be poured, not into a sand or loam mould, but into a mould made of cast iron, the exterior of the casting will be rapidly cooled (chilled) by contact with the iron sides of the mould, and the result will be the production of a material exceeding in hardness the very highest tempered steel. Such a projectile is competent to penetrate armour plate; and if time admitted I should very much like to go into the reasons why, but I must abstain from so doing. These shells contain only a small bursting charge. They weigh when empty, but with their gas check, as much as 700 lbs., and they are propelled by the battering charge of 110 lbs. of pebble powder. Samples of the actual shells, cut open, are before you.

To cause the shells to obey the action of the rifling, and to rotate on their axes, they are provided with as many rows of studs as there are rifle grooves—namely, nine rows, with three studs in each row. The studs are secured into the shells in the following manner. Diagram 9 shows, greatly exaggerated by Fig. 1, a section through a stud-hole. This is circular, and, as you will see, is undercut; it is made by drilling out a parallel hole, as represented by the dotted lines, and then by introducing a tool, a sample of which I now show you, provided with a hinged cutter, which projects further and further as the tool descends, until by the time it has reached the bottom of the hole the projection is sufficient to give the undercut form shown. Fig. 2 represents one of the gun-metal studs placed in such a hole. You will see the stud is cylindrical, but that the bottom of it is cupped. If severe pressure be applied to the top of the stud, the cavity of the cup will be flattened, and the inverted brim, so to speak, of the cup, will be swelled outwards, and will fill up the countersunk part of the hole (see Fig. 3), and in that way it will be securely fixed into the shell, without the possibility of becoming unscrewed or detached.

The Palliser chilled shell is too hard to admit of the stud-holes being drilled out; they are therefore formed in the act of casting, by a process which is technically known as coring—that is to say, sand is rammed into a box the shape of the desired hole; the sand is then dried, and becomes a plug which is fixed in the side of the chill mould; the metal flows round about the plug, and when the metal is set the plug is cleared away, and a hole of the form of the box in which the sand plug itself was made is left in the casting; but such a hole will not have the smooth surface of one that has been cut out by a drill. You have before you samples of the cores, samples of the cored holes, and samples of the drilled holes; also studs which have not yet been compressed into a hole, and a number of studs which have been compressed, some into drilled holes and some into cored holes. The appearance of the parts which have been in these respective classes of holes differs, as might be expected, bearing in mind that the one class has been cut out so as to be smooth, while the other has been cored out, and is comparatively rough. The difference is sufficiently marked to enable one to say which of the studs have been in cored holes and which of them have been in drilled holes.

The projectiles do not fit the bore of the gun accurately, and thus there is a space (a very small one, it is true, but still a space) through which the gases from the ignited powder can pass between the projectile and the walls of the bore. This escape of gas causes a slight decrease in the useful effect of the powder, but is more prejudicial for another reason, namely, that the high velocity at which the heated gases pass operates injuriously upon the bore of the gun by erosion in the neighbourhood of the base of the projectile. To prevent this escape and the injury arising from it, the base of the projectile is provided with what is known as a "gas-check." Reverting to Fig. 9, you will see attached to the base of the shell a slightly

saucer-shaped disc of copper, having thin edges. The pressure of the powder speedily expands the disc, making its edges fit the bore, and to some extent making them also fit into the rifle grooving. I may mention in passing, that advantage is now being taken of the gas-check as an implement to cause the rotation of the projectile, thus dispensing altogether with the studs.

Diagram 10 is a drawing of the wrought-iron *gun carriage* on which by its trunnions the gun is supported: this carriage, lettered *a*, bears on slides lettered *b*. The rear end of the slides is upon a pivot, *b′*, while the front end can be put upon any one of three steps, the lower, middle, or upper, so that the slides may be horizontal or inclined as desired. From the front of the carriage there depends an arm *c*, to which is attached a piston rod *d*, having on it a piston *e*, travelling to and fro within the hydraulic cylinder *f*, placed below the slide. By a handle *g*, when put into the position 1, water under pressure from an accumulator can be introduced into the cylinder between the end *d* and the piston *e*, so as to drive the piston towards the left hand, and thereby cause the carriage to move and to run the gun inwards as shown by the arrow in full lines, until stopped by a buffer coming against a stop at *y*. When the handle *g* is moved to 2, then the position of the valves is changed so as to stop the inflow of water from the front of the piston, and other valves are opened to allow an outflow, so as to admit of the water under pressure flowing into the cylinder between its end *k* and the piston *e* to drive the carriage towards the right hand as shown by the dotted arrow, and thus to run the gun out. By the foregoing mode, the gun, it will be seen, can be run both in and out without the exercise of any manual labour. But the apparatus attains not only this end, but the further one of checking the recoil which arises from the discharge of the gun when loaded, and this it does in the following manner. Connected to the rear end of the cylinder *f* is a valve box *l*, containing as many as six large safety valves loaded by spiral springs to a pressure nearly double that which prevails in the accumulator, so that when water from the accumulator is introduced behind the piston *e* to run the gun out, these valves do not open; but assuming the gun to have been run out to the firing position and to have been fired, then the recoil drives the gun back with great violence, and thereby causes the piston *e* to press on the water in the cylinder with such a pressure as to raise the safety valves notwithstanding the load upon them, and to allow the water to escape until the energy imparted to the gun and carriage by the recoil has become expended in driving out the water through the loaded valves. The resistance offered by the water under these circumstances is always sufficient to stop the recoil by the time that the gun has only made a portion of its inward run, and the remainder of that run has to be accomplished by putting the lever *g* into the position number 1, so as to introduce the water under pressure between the cylinder end *d* and the piston *e*. As a matter of fact when firing with powder, as soon as

the noise of the explosion is heard, the gunner No. 1 in charge of the lever *g*, does put it into this running-in position. At first, and without consideration, one is tempted to say this must be a wrong thing to do, because the object at that time to be obtained by the apparatus is to check the recoil, and therefore the putting the lever *g* in such a position as (apparently) to cause a water pressure to aid the recoil must be wrong. But reflection shows this not to be the case; the opening through which the water from the accumulator enters is so small, that even when all resistance is removed, the water cannot flow into the cylinder *f* with sufficient rapidity to fill the space between the end *d* and the piston *e*, as that space is enlarged by the rapid motion of the piston when driven in by the recoil; and I need hardly tell you that if the water does not fill the space, the unfilled part will be in a vacuous condition, and practically to as great an extent as if no water were there at all. When any water is there, the vacuous space will be less; but less though it be, it will be equally vacuous, if I may use such an expression, and therefore no pressure will be applied to the piston *e* to drive it towards the left hand, and thus to assist the recoil.

Now, as the object of the crew working the gun is to do so with rapidity, and as the necessary inward run of the gun, as I have already told you, is not effected by the recoil alone, there is every reason why the man in charge of the lever *g* should move it directly he hears the explosion, because by doing so he does not, as I have pointed out to you, aggravate the effect of the recoil, and he does make a connection with the water in the accumulator, so as to cause the pressure from it to continue, without a break, the run in which had been commenced by the recoil; whereas if this course of putting over the lever immediately were not adopted, the man would be compelled to wait until the motion from the recoil came to an end, and then the gun would have to wait until the space between the piston *e* and the cylinder end *d* was filled up before the gun would re-start on its inward run.

I have already told you how the position of the slides as regards elevation and depression may be altered; but I need hardly say that the inclination of the gun for aiming at each time of firing is not made in this manner, but is effected by vibrating the gun on its trunnions by appropriate apparatus; and it is this apparatus which also depresses the muzzle of the gun into the position for loading, about which I shall have to speak shortly: but previously I must say a few words as to the *general arrangement* of the 'Thunderer.'

Diagram 11 exhibits a plan view of the *deck* of the 'Thunderer.' A is the fore turret, containing the two 38-ton guns, one of which burst, and B is the after turret, containing two 35-ton guns; and let me mention that, as regards bore and thickness of metal, the 35-ton guns are practically identical with the 38, but being loaded by hand from within the turret they are of necessity some 3 feet shorter than the 38-ton guns, which, as you will presently hear, are loaded

hydraulically from without the turret, and in this way the after-turret guns are each 3 tons lighter than the guns of the fore turret.

Diagram 12 shows to a much larger scale a plan of the *fore turret*, where R represents the right-hand gun and L the left-hand (the one which burst); S represents the centre line of the starboard loading apparatus and P the centre line of the port, G G the position of the running in and out levers, already referred to in Diagram 10, H the lever of the hydraulic locking bolt, I the lever of the dead-lock, K the handle which controls the engine that causes the turret to revolve, and the numerous small circles show the positions occupied by the officer and ten men who are in the turret at the time of working the guns.

Diagram 13 is a transverse section through the fore turret, showing parts already described, and also exhibiting one of the *hydraulic loading gears.*

As you are aware, in a ship with a revolving turret the horizontal movement of the gun to bring it to bear on the object aimed at is attained by causing the turret to move in one direction or the other. Although the turret is 31½ feet diameter, and weighs, including the guns within it, as much as 406 tons, the revolution, which is due to a special steam-engine placed beneath the turret, is started, stopped, or reversed with the greatest ease by one man, the captain of the turret, through the instrumentality of the handle K. This same handle is used also to revolve the turret so that the two guns within it may be brought into a fit position to be loaded either by the pair of hydraulic loading cylinders S, on the starboard side, or the pair P, on the port side. When in position for loading it is necessary—as the loading is effected through tubes in the side of the turret, which at the time of loading are prolongations of the bores of the guns, the guns being then brought into such a position as to "line" with these tubes—it is necessary that the turret should be securely locked. This is effected by the use of two locking bolts, the hydraulic bolt and the deadlock bolt. The hydraulic bolt is one which being pressed outwards by a yielding pressure, that of water from the accumulator, can be safely protruded to lock while the turret still has some "way" upon it. This "way" is checked by the bolt without injurious shock, and the turret is brought to rest so near to the desired spot that the deadlock bolt worked by the handle, I, can be introduced.

Assuming the turret to be brought into position suitable for one of the loading gears, the muzzles of the guns are depressed, as you will see is shown in Diagram 13, until they are in a line with the loading tubes already mentioned, which loading tubes pass through the thickness of the walls of the turret in an inclined direction, and are placed so low down as to be just beneath the upper deck. When the guns are thus depressed and the turret locked, a visible signal is made from within the turret, by means of a "tell-tale," to the loading crew who are between decks outside the turret, "Sponge and load," whereupon these men proceed in the following manner. The sponging

and loading apparatus—for they are one and the same—is hydraulic, and consists of an inclined cylinder so placed on supports as to have its centre upon the prolongation of an imaginary line passing through the axis of the gun when in the loading position. Within this cylinder is contained a plunger, which is hollow and has within it a second plunger, so that the cylinder with its two plungers may be likened to a threefold telescope. The inner plunger carries on its end a head (the rammer), which is surrounded by the sponge, not an actual sponge, but a sponge-like fabric; the head is hollow, and is supplied with water under pressure by being in connection with a hole which extends along the centre of the plunger; the front of the head is provided with a little valve opening inwards and kept closed, therefore, by the pressure of the water. There is a small pin on the front of this valve, which projects. This being the construction of the apparatus, the action is as follows. The man in charge moves the lever, and thereby admits water under pressure into the cylinder of the rammers, which water drives out the plungers, and almost always, as it possesses the greater area, the large plunger starts first and goes outwards (until checked by a stop), carrying with it the smaller plunger and the head well up into the bore of the gun, and then the smaller plunger starts out under the influence of the pressure, and continues the carrying of the head forward until it reaches the end of the bore, when the pin on the little valve strikes the rear end of the bore, opens the valve, so as to allow the water to escape, to wash out the gun and to saturate the sponge cloth. The lever is then reversed, the water pressure is made to act upon certain annular surfaces round about the plungers and in the reverse direction, and in this way the telescope shuts up, withdrawing the head from the gun. Two men then lift the cartridge and put it into the loading tube. The projectile has been previously brought in a truck and placed on the platform of the hydraulic lifting gear, and, the cartridge being in, the lever is moved, which admits water pressure into a vertical hydraulic cylinder, and thereby raises the plunger within it, carrying upwards the platform, the truck, and the projectile to a definite position which is one that places the projectile in the exact line of the bore.

A papier-mâché disc wad is next put upon the rammer head. You will see the wad consists of a disc, of a tubular socket, and of a collar round about that socket. The wad is held in place by the socket, being received into a cavity provided for it in the rammer head, while the collar keeps the back of the disc away from the head and from pressing on the pin of the water valve, and thus prevents the pressure exerted in ramming from opening that valve and deluging the gun while loading. A wad is wanted for two reasons: one, the ordinary one, that the ship may not in rolling cause the projectile to move in the gun; for this purpose wedge wads, of which I show you a sample, have long been used. You will see that this wedge wad consists of a large number of hard wooden wedges strung upon a rope, made up into a ring just suited to go inside the bore, so that the wedges may

be introduced with their points between the shot and the gun, and be rammed hard home. The second purpose, and the unusual purpose for which a wad is required in the case of the 38-ton guns of the 'Thunderer,' is to make sure that the projectile shall not return down the inclined bore of the gun on the withdrawal of the rammer.

I have told you that the muzzle of the gun is inclined downwards when loading, but I have not yet stated the angle: it is $11\frac{1}{2}°$, and I may say that very careful experiment on board the 'Thunderer' proved that this angle is as nearly as possible the angle of repose of a projectile lying on the bore of the gun.

In several cases the rammer on being very quietly withdrawn, was not followed by the projectile; in other cases it was; but in no instance were more than some 7 or 8 lbs. needed to uphold the projectile with absolute certainty. As regards the cartridge, the inclination must be much greater before that will commence to slide down the bore; we made careful experiments with cartridges put in loosely and with cartridges rammed home, and we found, speaking roundly, that some 45 to 50 per cent. of the weight of the cartridge was needed in direct pull to keep the cartridge moving down the bore at this inclination. The suggestion that the cartridge has at any time slipped down the bore when once rammed home, or even when once placed home loosely, is entirely unwarranted, as the makers of such a suggestion would very soon find if they were to perform the simple experiment of trying to pull the cartridge down the inclined bore. But although there is not the slightest ground for fearing that the cartridge would slip down, there is great probability that the projectile would do so, and therefore it becomes necessary, irrespective of the question of the ship rolling, to use a wad. Assuming as some have done that one of these guns can be burst by means of a wad, I would ask you which of the two wads is the more dangerous, the old wooden wedge wad, where the points of the wedge are driven in between the projectile and the walls of the bore, or the papier-mâché disc wad, which is not inserted between the projectile and the walls of the bore at all, but is merely retained in the gun by the pressure around the edge of the disc. The effect of this pressure can be overcome (as we ascertained by direct experiment) by nothing short of the force of 8 to 10 men pulling directly at the wad.

That which I have had to say about the wad has been so lengthy I fear you may have forgotten that we left the cartridge in the loading tube, and the projectile elevated in a line with the bore waiting to be rammed in; this ramming in is effected by a similar movement of the lever to that which was employed in the sponging. The telescope again shoots out and the head goes into the gun ramming before it the wad, the projectile, and the cartridge. You will have remarked that owing to the larger plunger of the two moving first and then coming to a stop when the ramming home is by no means complete, that it is impossible to see from any mark on the rammer how far the head has advanced into the gun after the first joint of the rammer has come to

rest. The information that the charge is home, is afforded audibly, by the concussion arising from the striking of the cartridge against the end of the bore, and the position of the rammer is indicated visibly by a hand made to revolve on a dial by means of a line attached to the rammer head.

When the gun is loaded, those in charge of that operation give also a visible signal by a tell-tale, to the crew within the turret: "Left gun ready," or "right gun ready," as the case may be. The gun is then raised from the depressed position, is run out by the hydraulic apparatus through the port, and is adjusted as to level for firing, the turret is unlocked and is revolved until the gun bears upon the object, and then the gun is fired. This firing may be done either electrically or by hand: in either case a tube is inserted into the vent; this tube contains powder closely rammed, and there is a composition in its head which in the case of electrical firing is ignited by an electric spark conveyed through a wire coupled up to another small wire which you see projecting from the head of the tube.

Electrical firing is used to give a simultaneous discharge from all the guns, so as to concentrate their fire upon the object aimed at. When electrical firing is employed, the whole ship becomes the gun carriage and the firing is done not by the officers in the turret, but from the "conning" tower; to a key in which the wires are connected. When the officer in the conning tower sees by the aid of an instrument, the "Director," which he has there, that the guns are bearing on the object, he depresses the key, and thus if no misfire takes place delivers a concentrated broadside. In the other mode of firing the guns, the composition in the head of the tube is ignited by a friction arrangement; this is put into operation by pulling a lanyard, and there is a contrivance by which, if desired, both the lanyards in one turret can be pulled simultaneously.

Having now described to you the construction of the gun, the mode of loading, and other matters necessary to be described in order to place you in a position to appreciate what occurred, I will briefly narrate the circumstances attendant on the working of the guns on the 2nd of January last.

The two guns of the fore turret and the two in the after turret were each loaded with a battering charge of 110 lbs. of pebble powder, and with a Palliser shell, empty, the two guns of the fore turret having in addition disc wads. In the after turret the naval wedge wad was not used, as the guns were loaded in a horizontal position, and the sea being smooth there was no fear of the projectile being shifted by the rolling of the vessel.

All four guns were primed with electric tubes, and were to be discharged as an electric broadside, the discharge being effected, as already stated, by the depressing of the key in the conning tower. On the depression of the key on this occasion, beyond all question there was a misfire as regards one of the two guns of the after turret.

I say beyond all question, because the charge was subsequently "wormed" out of the gun, and the torn cartridge, with its 110 lbs. of powder, was thrown overboard. The Committee say, and I, the assessor to that Committee, say that there was a similar misfire as regards the left-hand gun of the fore turret, the one which afterwards burst. Following the electric broadside, the order was given for independent firing, that is to say, each gun was to be fired by itself, and the firing was to take place while the turrets were revolving. The charge was to be the "full charge" of 85 lbs. pebble powder, and an empty common shell. This was inserted into the left gun of the fore turret. The gun was raised and run out to its firing position, was fired, and burst, with the disastrous results we all but too well know; and I now, at this late period, come to the real object of my lecture, the consideration of what it was that caused the explosion.

I should like to deal with this subject in the manner in which the Committee have dealt with it; that is, I should like to review and dispose of all the suggestions which have been put forward other than the true one before considering that true one itself, but I must not be tempted into following this course, as I well know if I do the clock will sound the end of the hour allotted for this lecture before I have reached the true cause.

I will refer you to Diagram 14, which shows the external appearance presented by the ruins of the gun when brought together, and to Diagram 15, which represents the interior of the splinters of the steel tube that have been found, when also laid side by side in their proper juxtapositions. This last diagram, you will see, is like the diagrams of the rifling, a "development," that is to say, as I explained to you in speaking of those diagrams, it gives a representation of that which would appear if a picture made on a paper tube were, by the cutting open of the tube from end to end, to be laid out flat.

I will ask your particular attention to splinters 1, 2, and 3, and to their left-hand ends, which represent the ends where they joined the piece of steel tube remaining in the breech coil; you will see a shaded mark upon each of them at the left-hand end, which was caused by an abrasion extending here across the splinters, and made at an angle to the surface of the tube.

Diagram 16 shows a longitudinal section through splinter 1, and through splinter 13, which is one of the splinters forward of 1, and it shows them in the position and under circumstances which account for the abrasions on the left-hand ends of 1, 2, and 3, and for similar but reverse abrasions on the front piece 13.

An inspection of the remains shows clearly that the centre of the explosion was at the point A, the former point of union between splinter 1 and the pieces which were in continuation rearward of splinter 13. With A as the centre of the explosion, the effect would have been as shown, to bulge the gun out at that part, and thus to break away the left-hand end of splinter 1 from the part of the

tube remaining in the breech, and to do so by making the corner B of the breech piece into a fulcrum. On this happening, the left-hand corner of splinter 1 would be raised above the general line of the bottom of the bore, and thus if any part of a projectile were at that moment in the rear of the splinter, that projectile could not pass forward without abrading away the protruding corner of the splinter, and this is precisely what has happened.

And I will tell you what is the proof that the marks on these splinters 1, 2, and 3 must have been made by a projectile in motion, and not by accidental collision with any hard substance after the explosion occurred. Pieces 1, 2, and 3 form among them about one-half of the circumference of the 12-inch tube, and therefore, being hollow, they could not be uniformly marked, as we now see they are, by anything except a convex body of the same diameter of 12 inches. Is it credible that these three pieces each of them happened to strike in its flight some cylindrical body of 12 inches diameter, the axis of which was in an exact alignment with that of the concave curve of the fragment at the time of impact? I venture to say it is impossible, and that no other explanation can be given of these marks than that the pieces were tilted so as to form parts of a cone, the base being at A, and that while thus tilted a projectile passed by them. An examination of the marks shows that the abrasion was in the direction of the motion of the projectile.

Further, I will now show you why it is impossible that these marks could have been made by something protruding from the projectile. They commence at the left-hand end of the splinters 1, 2, and 3; the remains of the tube from which these splinters have been parted are absolutely free from mark: had the marks on 1, 2, and 3 been made while they were in one with the rest of the tube forming part of a cylinder, it is clear that fellow marks must have been found on the tube itself. There are no such marks, and only one conclusion can be drawn, and that is, that the marks are not due to any protrusion from the projectile, but owe their origin to the canting of the splinters 1, 2, and 3, and to this canting having brought their left-hand top corners above the line of the bore, so as to necessitate the abrading of these corners (at the angle at which they have been abraded) to allow of the passage of the projectile.

There may be some of you who will say, if the point B were used as the fulcrum for the enormous strain required to tear the splinter away from the part of the tube remaining in the breech piece, that fulcrum being of a soft material, wrought iron, must exhibit signs of the pressure to which it has been subjected; and I may tell you that it does exhibit these signs, and in the most unmistakable manner. It is literally bell-mouthed by the pressure that has been exerted upon it, and there are distinct prominences left in this bell-mouth between the parts where the splinters of the tube pressed.

I will now ask you to turn your attention to splinter 13. If this were abraded, it should be at the right-hand end, and the direction of

the bevel should be the opposite of that of the abrasion on splinter 1; and that which should be, is, for splinter 13 is thus abraded, as shown at C, and the abrasion has been made by a cylindrical body moving with enormous rapidity; that is to say, by the projectile which had previously abraded the left-hand end of splinter 1.

These indications, in the judgment of the Committee, and I trust in your judgment, prove to demonstration that at the time of the explosion, the centre of effort of which was at the point A, a projectile was to the rear not only of A, but as regards some part of it at least to the rear of the left hand of splinter No. 1. But if that projectile were the common shell that had just been loaded into the gun, its 85 lbs. of powder must have been in its rear, and therefore 6 to 7 feet away from the seat of the explosion. If this had been so, what force was it that produced explosion in a part of the gun in advance of the projectile, and where, according to the suggestion, there was nothing but atmospheric air?

I leave it to those who say there was but the single charge in the gun, to give a satisfactory answer to this question, and in the meantime I will offer to your consideration the hypothesis of the *double-loading* — an hypothesis which fulfils every necessary condition. Probably the best way of showing to you how exactly it does fulfil these conditions will be to make use once more of our diagram model, No. 13. This model has already been loaded with the battering charge of 110 lbs., and the Palliser shell with its gas check and wad; and I will take it that at the electric broadside there was, as regards the gun this model is intended to represent, a misfire; that this circumstance not being known, the gun was depressed to the loading position, and that the order was given (by the tell-tale) from those within the turret to those without to sponge and load, whereupon they sponge out, they put in the 85-lb. charge, the common shell in front of it with its gas check and wad, and then send in the signal, "Gun loaded." Look at it when thus double-loaded, and observe where the 85 lbs. of powder are in reference to the seat of the centre of the explosion, a little to the rear, but not more than would be rectified by the very first movement forward of the Palliser shell (see A′, Fig. 17). Now imagine the 110-lb. charge ignited, the flash from it passing along the rifle grooves outside the gas check to the 85 lbs. in front, and igniting this 85 lbs. placed between a common shell weighing 590 lbs. in front, and the Palliser shell of 700 lbs. in the rear, and being urged forward at that time by the commencement of the explosion of the 110 lbs. of powder, and thereby compressing the 85-lb. charge into the smallest possible space, and it may be, as suggested by Professor Osborne Reynolds, generating as much heat as would have ignited that powder, even in the absence of the flash along the rifle grooves.

You will remember how, in an early part of this lecture, I pointed out to you that the diminution of the space occupied by the powder added to the intensity of the explosion, and also how, if you could imagine powder heated throughout to nearly the exploding point,

so that it would be ignited all at once, the intensity would be at a maximum, in fact a detonation would take place. This is what well may have happened under the circumstances occurring here; but even in the absence of such additions to the ordinary force of the explosion, you have but to look at the proportions of that part of the gun where the 85-lb. charge was at the time of its explosion, to see that those proportions could not withstand the pressure arising from even a common ignition of that weight of powder. Referring also to Diagram 6*a*, the curve of pressures in the gun, you will see that the maximum pressure which comes on this part of the gun in ordinary use is only some 4 to 5 tons, instead of the 24 tons which would arise from the ordinary explosion of 85 lbs. of pebble powder.

Diagram 17 is intended as a rough representation of what took place when the hinder charge was fired with the other charge in front.

There is a further evidence that at the time the gun exploded the Palliser shell was still in it, and that is that which is afforded by a stud which was picked up in the turret.

I have explained to you the difference between the modes of making the holes in the common shells and in the Palliser shells. The Pallisers are cored, the others are made with a cutter; as a result, studs when taken out indicate by their appearance whether they have been in a Palliser shell or in a common one.

The stud which was picked up is much battered, but enough remains to cause all those who see it and who are acquainted with the subject to say it is a Palliser stud, and that it is so is now further corroborated by the fact that even although some pieces have been knocked away from the stud, its weight is still slightly in excess of that of a similar and perfect stud from a common shell.

Now it must be remembered that this stud could not have been accidentally in the turret. In the first place, owing to the manner in which the studs are fixed in the shells, it is practically impossible for them to come out; and in the next place, the gun is loaded from the outside of the turret, and a shell is never inside the turret at all except when it is in the bore of the gun.

There is another circumstance which points to there having been two charges of powder in the gun, and that is the tremendous recoil, a recoil so violent as to drive the buffers through the wrought-iron transom on which they were carried, and this, notwithstanding that the hydraulic apparatus for absorbing the recoil was acting, and must have been doing so with extra vigour, as will be readily understood when one recollects the augmentation of pressure necessary to drive water through orifices at a greatly increased rate. Let me say here, in anticipation of any objection that may be urged against the hydraulic apparatus, as being too delicate an implement for purposes of warfare, that notwithstanding this explosion occurred within the turret the hydraulic apparatus was uninjured, and we were able to use it for the purposes of our experiments.

I hope I have now proved to you that double-loading would account

for all that happened, and if time permitted I would endeavour to show you that nothing else would account for it. I know, however, that the common impression is, although the double-loading would account for all, double-loading is simply impossible, and must be discarded. I have had this said to me by many, but I have had it persevered in by none after the facts of the case were brought to their knowledge.

Before stating what the objections are that I find generally advanced against the possibility of double-loading, I will allude to portions of the evidence which bear on this subject, more especially as most of the objectors have not been at the pains even of reading the report, still less of studying the evidence. The captain of the vessel, one of the officers, and a sailor were watching the electric broadside, and they give evidence that three shots came from it. Now, as we know one of the guns in the after turret did not go off, if these three witnesses were not deceived as to what they saw, the suggestion of double-loading is a mistake; but in opposition to this evidence is to be set that of five sailors, including a signalman, who were all of them clear that two shots only came from that broadside, and that one of them came from each turret. Having said this much, I will now comment upon the reasons commonly given for holding the double-loading hypothesis to be impossible.

The first observation generally is: The noise and concussion of the explosion could have left those in the turret in no doubt as to whether the gun had gone off. The answer, concurred in by all who have been present in a turret at firing, is that the noise and concussion produced by one 38-ton gun when fired off are as great as the human ear can take in, and that two guns going off simultaneously (for I must ask you to remember that these guns were to be fired together electrically) would not add to the effect that would be produced by the explosion of one gun.

The next observation is: Those who loaded the gun must have known by the position of the rammer whether there was a charge remaining in the gun; but when it is explained to them that the rammer is telescopic, and that all motion of the visible part has ceased long before the second charge would be rammed home against the first, and that the index was out of action, that ground fails the objector.

The third and final reason for its being impossible to have double-loaded the gun that has ever been offered to me is: That the misfire must have been known, because the gun would not have recoiled and would have required running in by hand.

The answer to that is already in your possession, but I will repeat it. The gun is run in without manual labour, and by hydraulic power. Even when the gun is fired the recoil is not sufficient to send it in to the required extent and has to be supplemented by the hydraulic pressure which is applied immediately the explosion is heard.

Direct experiment shows that in from 4 to 6 seconds the hydraulic pressure alone will bring the gun in as far as the recoil

would send it, and thus, unless the gun is being watched during these 4 to 6 seconds, no one can tell how the gun got to the spot where the recoil, if there had been one, would have put it, whether by the recoil or by the hydraulic. Thus all three grounds for the "impossible" fail. Simultaneous electric firing makes an end of the information to be given by the noise of the explosion; telescopic rammers, of the information to be afforded by the length of rammer left protruding from the gun; and hydraulic running-in masks the recoil and renders that source of information nugatory.

Just a few words about other suggested causes.

First, and most important, that the gun was inherently too weak. I will not trouble you with the calculation which proves that it is abundantly strong, not only to bear a charge of 85 lbs. of pebble powder, but to bear far greater charges, but I will ask you to consider facts. The fellow gun has fired precisely the same number of rounds, and is unstrained. The two 35-ton guns, which I have told you are the same as the 38 only shorter, have also fired the same number of rounds, and with the same result. At the time of the explosion there were many guns identical with the exploded gun, with the important exception that they were bored to 12½ inches instead of to 12 inches, and that therefore they were subjected to greater total pressure, and that they had less metal to resist it. These guns had each been proved with at least two rounds of 150 lbs. of P^2 powder, and at least 130 lbs. of similar powder, the projectile used weighing over 800 lbs. One gun, No. 1, had fired 271 rounds varying from 130 lbs. to 170 lbs. of powder, with an 800-lb. projectile; another gun, 217 rounds of the same character as those last mentioned; and another gun, an experimental one, 503 rounds, some of them containing as much as 200 lbs. of powder. Since the explosion, the 'Dreadnought's' four 38-ton guns have been tried. This was done on the 29th and 30th of April, when 70 rounds were fired from them. These guns are hydraulically worked and loaded like those of the 'Thunderer.' Full charges and battering charges were used, and projectiles of as much as 800 lbs;* the 'Dreadnought's' guns being of 12½ inches bore in lieu of the 'Thunderer's' 12 inch, and being thereby weaker than the 'Thunderer's' guns; and these trials have been attended with the most favourable results.

The next suggested cause is that the gun had been injured by previous service. It is sufficient to say that the other guns in the ship had had an exactly similar service and are uninjured, and that there is not an appearance of the commencement of a crack in that part of the exploded gun where cracks do commence when a gun is beginning to fail.

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* In the case of these 12½-inch guns the full charge is as much as 100 lbs., while the battering charge is 130. On more than one occasion charges of 160 lbs. were used, and at the close of the Wednesday's proceedings an electric broadside was given with all four guns thus loaded.

Then it is said the materials might have been bad, or the workmanship might have been faulty. The answer to this is that we have tried samples cut out of the very gun itself, and notwithstanding these samples have been taken from pieces of metal which have been so much strained all over as to have been torn asunder in many places, the intervening metal from which we cut our samples, which must have been equally strained, afforded good results. Moreover, the splinters of the tube must have been extremely tough, for they exhibit an extension equal to about $1\frac{1}{2}$ inch in the whole circumference of the tube. With respect to the workmanship, it was excellent, as anyone may see by examining the remains.

Next it is said that the projectile was wedged by a stud, or by the wad—a papier-mâché wad, as I have told you. The navy, and indeed the land service, have for years used hard wedge wads rammed in between the shot and the tube, and nobody ever suggested that evil resulted from this practice. With respect to the stud, if that be the cause, why are not guns repeatedly burst? and how is a stud to become detached from one of these shells? I must confess I cannot conceive the possibility of it. Moreover, and this is the true answer to all the foregoing suggestions, and to the final suggestion, which I will deal with presently, none of these supposed causes could have produced an explosion in front of the projectile, and nothing but an explosion in front of the projectile can account for the appearances presented by the remains of the gun.

The last and most taking suggestion is, as I have just said, open to the same fatal remark; but were it not for this, it is one of considerable interest, as it attributes the bursting to an air space between the cartridge and the projectile, or as some have suggested, between the projectile and the wad which it is alleged was canted.

You probably are all acquainted with the principle of the Mongolfier water ram, where the sudden arresting of a column of water in motion causes an intensity of pressure which drives a portion of the water up to a much greater height than that which had produced the movement of the column. Similarly, to state it very familiarly, a gunpowder ram may be produced. Assume a projectile placed some distance away from the cartridge, and then that cartridge to be fired. The powder, as we know, weighs probably from $\frac{1}{8}$ to $\frac{1}{6}$ of the weight of the projectile itself. And that the powder, or the gases arising from it, being in motion and suddenly arrested by the projectile, might produce a pressure immediately at the base of the projectile greater than that which would have prevailed if the projectile had been close to the cartridge when it was fired. With a very explosive powder, a fine-grained powder and suitably fired, this may be the case; but even when it is the pressure is entirely local, being confined to a narrow ring of the bore of the gun in the neighbourhood of the base of the projectile, and when confined to a narrow ring, enormous pressures can be sustained without injury, and can be so sustained for this very simple reason, that the narrow ring has not only its own strength

to depend on, but has also the support of the metal on each side of it, which metal at the sides is not similarly strained. Obviously the metal in advance of the base of the shot is not exposed to pressure at all. I wish time admitted of my describing to you certain experiments I made as long back as the year 1870 on the "Contre-vapeur" break, in which I succeeded in obtaining by this ram principle a much higher pressure of steam in the cylinders of a locomotive engine than was at the time prevailing in the boiler; but I must content myself by referring those who may be interested in pursuing this subject to the volume for the year 1870 of the 'Transactions of the Institution of Mechanical Engineers.'

Further, with regard to this question of air space, it is well known that a sporting gun may be burst by the muzzle being choked with snow or with dirt when it is fired. The explanation of this is simple. The bullet, at the time it reaches the neighbourhood of the muzzle, has attained great velocity, and the pressure required to set even such light weights as snow or a little earth practically instantaneously into motion as rapid as that of the bullet, is sufficient to force these materials out sideways, and to produce a burst in the same way as the resistance offered by water to the setting up of sudden great velocity is sufficient to cause a shot to ricochet.

The best illustration of the way in which a bullet striking a soft obstacle near the muzzle, would act on that obstacle is, probably, to imagine a diaphragm of water extended across the barrel of a musket. One then has no difficulty in seeing that the intense pressure set up on such a diaphragm must be communicated to the side of the barrel. Again, in a musket the bulging may be due to the expansion of the projectile itself when striking a foreign object placed in its way. And I may tell you that Captain Noble has experimented, by putting a wax wad near the muzzle of a musket, the wad being equal in weight to the projectile itself, and that on firing, this obstacle was expanded and bulged, and split the barrel. He then placed the projectile near the muzzle, and fired the wax wad: a similar result ensued. Finally, on this subject I will ask your attention to Diagram 18, which shows the experiments as to air spaces made by Captain Noble in a 10-inch gun, having a 12-inch powder chamber, and with the following results:—

The powder used in the first experiment was pebble powder, and occupying such a space as would give a pressure of about 20 tons to the square inch, and would impart to a 400-lb. projectile a muzzle velocity of 1487 feet per second. A crusher gauge was placed in the bottom of the bore of the gun, and three such gauges were fixed in the base of the projectile; one as near as possible to its top, one in its centre, and one near the bottom; these gauges had been previously compressed so as not to move unless a pressure exceeding 10 tons per square inch came upon them. The projectile was placed 2 feet away from the cartridge, that is to say, there was a 2-feet air space; as the result, the velocity was only 1240 feet per second instead of the

1487, showing that the mean pressure must have been only 76 per cent. of that which would have prevailed if no air space had been left; and further, so far from the maximum pressure having been even locally increased, at the bottom of the bore of the gun it was but 11·7 tons, while at the base of the projectile it could not have amounted to 10 tons on the square inch, as none of these three gauges had moved.

In the next experiment the air space was as much as 4 feet; the muzzle velocity was only 1067 feet, instead of the 1487, showing that the average pressure was only 68½ per cent. of that which would have prevailed without an air space.

In this experiment the crusher gauge in the bottom of the bore and that in the centre of the base of the projectile had been previously subjected to 10 tons pressure, while the gauges at the top and bottom were left in their natural condition. The pressures recorded were, at the bottom of the bore 10·15 tons; at the top of the projectile 7·6 tons; at the bottom 6 tons, and at the centre something below 10 tons, as the gauge was not affected.

These two experiments having totally failed to set up a local pressure, the next experiment was made not with pebble powder but with the rifle large grain. The air space was again 4 feet; on this occasion the pressure at the bottom of the bore was 16·7 tons upon the square inch; on the top gauge in the projectile was 25·5 tons; in the centre of the projectile was 35·3 tons; and at the bottom was 23·3 tons; while with this powder had the gun been fired without an air space the pressures would have been from 27 to 30 tons. In these experiments no wad was used, and it was determined to make others with a disc wad. In the first instance 85 lbs. of pebble powder being employed, and a 6-feet air space being left, the wad being close to the projectile, the muzzle velocity was 849 feet, the pressure at the bottom of the bore under 10 tons, in the base of the shot at top under 8 tons, in the centre 5·6 tons, and at the bottom under 8 tons. The last experiment consisted in leaving a 2-feet air space between the cartridge and projectile, and a 4-feet air space between the projectile and the canted disc wad, while the muzzle velocity was 1208 feet as compared with the 1240 feet of the previous experiment with the 2-feet space; the pressure at the bottom of the bore was 11·1 tons as compared with 11·7 on the former occasion; while the pressure at the top of the base of the projectile was 7·9 tons, at its centre was 9·2 tons, and at its bottom 8·4 tons on the square inch. During the previous experiment it will be remembered that the gauges having been adjusted for 10 tons afforded no other information than that that pressure had not been reached.

I think these results sufficiently show that no harm would ensue from pebble powder, even if air spaces had existed, but there was abundant evidence to prove that in the 'Thunderer' gun no air space did exist; but time compels me to refer you to the report and minutes for this proof. But once more, let me remind you that an explosion of

Diagram 1

38 TON GUN

H M S THUNDERER

Dia' of Bore 12 Inches
Length of Bore 198 Inches 16 Ft 6 in
Largest Dia' outside C coil 57½ Inches 4 Ft 9½ in
Total length overall 230 Inches 19 Ft 2 in

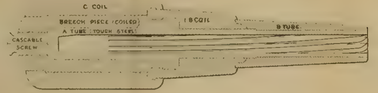


Diagram 2

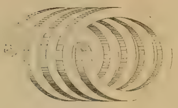


Diagram 3

Development of Bore of Gun
Increasing Twist of Rifling

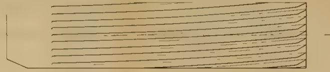


Diagram 4

Development of Bore of Gun
Uniform Twist of Rifling

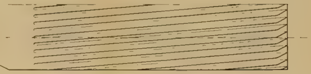


Diagram 5

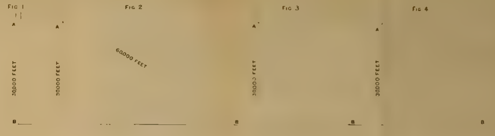


Diagram 6

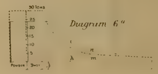


Diagram 6a

the gun from an air space would not have occurred 4 to 5 feet in front of the projectile.

I will now leave this subject of other suggested causes, and will, without pausing to describe them, merely remark that precautions have been proposed by the Committee, which, if adopted, would render double-loading in future practically impossible. I will also refer you to Diagram 19, which exhibits the improved construction of the rammer indicator on board the 'Dreadnought.'

In conclusion, let me say that the fellow 38-ton gun being, as you know, in England, I do hope the authorities will accede to the recommendation of the Committee, will try that gun with air spaces, and in any other way that anyone can reasonably suggest as having any real connection with the 'Thunderer' explosion; and further, I trust that then this gun will be double-loaded and fired. I do not think that such a trial is needed to give confidence to the service at large; it certainly is not needed to give confidence to those who are best able to form an opinion—witness the attendance at the working of the self-same kind of guns the other day on board the 'Dreadnought'—but I believe such an experiment with the fellow gun would be the most ready and most efficacious mode of satisfying the general public, who have not the means of investigating the question, or of coming to a right judgment upon it.

Pending such an experiment, if by what I have said to-night I have assisted in restoring confidence to the audience present, and I hope, through them, to many others, in the safety of the guns on which we rely for our defence both on sea and land, I shall feel I have a sufficient excuse for the trespass I have made on the time of my hearers this evening.

[F. J. B.]

GENERAL MONTHLY MEETING,

Monday, July 7, 1879.

C. William Siemens, Esq. D.C.L. F.R.S. Vice-President, in the Chair.

Francis Fesser, Esq.
James Garnett Heywood, Esq.
Miss Henrietta Lambert,
Edmund de Quincey Quincey, Esq.
Henry Smith, Esq.
Herbert A. Taylor, Esq.

were *elected* Members of the Royal Institution.

The Special Thanks of the Members were given to the Earl Bathurst, M.R.I. for his munificent Present of a large Bust of William Hyde Wollaston, M.D. F.R.S. M.R.I. by Chantrey.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

Governor General of India:—
Geological Survey of India:
Memoirs, Vol. XIV. and Vol. XV. Part 1. 8vo. 1878.
Palæontologia Indica: Series IV. 3; Series XII. 1. fol. 1879.
H. B. Medlicott and W. T. Blanford: Manual of the Geology of India. 2 vols. and Atlas. 1879.
The French Government—E. Le Blant: Étude sur les Sarcophages Chrétiens Antiques de la Ville d'Arles. fol. Paris. 1878.
Government of Victoria—J. J. Shillinglaw: Historical Records of Port Phillip. 8vo. Melbourne. 1879.
Actuaries, Institute of—Journal, No. 115. 8vo. 1879.
Asiatic Society of Bengal—Proceedings, 1878, Nos. 9, 10. 1879, No. 1. 8vo.
Astronomical Society, Royal—Monthly Notices. No. 7. 8vo. 1879.
British Architects, Royal Institute of—1878–9: Proceedings, Nos. 12–16. 4to. Transactions, Nos. 11, 12. 4to.
British Museum Trustees—Catalogue of Greek Coins: Macedonia, &c. 8vo. 1879.
Catalogue of Persian Manuscripts. Vol. I. 4to. 1879.
Brown, A. Crum, Esq. M.A. (the Author)—On the Theory of Chemical Combination. (K 103) 8vo. 1879.
Chemical Society—Journal for June, July, 1879. 8vo.
Dutch Church, Austin Friars; The Consistory—Catalogue of Books, MSS., Letters, &c. 8vo. 1879.

Editors—American Journal of Science for June, 1879. 8vo.
Analyst for June, 1879. 8vo.
Athenæum for June, 1879. 4to.
Chemical News for June, 1879. 4to.
Engineer for June, 1879. fol.
Horological Journal for June, 1879. 8vo.
Iron for June, 1879. 4to.
Journal for Applied Science for June, 1879. fol.
Monthly Journal of Science, June, 1879.
Nature for June, 1879. 4to.
Telegraphic Journal for June, 1879. 8vo.
Franklin Institute—Journal, Nos. 642. 8vo. 1879.
Geographical Society, Royal—Proceedings, New Series. Vol. I. Nos. 6, 7. 8vo. 1879.
Geological Institute, Imperial, Vienna — Verhandlungen, 1878. Nos. 14–18. 1879, Nos. 1–6. 8vo.
Jahrbuch: Band XXVIII. No. 4. Band XXIX. No. 1. 8vo. 1878–9.
Harrison, W. H. Esq. (the Author)—Spirits before our Eyes. Vol. I. 16to. 1879.
Capt. John James: Mesmerism, with Hints for Beginners. 16to. 1879.
Hillebrand, Karl, Esq. (the Author)—Zeiten, Völker und Menschen. Band I. 16to. Berlin. 1879.
Institution of Civil Engineers—Minutes of Proceedings, Vol. LVI. 8vo. 1879.
London, Corporation of—Analytical Index of the Remembrancia, 1579–1664. 8vo. 1878.
Manchester Geological Society—Transactions, Vol. XV. Parts 3, 4, 5. 8vo. 1879.
Mensbrugghe, Professor G. Van der — Nouvelles Applications de l'Energie Potentielle des Surfaces Liquides. (K 103) 8vo. 1879.
Meteorological Office—Meteorology of the Arctic Regions. Part I. 4to. 1879.
Meteorological Society—Quarterly Journal, No. 30. 8vo. 1879.
List of Members. 8vo. 1879.
Photographic Society—Journal, New Series, Vol. III. No. 9. 8vo. 1879.
Physical Society—Proceedings, Vol. III. Part 1. 8vo. 1879.
Sir C. Wheatstone: Scientific Papers (with the Harmonic Diagram). 8vo. 1879. (Two copies.)
Pole, William, Esq. F.R.S. Mus. Doc. (the Author)—The Philosophy of Music: being the Substance of Lectures at the Royal Institution in 1877. 8vo. 1879.
Preussische Akademie der Wissenschaften—Monatsberichte: März, April, 1879. 8vo.
Reilly, F. S. Esq. M.R.I. (the Translator)—J. M. Ludwig: Pontresina and its Neighbourhood. 16to. 1879.
Royal Society of London—Transactions, Vol. CLXVIII. and Vol. CLXIX. Part 2. 4to. 1879.
Society of Arts—Journal for June, 1879.
Stanley, W. F. Esq. M.R.I.—Professor G. Fuller's Spiral Slide Rule. (O 17) 16to. 1879.
Symons, G. J.—Monthly Meteorological Magazine, June, 1879. 8vo.
Tuson, Professor R. V. (the Editor)—Cooley's Cyclopædia of Practical Receipts. Part 13. 8vo. 1879.
Zoological Society of London—Transactions, Vol. X. Part 12. 4to. 1879.
Proceedings, 1879, Part 1. 8vo.

GENERAL MONTHLY MEETING,

Monday, November 3, 1879.

C. WILLIAM SIEMENS, Esq. D.C.L. F.R.S. Vice-President, in the Chair.

Major-General Henry Philip Goodenough, R.A.
John Henry Sampson, Esq.

were *elected* Members of the Royal Institution.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The French Government:
Documents Inédits sur l'Histoire de France Recueil des Chartes de l'Abbaye de Cluny, formé par Auguste Bernard. Ed. A. Bruel. Tome I. (802–954.) 4to. 1876.
Inscriptions de la France de V^e Siècle au XVIII^e. Ed. M. F. De Guilhermy. Tome III.–IV. 4to. 1877–9.
Inventaire du Mobilier de Charles V. Roi de France. Ed. Jules Labarte. 4to. 1879.
Le Livre des Psaumes. Ancienne Traduction Française: d'après les MSS. de Cambridge et de Paris. Ed. F. Michel. 4to. 1876.
Lettres, &c. du Cardinal de Richelieu. Tome VIII. 4to. 1877.
Lettres du Cardinal Mazarin. Ed. M. A. Cheruel. Tome II. (Juillet 1644–Déc. 1647). 4to. 1879.
Mélanges Historiques: Choix des Documents. Tome II. 4to. 1877.
Histoire Générale de Paris: Topographie Historique du Vieux Paris. Tome III. 4to. 1876.
Inventaire des MSS. Français de la Bibliothèque Nationale. Par L. Delisle. Tome II. 8vo. Paris, 1878.
Governor General of India:—
Geological Survey of India:
Records, Vol. XII. Parts 1–3. 8vo. 1879.
The India Office—Account of the Operations of the Great Triangular Survey of India. Vols. II. III. IV. 4to. 1873–6–9.
F. Day: The Fishes of India, Vol. II. 4to. 1878.
Accademia dei Lincei, Reale, Roma—Atti, Serie Terza.
Memorie delle Classe di Scienze Fisiche, &c. Vol. II. Dispensa I. II. 4to. 1878.
Memorie delle Classe di Scienze Morale, Storiche, e Filologiche, Vol. II. 4to. 1878.
Actuaries, Institute of—Journal, No. 116. 8vo. 1879.
American Philosophical Society—Catalogue of Library, Parts 1, 2. 8vo. 1878.
Proceedings, No. 102. 8vo. 1878.
Antiquaries, Society of—Proceedings, Second Series, Vol. VII. No. 6. 8vo. 1879.
Asiatic Society of Bengal—Proceedings, 1879, Nos. 2, 3, 4. 8vo.
Journal, Vol. XLVIII. Part 1. Part 2, No. 1. 8vo. 1879.
Astronomical Society, Royal—Monthly Notices. No. 8. 8vo. 1879.
Memoirs, Vol. XLIV. 4to. 1879.
Author—Thoughts on Theism. (K 103) 8vo. 1878.
Batavia Observatory—Meteorological Observations, Vols. II. III. (1869–75.) fol. 1878.

Bath Royal Literary and Philosophical Society—Catalogue of its Library, and of that of the Bath and West of England Society. 8vo. 1879.
Bavarian Academy of Sciences, Royal—Sitzungsberichte, 1879, Hefte 1, 2. 8vo.
Abhandlungen. Band XIII. Abth. 1, 2. 4to. 1879.
Dr. A. Baeyer, Festrede. 4to. 1878.
Meteorologische und Magnetische Beobachtungen. München, 1878. 8vo. 1879.
British Architects, Royal Institute of—1878–9; Proceedings, No. 17. 4to.
Transactions, No. 13. 4to.
Brown, A. Crum, Esq. M.A. (*the Author*)—On the Theory of Chemical Combination. (K 103) 8vo. 1879.
Cambridge Philosophical Society—Transactions, Vol. XII. Part 3. 4to. 1879.
Proceedings, Vol. III. Parts 3–6. 8vo. 1878–9.
Chemical Society—Journal for Aug. Sept. Oct. 1879. 8vo.
Cornwall Polytechnic Society, Royal—Forty-sixth Annual Report, 1878. 8vo. 1879.
Dawson, G. M. Esq. (*the Author*)—The Indians of Canada. (K 103) 8vo. 1879.
Dax: Société de Borda—Bulletins, 2e Série, Quatrième Année: Trimestre 2. 8vo. Dax, 1878.
Devonshire Association for the Advancement of Science, Literature, and Art—Report and Transactions, Vol. XI. 8vo. 1879.
Devonshire, The Duke of, K.G. D.C.L. F.R.S. M.R.I.—Catalogue of the Library at Chatsworth (with Introduction by Sir J. P. Lacaita). 4 vols. 8vo. 1879.
Editors—American Journal of Science for July–Oct. 1879. 8vo.
Analyst for July–Oct. 1879. 8vo.
Athenæum for July–Oct. 1879. 4to.
Brain: a Journal of Neurology, No. 2. 8vo. 1879.
Chemical News for July–Oct. 1879. 4to.
Engineer for July–Oct. 1879. fol.
Horological Journal for July–Oct. 1879. 8vo.
Iron for July–Oct. 1879. 4to.
Journal for Applied Science for July–Oct. 1879. fol.
Nature for July–Oct. 1879. 4to.
Telegraphic Journal for July–Oct. 1879. 8vo.
Edwards, Morton, Esq. (*the Author*)—Guide to Modelling in Clay and Wax, and for Terra Cotta, Bronze and Silver Chasing, &c. 8vo. 1879.
Franklin Institute—Journal, Nos. 643–646. 8vo. 1879.
Geographical Society, Royal—Proceedings, New Series. Vol. I. Nos. 8, 9, 10. 8vo. 1879.
Journal, Vol. XLVIII. 8vo. 1879.
Geological Institute, Imperial, Vienna—Verhandlungen, 1879, Nos. 7, 8, 9.
Jahrbuch: Band XXIX. No. 2. 8vo. 1879.
Abhandlungen: Band XII. Heft 1. fol. 1879.
Gill, T. and E. Coues—Bibliography of North American Mammals. 4to. 1877.
Harlem, Société Hollandaise des Sciences—Archives Néerlandaises. Tome XIV. Liv. 1, 2. 8vo. 1879.
Henry, Dr. James (*Trustees of*)—Æneidea or Critical, Exegetical, and Æsthetical Remarks on the Æneis, by James Henry. Vol. II. (Book III.) 8vo. 1879.
Hillebrand, Karl, Esq. (*the Author*)—Zeiten, Völker und Menschen. Band I. 8vo. 1879.
Geschichte Frankreichs (1830–71). Theil II. 8vo. 1879.
Institution of Civil Engineers—Minutes of Proceedings, Vols. LVII. and LVIII. 8vo. 1879.
Knox, J. J. Esq. (*the Author*)—Annual Report of the Controller of the United States Currency. (L 17) 8vo. 1878.
Linnean Society—Journal, Nos. 80, 102, 103. 8vo. 1879.
Transactions: Second Series. Botany, Vol. I. Part 6. Zoology, Vol. I. Part 8. 4to. 1879.
Lunacy Commissioners—Thirty-third Report. 8vo. 1879.
Manchester Geological Society—Transactions, Vol. XV. Parts 6, 7, 8, 9. 8vo. 1879.
Mechanical Engineers, Institution of—Proceedings, June, 1879. 8vo.
Meteorological Society—Quarterly Journal, No. 31. 8vo. 1879.

Montpellier, Académie des Sciences—Mémoires de la Section des Sciences. Tome VIII. Fasc. 2; Tome IX. Fasc. 2 (1877–8). 4to. 1879.
Moscrop, E. H. Esq. M.R.I. (*the Author*)—The Introduction of Salmon and Trout at the Antipodes. (K 103) 8vo. 1879.
Musical Association—Proceedings, Fifth Session, 1878–9. 8vo. 1879.
Norfolk and Norwich Naturalists' Society—Transactions, Vol. II. Part 5. 8vo. 1878–9.
Pangborn, J. G. (*the Author*)—Rocky Mountains, Arkansas Valley, and San Juan Guide. 4to. Chicago, 1878.
Pharmaceutical Society—Journal, July to Oct. 1879. 8vo.
Photographic Society—Journal, New Series, Vol. IV. No. 1. 8vo. 1879.
Plateau, Professor J. Hon. M.R.I.—Sur la Viscosité superficielle des Liquides. (Bulletins de l'Académie de Belgique, t. xlviii.) 8vo. 1879.
Preussische Akademie der Wissenschaften—Monatsberichte: März, April, Mai, Juni, 1879. 8vo.
Royal College of Surgeons of England—Catalogue of Specimens illustrating the Osteology of Vertebrated Animals in the Museum. By Professor W. H. Flower. Vol. I. 8vo. 1879.
Royal Irish Academy—Transactions: Vol. XXIV. Antiquities, No. 9. 4to. 1874.
" XXV. Science, Nos. 9, 10. 4to. 1874–5.
" XXVI. Science, Nos. 18–21. 4to. 1879.
" XXVII. Polite Literature, No. 2. 1879.
Proceedings, Series II. Vol. I. Nos. 11, 13; Vol. III. No. 3. 8vo. 1875–9.
Royal Society of Literature—Transactions, Vol. XII. Part 1. 8vo. 1879.
Royal Society of London—Proceedings, No. 196. 1879.
Saxon Society of Sciences, Royal—
Philologisch-Historische Classe:
Abhandlungen. Band VII. No. 5–8; Band VIII. No. 1. 4to. 1876–9.
Berichte. 1875, No. 2, 1876, 1877, 1878. 8vo.
Mathematisch-Physische Classe:
Abhandlungen. Band XI. Nos. 6, 7, 8; Band XII. No. 1. 4to. 1876–8.
Berichte. 1875, Nos. 2, 3, 4, 1876, 1877, 1878. 8vo.
Smithsonian Institution, Washington—Annual Report for 1877. 8vo. 1878.
Smithsonian Miscellaneous Collections, Vols. XIII. XIV. XV. 8vo. 1878.
Statistical Society—Journal, Vol. XLII. Parts 2, 3. 8vo. 1879.
St. Petersburg, Académie des Sciences—Mémoires. Tome XXVI. Nos. 5–11. 4to. 1879.
Symons, G. J.—Monthly Meteorological Magazine, July to Oct. 1879. 8vo.
Tasmania, Royal Society—Papers and Proceedings for 1877. 8vo. 1878.
Telegraph Engineers, Society of—Journal, Part 27. 8vo. 1879.
Tuson, Professor R. V. (*the Editor*)—Cooley's Cyclopædia of Practical Receipts. Part 14. 8vo. 1879.
University College, London—Catalogue of General Library, &c. Vols. I. II. 8vo. 1879.
Calendar for 1879–80. 8vo. 1879.
United Service Institution, Royal—Journal, Appendix to Vol. XXII. and Nos. 100, 101. 8vo. 1879.
United States Geological Survey (*through Dr. F. V. Hayden*)—Elliott Coues: Birds of the Colorado Valley. Part 1. 8vo. 1878.
Reports on Natural History, &c. (Author's Editions). 8vo. 1878–9.
United States Naval Observatory—Simon Newcomb: Researches on the Motion of the Moon. Part I. 4to. 1878.
Upsal University—Bulletin Mensuel de l'Observatoire Météorologique, Vol. X. 1878. Vol. XI. Nos. 1–6. 4to. 1878–9.
Verein zur Beförderung des Gewerbfleisses in Preussen—Verhandelungen, 1879. Hefte 6, 7, 8. 4to.
Victoria Institute—Journal, No. 50. 8vo. 1879.
Zoological Society of London—List of Vertebrated Animals in the Gardens. 7th ed. 8vo. 1879.
Proceedings, 1879, Parts 2, 3. 8vo.

GENERAL MONTHLY MEETING,

Monday, December 1, 1879.

THE DUKE OF NORTHUMBERLAND, LL.D. D.C.L. Lord Privy Seal, President, in the Chair.

Miss Henrietta Maria Adair,
Edward Greenhill Amphlett, Esq. M.A.
Henry Fearnside, Esq. M.B. F.R.C.P.
Major Edward Smith Gordon, R.A.
Thomas Henry Sanderson, Esq.

were *elected* Members of the Royal Institution.

The following Lecture Arrangements before Easter, 1880, were announced:—

CHRISTMAS LECTURES.

PROFESSOR TYNDALL, D.C.L. F.R.S.—Six Lectures on AIR AND WATER; on December 27 (Saturday), 30, 1879; Jan. 1, 3, 6, 8, 1880.

PROFESSOR EDWARD A. SCHÄFER, F.R.S.—Ten Lectures on THE PHYSIOLOGY OF MUSCLE; on Tuesdays, Jan. 13 to March 16.

H. HEATHCOTE STATHAM, Esq.—Two Lectures on MODERN ARCHITECTURE SINCE THE RENAISSANCE; on Thursdays, Jan. 15 and 22.

PROFESSOR DEWAR, M.A. F.R.S. — Eight Lectures on RECENT CHEMICAL PROGRESS; on Thursdays, Jan. 29 to March 18.

PROFESSOR T. RUPERT JONES, F.R.S.—Three Lectures on COAL; on Saturdays, Jan. 17, 24, 31.

PROFESSOR ERNST PAUER.—Three Lectures on HANDEL, SEBASTIAN BACH, AND JOSEPH HAYDN. With Musical Illustrations. On Saturdays, Feb. 7, 14, 21.

Four Lectures, on HISTORY or LITERATURE, on Saturdays, Feb. 28, March 6, 13, 20.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

Governor General of India:—
Geological Survey of India:
Memoirs, Vol. XVI. Part 1. 8vo. 1879.
Palæontologia Indica: Series II. Vol. I. 4; Series XIII. 1. fol. 1879.
Asiatic Society of Bengal—Proceedings, 1879, No. 7. 8vo.
Journal, Vol. XLVIII. Part 1, Nos. 1, 2. Part 2, No. 2. 8vo. 1879.
Description of New Lepidopterous Insects from the Collection of W. S. Atkinson. By W. C. Hewitson and F. Moore. Part 1. 4to. 1879.
Astronomical Society, Royal—Monthly Notices, Vol. XXX. No. 9. 8vo. 1879.
Brown, James F.Esq. F.C.S.—Apparatus, Past and Present: Engravings. (Sheet I.) 1879.

Chemical Society—Journal for Nov. 1879. 8vo.
Clinical Society—Transactions, Vol. XII. 8vo. 1879.
Editors—American Journal of Science for Nov. 1879. 8vo.
Analyst for Nov. 1879. 8vo.
Athenæum for Nov. 1879. 4to.
Chemical News for Nov. 1879. 4to.
Engineer for Nov. 1879. fol.
Horological Journal for Nov. 1879. 8vo.
Iron for Nov. 1879. 4to.
Journal for Applied Science for Nov. 1879. fol.
Nature for Nov. 1879. 4to.
Telegraphic Journal for Nov. 1879. 8vo.
Franklin Institute—Journal, No. 647. 8vo. 1879.
Geographical Society, Royal—Proceedings. New Series. Vol. I. No. 11. 8vo. 1879.
Geological Society—Quarterly Journal, No. 140. 8vo. 1879.
Geological Society of Ireland—Journal, Vol. XV. Part 2. 8vo. 1879.
Glasgow Philosophical Society—Vol. XI. Part 2. 8vo. 1878–9.
Jablonowski'sche Gesellschaft, Leipzig, Fürstliche—Preisschrift, No. 22. 8vo. 1879.
Mechanical Engineers, Institution of—Proceedings, August, 1879. 8vo.
Meteorological Society—Quarterly Journal, No. 32. 8vo. 1879.
Pharmaceutical Society—Journal for Nov. 1879. 8vo.
Physical Society—Proceedings, Vol. III. Part 2. 8vo. 1879.
Preussische Akademie der Wissenschaften—Monatsberichte: Juli, 1879. 8vo.
Rossetti, F.—Potere Assorbente e Potere Emissivo Termico delle Fiamme, &c. (Accademia dei Lincei, 1878–9). 4to. Rome. 1879.
Royal Medical and Chirurgical Society—Catalogue of the Library: Authors, 2 vols. Subjects, 1 vol. 8vo. 1879.
Transactions, Vol. LXII. 8vo. 1879.
St. Petersburg, Académie des Sciences—Bulletins, Tome XXV. No. 5. 4to. 1879.
Symons G. J.—Monthly Meteorological Magazine, Nov. 1879. 8vo.
Telegraph Engineers, Society of—Journal, Part 28. 8vo. 1879.
United Service Institution, Royal—Journal, No. 102. 8vo. 1879.
United States Naval Observatory, Washington—Results of Astronomical Observations, 1853–60. 4to. 1872.
Astronomical and Meteorological Observations, 1868, 1869, 1870. 3 vols. 4to. 1871–3.
Verein zur Beförderung des Gewerbfleisses in Preussen—Verhandelungen, 1879. Heft 9. 4to.
Yorkshire Archæological and Topographical Association—Journal, Part 20. 8vo. 1879.

Royal Institution of Great Britain.

WEEKLY EVENING MEETING,

Friday, January 16, 1880.

George Busk, Esq. F.R.S. Treasurer and Vice-President, in the Chair.

Professor James Dewar, M.A. F.R.S.
FULLERIAN PROFESSOR OF CHEMISTRY R.I.

(Abstract.)

Investigations at High Temperatures.

I intend to discuss on the present occasion the results of a preliminary study of the chemical interactions taking place at the temperature of the electric arc, and the inferences which can be deduced from a series of radiation experiments as to the probable temperature of this source of heat.

On the Formation of Hydrocyanic Acid in the Electric Arc.

The conclusion that the so-called carbon spectrum is invariably associated with the formation of acetylene,* induced me to try and ascertain whether this substance can be extracted from the electric arc, which invariably shows this peculiar spectrum at the positive pole, when it is powerful and occasionally intermittent. For this purpose the carbons were used in the form of tubes, as shown in the following figure, so that a current of air could be drawn by means of an aspirator through either pole, and the products thus extracted from the arc, collected in water, alkalies, and other absorbents. Gases may be led through one of the poles, and suction induced through the other, in order to examine their effect on the arc and the products obtained from it.

The following results were obtained by means of the Siemens and De Méritens magneto-machines, recently presented to the Royal Institution through the munificence of the Duke of Northumberland and Mr. Siemens :—

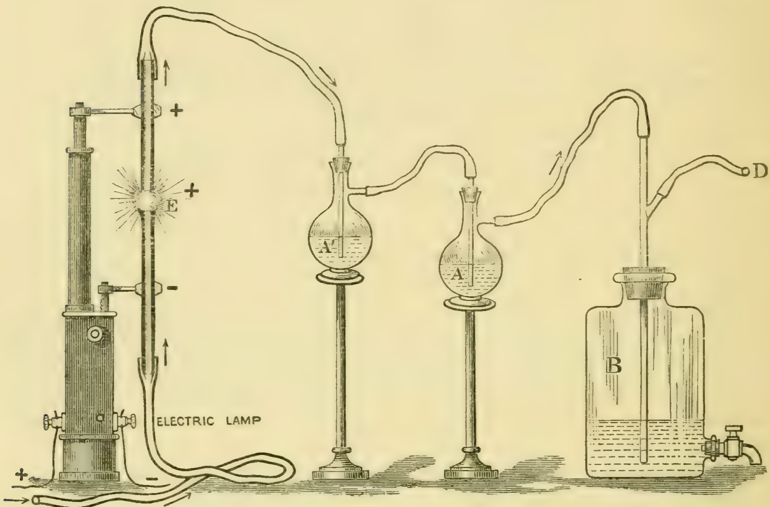
Air drawn by an aspirator from the arc through a drilled negative carbon, and the gases passed through potash, iodide of potassium, and

* As suggested by Plücker, Ångström, and Thalén.

starch paste, gave no reaction for the presence of nitrites. The potash contained sulphides.

Hydrogen led in through the positive pole, and the gases extracted as above gave the well-known acetylene compound with ammoniacal sub-chloride of copper; while, at the same time, a wash-bottle containing water gave distinct evidence of the presence of hydrocyanic acid.

Fig. 1.



A hydrogen flame burning between the carbon poles gave no sulphides or hydrocyanic acid, when treated in the above manner. The condensed water from the combustion gave the reaction for nitrites.

Air drawn through the negative carbon gave considerable quantities of hydrocyanic acid, which was greatly increased by extracting the gases through the positive carbon. Air was aspirated at the rate of about one litre per minute.

The same carbons used with the long arc of the De Méritens magneto-machine gave no hydrocyanic acid.

Carbons purified in chlorine and hydrogen gave with De Méritens' arc nothing; with Siemens' and a draught of air through the negative pole, a small quantity of hydrocyanic acid, but a larger yield when the positive pole was used. The gases extracted from the arc after the absorption of the hydrocyanic acid contained acetylene. If the carbons are not purified, sulphuretted hydrogen is always found along with the other gases.

The inference drawn from the above experiments is that the high temperature of the positive pole is required to produce the reaction, which is in all probability the result of acetylene reacting with free

nitrogen, as when induction sparks are passed through the mixed gases, viz.:—

$$C_2H_2 + N_2 = 2HCN,$$

and that the hydrogen is obtained from the decomposition of aqueous vapour, and the combined hydrogen in the carbons. It is possible that traces of alkaline salts in the carbon poles may favour the formation of hydrocyanic acid, but, as all attempts to purify the poles so as to stop the reaction failed, I am inclined to believe it is a direct synthesis. The acetylene reaction is one of the many remarkable syntheses discovered by Professor Berthelot, of Paris. The presence of sulphuretted hydrogen is doubtless due to the reduction of the sulphates, invariably present in the ash of the carbon.

The discovery of the formation of hydrocyanic acid in the electric arc necessitated a more complete examination of the various reactions taking place in the arc with poles of various kinds, and in presence of different gaseous media.

Various difficulties have impeded the satisfactory progress of the investigation. During the course, however, of numerous experiments, facts of interest have been recorded which are worthy of appearing as preliminary results in a very extensive and difficult research.

Formation of Cyanogen Compounds.

The influence of impurities in the carbon on the production of hydrocyanic acid had first to be ascertained. For this purpose, drilled Siemens' carbons were placed in a porcelain tube, and treated for several days at a white heat with a rapid stream of chlorine, until the greater part of the silica, oxide of iron, alumina, &c., were volatilized in the form of chlorides. Sometimes the carbons had a subsequent treatment with hydrogen, or were directly treated with a current of chlorine while the arc was in operation.

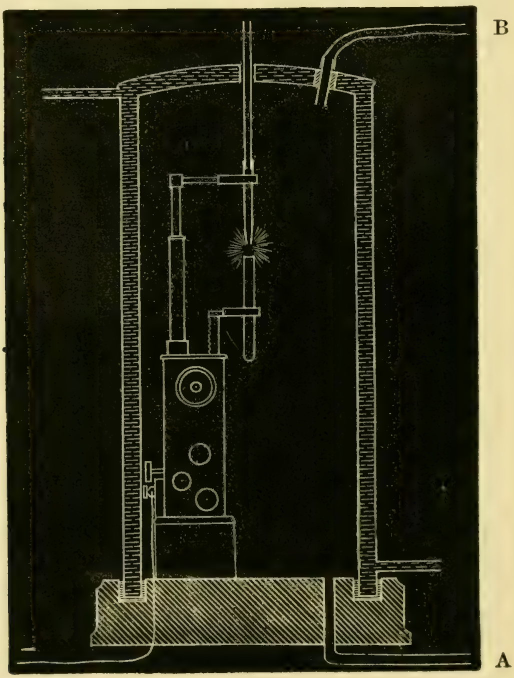
Carbons treated in this way continued to yield hydrocyanic acid, when a steady current of air was drawn through the positive pole as formerly described, even when the same pole had several successive treatments with chlorine during the electric discharge. Natural graphite poles gave the same result.

As it was evident that the elimination of a large portion of the impurities had little influence on the production of the hydrocyanic acid, the only other explanation of its formation appeared to be the presence of aqueous vapour, and organic impurities in the air, or a direct formation of cyanogen from carbon and nitrogen through the acetylene reaction formerly described. To obtain a pure and dry atmosphere in which such experiments could be carried out, the following apparatus was employed:—

A tin vessel, Fig. 2, about 2 feet high and 1 foot in diameter, had an annular space, through which a constant stream of water was kept flowing. This cylinder was placed upon a porcelain stand, having a narrow groove filled with mercury, so as to make an air-tight joint.

The lamp was placed inside this vessel, the wires connecting it with the machine being brought through the bottom of the stand. A tube passed through the porcelain base, which allowed a current of dry air to be forced through the vessel. A small aperture in the top of the tin vessel allowed the glass tube coming from the positive pole to pass with little friction, through which the products from the arc were drawn. This annular vessel was very convenient, not only for examining the products formed in the arc, but also those formed outside of it, and the water flowing round it served the double purpose of keeping it cool and enabling a determination of the amount of total radiation in heat units to be made.

Fig. 2.



Vessels containing pumice moistened with sulphuric acid and phosphoric anhydride were placed inside the cylinder in order to dry the interior as completely as possible.

Numerous experiments made by forcing perfectly dry air into the vessel through the tube A, and drawing it out by the tube B through a weighed sulphuric acid bulb, gave after an hour a few milligrams of increase, owing, no doubt, to some slight defect in the soldering of

the tin, which allowed a capillary film of water to cover part of the surface and diffuse into the interior.

When the ordinary Siemens' carbons were used as poles in this almost dry atmosphere, the yield of hydrocyanic acid was still very marked, purified carbons yielding the same results.

As the yield of cyanogen compounds did not appear to be diminished, and it seemed almost impossible to get the large volume of air in the tin vessel perfectly dry, another plan was adopted. The poles were enclosed in an egg-shaped glass globe about 8 inches long and 6 inches in diameter, in order to diminish the volume of air to be dried and dispense with the water covering. The globe, balanced through a system of pulleys, was firmly attached to the lower or negative pole, with which it moved without impeding the automatic action of the lamp.

Dry air was sometimes forced through the negative carbon itself, at other times through a glass tube passing up the side of it into the globe, the products from the arc being drawn through the positive pole as before.

As the glass globe soon became very hot, and as a far larger supply of dry air was forced through the globe than was drawn out from the arc, it is inconceivable that any moisture could remain near the arc after it had been in operation for a few minutes.

Seven consecutive experiments, each of ten minutes' duration, made with the same purified carbon poles, did not show any diminution in the quantity of hydrocyanic acid, unless in one of the experiments, when the arc would not be drawn into the interior of the carbon tube, but persisted in rotating round it.*

These experiments show that drilled carbons even after prolonged treatment with chlorine, still contained a quantity of combined hydrogen, and organic analyses showed that the amount of ash and combined hydrogen in the various samples was never less than about 0·75 of the former, and as much as 0·1 of the latter. Poles made with especially purified carbon by Messrs. Siemens for these experiments proved to be no better in respect to the quantity of hydrogen and ash they contained.

The well-nigh impossible problem of eliminating hydrogen from masses of carbon such as can be employed in experiments of this kind, proves conclusively that the inference drawn by Mr. Lockyer,† as to the elementary character of the so-called carbon spectrum from an examination of the arc in dry chlorine, cannot be regarded as satisfactory, seeing that undoubtedly hydrogen was present in the carbon, and in all probability nitrogen in the chlorine.

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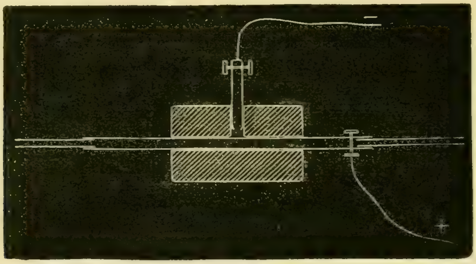
* Cyanogen is difficult to recognize in presence of prussic acid when in small quantity, especially when impurities from the carbons complicate the tests. In speaking generally of the formation of this acid in the arc, I do not mean to exclude the possibility of cyanogen being formed as well.

† "Note on the Existence of Carbon in the Coronal Atmosphere of the Sun," Proc. Roy. Soc.,' vol. xxvii. p. 308.

Experiments with Carbon Tubes.

In order to ascertain whether the formation of hydrocyanic acid and acetylene in the arc was really due to transformations induced by some occult power located in the arc, or was simply the result of the high temperature attained by the carbons, experiments were made in carbon tubes, the arc being merely used as a means of heating. The method of arranging the arc for this experiment is represented in Fig. 3.

Fig. 3.



A block of lime about 5 inches long by 3 inches thick was drilled horizontally, as shown in the drawing, another hole being drilled so as to meet it in the centre of the mass.

The new bricks used in the Bessemer converters do very well for all the experiments of this description.

A drilled purified carbon was placed in the horizontal channel and made the positive pole, the negative pole being a solid rod of carbon passing through the vertical aperture. Gases were passed through the positive carbon, and were thus subjected to the intense heat of the walls of the tube, the arc passing outside.

The walls of the positive carbon were pierced by the arc with great rapidity, not lasting, as a rule, more than fifteen minutes. This action could only be retarded by using thicker carbons, or by rotating the tube.

The porosity of the carbons, which allowed a constant diffusion of gases through their walls, was a great source of difficulty.

In order to prove that the temperature in the interior of the carbon tube is higher than that of the oxyhydrogen flame, it is sufficient to place in it a few small crystals of diamond, and to maintain a current of hydrogen to prevent oxidation. In a few minutes the diamond is transformed into coke.

On passing a mixture of three volumes hydrogen and one volume nitrogen thoroughly dried through the positive pole, a large yield of hydrocyanic acid was always obtained, and on using equal volumes of hydrogen and nitrogen the quantity was, if anything, increased.

Pure dry hydrogen by itself gave a trace of hydrocyanic acid, and a considerable quantity of acetylene.

Pure dry air gave no hydrocyanic acid or acetylene; moist air, on the contrary, giving abundance of the former, but only a trace of the latter.

The yield in all these experiments altered considerably with the rate at which the gases were passed, a quick stream always producing more than a slow one, unless when oxygen was present.

Formation of Nitrites in the Arc.

In these experiments the annular vessel was made use of, in which the lamp was allowed to work automatically, often for an hour or two. A continuous stream of dry air was kept circulating through the interior, being afterwards passed through a series of wash bottles containing dilute caustic soda, or directly through strong sulphuric acid, to absorb the oxides of nitrogen. The nitrous acid was estimated in the former case by titration with permanganate of potash, and the total combined nitrogen by the mercury process.

In this way many experiments were made with a Siemens lamp, both with a long and short arc; Jablochkoff's candles without any insulating material between the poles were also employed with the highest intensity current of a De Méritens machine, in order to have the greatest variety in the character of the discharge.

The stream of dry air was forced through the vessel at varying degrees of speed, and was found to have a decided effect on the quantity of nitrites produced, the more rapid stream giving the largest yield of nitrites.

The following table gives the amount of the nitrous acid produced in a number of different experiments.

The nitrites are calculated as nitrous acid.

1. Siemens' machine and lamp. 2. Jablochkoff's candles.

		Nitrites produced in 1 hour.		De Méritens' Highest Intensity Current.
		Siemens' Long Arc.	Siemens' Short Arc.	
		milligrams.	milligrams.	milligrams.
1st experiment	=	193	28	769
2nd ,,	=	804	97	723
3rd ,,	=	618	73	1225
4th ,,	=	500	121	548
5th ,,	=	622	90	955
6th ,,	=	474	85	1006
7th ,,	=	380	..	1257
8th ,,	=	459	..	964
9th ,,	=	..	..	664
10th ,,	=	..	..	489
11th ,,	=	..	..	693
	=	509 mean	..	930 mean

In these experiments, the total nitrogen estimated by the mercury process was almost identically the same as the amount of nitrogen obtained by a very careful dilution of the acid in a large quantity of water and titration with permanganate, proving that the main product was nitrous anhydride, which may be explained by the fact that the quantity of oxygen in presence of nitrogen in the immediate neighbourhood of the poles is greatly diminished by the combustion of the carbons, or that the nitric peroxide formed is subsequently reduced by contact with the red hot carbon, or other reducing products.

It is thus proved the carbon, nitrogen, oxygen, and hydrogen being present at the temperature of the electric arc, the compound substances hydrocyanic acid, acetylene, and nitrous acid are invariably formed.

Radiation Experiments.

In a report to the British Association * on the determination of high temperatures in the year 1873, it was experimentally proved that the law of Dulong and Petit could not be used as a basis for the estimation of high temperatures, seeing that it "gives a far too rapid increase for the total radiation." It was further observed that the value of the radiation emitted by the same substance at different temperatures expressed in terms of the thermo-electric current increase of intensity, plotted in terms of the temperature, represented a "parabolic curve." Assuming the general accuracy of this law for high temperatures, the total radiation may be taken as nearly proportional to the square of the temperature. From this law the hypothetical temperature of the sun was "estimated as at least 11,000 C." Rosetti has recently made a more elaborate investigation on the subject, and has arrived independently at a formula of a parabolic order. Rosetti † represents his results by the equation—

$$\mu = a\,T^2 (T - \theta) - b\,(T - \theta),$$

where μ is the total radiation measured by intensity of thermo-electric current, T° the absolute temperature of the source, θ° that of the medium surrounding the pile, and a and b constants. However well this formula may represent the complete series of the experiments, it is certain that his results for temperatures above 150° may be expressed within the limits of probable error as proportional to the square of the temperature. To be convinced of this, it is sufficient to plot the logarithm of the respective values of the radiation and temperature, when it will be found the results arrange themselves in a straight line, the tangent of which may be 1·9 or 2 for the observations above 150°. Experiments made with the thermopile, surrounded with an annular vessel, through which a continuous current of water

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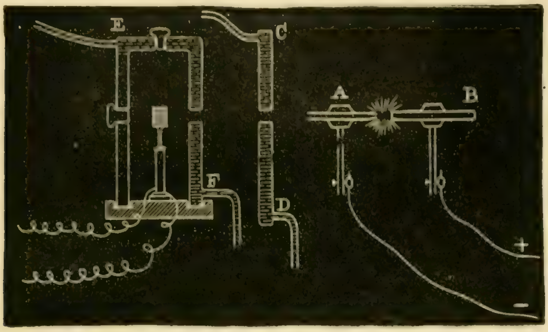
* Report of the Committee for determining High Temperatures by means of the Refrangibility of the Light evolved by Fluid or Solid Substances. Bradford, 1873. Page 461.

† "Recherches Expérimentales sur la Température du Soleil" (Accad. R. dei Lincei. 1877–78).

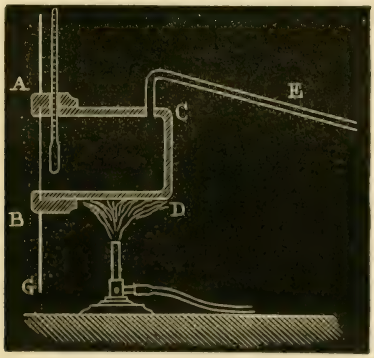
at constant temperature is caused to circulate, as represented in Fig. 4, where EF represents the section of the vessel, and CD a large water screen, on the same plan, each having a narrow opening, about half an inch in diameter, through which the radiant heat passed to the pile, have confirmed the earlier results. The vessel holding the

FIG. 4.



mercury or other substance to be heated to different temperatures has a radiating face, which was made of the sheet iron used in the construction of telephone plates, and the thermometer must be placed close to the back of the front surface, and the face guarded with a screen, FG. The tube, CE, is connected with a condenser, when substances at their boiling point are employed for giving fixed points. The form of the apparatus is shown in Fig. 5.

FIG. 5.



This arrangement of the apparatus is necessary in order to get anything like comparable results. The two following tables give the records of two series of experiments, without any correction being

made in the numbers representing the deviations of the Thomson galvanometer :—

TABLE I.

Temperature.	Deviation.	Difference.	Temperature.	Deviation.	Difference.
°			°		
80	32	6·5	160	95·5	10·5
90	38·5	6·0	170	106	11·0
100	44·5	6·5	180	117	13·5
110	52	7·5	190	130·5	13·0
120	59·5	7·0	200	143·5	14·5
130	66·5	9·0	210	158	14·5
140	75·5	9·5	220	172·5	15·5
150	85	10·5	230	188	

TABLE II.

Temperature.	Deviation.	Temperature.	Deviation.
°		°	
100	21	200	71
120	29	220	86
150	41	355	240
160	46	448	370
180	57		

If the differences in the galvanometer readings for every ten degrees in the first table be tabulated, it will be observed the second difference may be regarded as constant, considering the errors of this kind of observation. A parabolic formula can therefore represent the results with sufficient accuracy. These second differences are far more constant than similar numbers deduced from Rosetti's observations, and his more complete formula in terms of the absolute temperature is too extensive, considering the range of the experiments where temperature was accurately known. The results of Table II. extend to the boiling points of mercury and sulphur, and the numbers are in near accord with the simple square of the temperature. The alteration in the condition of the radiating surface at high temperatures causes great complications, and until this difficulty is overcome, experiments at high temperatures must remain uncertain. All the experiments show that for an approach to a knowledge of temperatures beyond the range of our actual thermometric scale, the law given in 1873 is a sufficiently correct reproduction of the facts, considering the limited data at our disposal.

The intensity of the radiation of the positive pole of the Siemens' arc, as compared with the same surface heated with a large oxy-hydrogen blowpipe, was determined by employing a hollow negative carbon which allowed the intensely heated surface to radiate directly on to the pile, as shown in Fig. 4. A large number of observations

have been made by this method at different times, and with slight modifications in the order of the experiments, leading to the average result that the intensity of the total radiation of the positive pole of the Siemens' arc is ten times that of the same substance at the temperature of the oxyhydrogen flame. If we take an average result of nine to one, then we may infer that the temperature of the limiting positive pole is about 6000° C., seeing that the mean temperature of the oxyhydrogen may be taken as 2000° C. The mean value of the total radiation of the Siemens' arc was determined by observing the rate of flow of the water through the annular vessel, represented in Fig. 1, together with the mean increment of temperature. This gave on the average 34,000 gram-units per minute, or a little more than three horse-power.

[J. D.]

WEEKLY EVENING MEETING,

Friday, January 23rd, 1880.

Thomas Boycott, M.D. F.L.S. Manager, in the Chair.

Dr. William B. Carpenter, C.B. F.R.S.

Land and Sea considered in relation to Geological Time.

When, in the summer of 1871, I placed before the First Lord of the Admiralty (Mr. Goschen) the scheme of the 'Challenger' Expedition, I ventured to say that "the key to the interpretation of much of the past history of our globe is at present lying at the bottom of the sea, waiting only to be brought up." This prediction has been most fully verified; but, as in the case of many another prophecy, in a sense very different from that in which it was uttered.

The first of the general objects specified in my programme was "the determination of the *Physical* condition of the Deep Sea in the great Ocean Basins, as to depth, temperature, composition, and movement," carrying out, over the Oceanic area generally, the inquiry which had been inaugurated by my colleagues and myself on the eastern margin of the North Atlantic. This object has been most successfully accomplished, by a series of observations taken along well-selected lines in the North and South Atlantic, the North and South Pacific, the Southern and Antarctic Oceans; which, combined with the observations taken in the recent Arctic expeditions—British, German, and Norwegian—afford a body of information as to the Physics of the Ocean, sufficiently complete to afford a safe basis for the scientific discussion of the remarkable phenomena now for the first time brought into clear view.

The second of the general objects which I specified was the determination of "the distribution of *Animal Life* on the Deep-sea bottom, and the relation of the Deep-sea Fauna to that of past Geological epochs." The inquiries previously carried out by my colleagues and myself had shown (1) that there is probably no limit to the *depth* at which Animal life can exist on the ocean-bed—a Fauna containing representatives of all the principal types of marine Invertebrates, having been found nearly *three miles* beneath the surface; (2) that *temperature* exerts a most important influence on the distribution of animal life on the sea-bottom; and (3) that many of the forms now

existing on the deep-sea bed so nearly represent Cretaceous types supposed to have long since become extinct, that we may fairly suppose them to be their lineal descendants. Hence, I went on to say, "the question of the continuity of 'descent with modification' will probably receive more elucidation from the study of the Deep-sea Fauna, than from any other line of scientific inquiry." This anticipation, also, is in course of complete fulfilment. An enormous amount of Zoological material has been carefully collected from various parts of the great Oceanic area, and at depths ranging downwards to from three to five miles; and this is being studied, with a view to all the determinations I have indicated, by Naturalists of the highest competency in their respective departments. The results of this part of the inquiry have so far been only disappointing to those who had somewhat unreasonably expected that, because Cretaceous types had been found still living in the deep seas of our part of the globe, the Ammonites of the Secondary period, and even the Trilobites of the Palæozoic, might be lurking in abyssal depths elsewhere,—an expectation which I never myself shared.

But whilst the past history of Animal Life on our globe will doubtless receive all the new light which I had anticipated from the scientific study of the 'Challenger' collection, an unexpected clue has been found in the examination of the *sediments* now in process of deposition on the Ocean-bottom, to the solution of a question in Physical Geology, second to none in importance and interest, which I propose now to bring before you.

Every tyro in Geology knows it to be a fact not admitting of a doubt, that all our existing Land has at some period or other been under the sea; and the converse proposition—that every part of the Sea-bottom has at some period or other risen above the surface—has been very generally accepted, even by geologists of the highest eminence. Thus Sir Charles Lyell, in his chapters on the vicissitudes in Climate caused by geographical changes, assumed it as a fact beyond dispute, not only "that every part of the space now covered by the deepest ocean has been land," but even that "the bed of the ocean has been lifted up to the height of the loftiest mountains;" and considered it proved that "if we had a series of maps, in which restorations of the physical geography of thirty or more periods were depicted, they would probably bear no more resemblance to each other, or to the actual position of land and sea, than does the map of one hemisphere bear to that of the other."—These statements, I may remark, are repeated without any qualification in the twelfth chapter of the latest edition of his masterly 'Principles'; notwithstanding that towards the conclusion of the same chapter, he distinctly recognized the enormous disproportion between the average elevation of the Land and the average depth of the Ocean-basins, whereby, while a vertical *depression* of 1000 feet would submerge a large part of the present continental land, a vertical *elevation* of from twelve to fifteen

times that amount would be required to raise any large areas of the ocean-bed above the existing sea-level.

Many Geologists who would not accept in all their fulness Sir C. Lyell's rather sweeping assertions, seem by their language to imply their belief in less extensive interchanges between Land and Sea; in fact, I think a general belief has been entertained of a sort of see-saw movement in the Earth's crust,—one portion going up while another goes down,—which has seemed to draw confirmation from Mr. Darwin's admirable researches on Coral Islands.

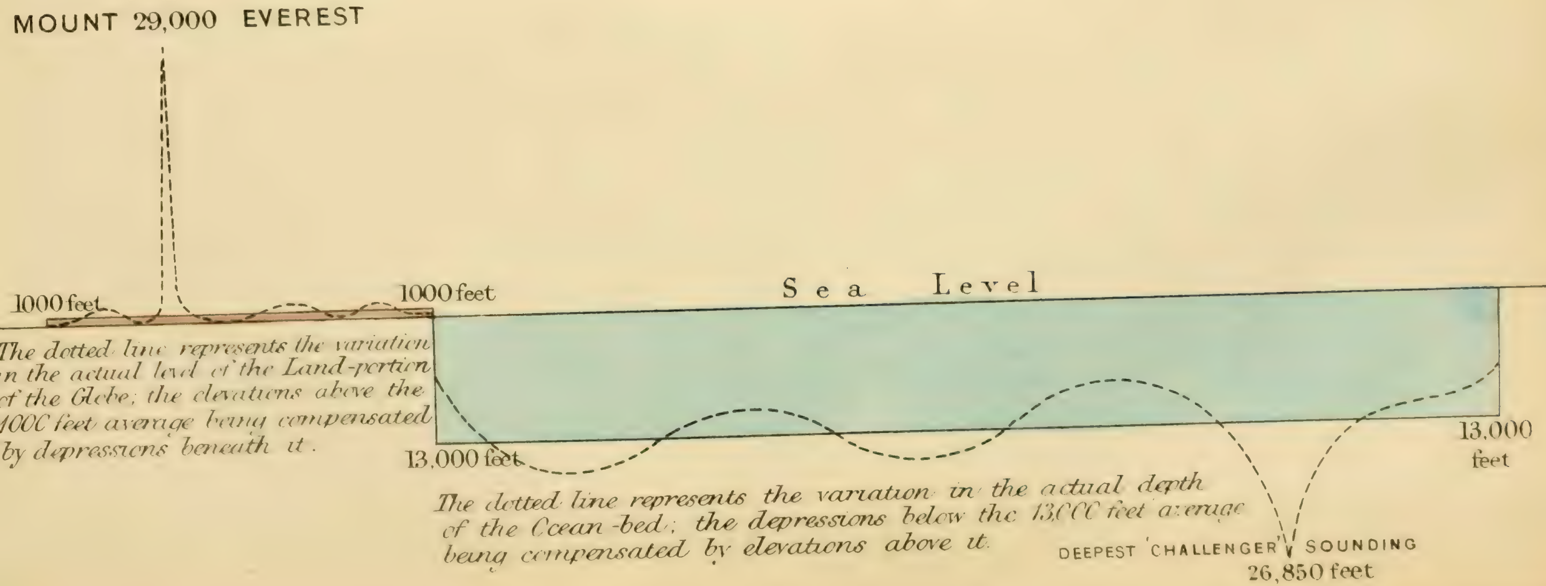
Some of the ablest among living Geologists, on the other hand, have been led by the convergence of several independent lines of inquiry—of which it is my purpose to give you a concise sketch—to a belief in the *permanence*, throughout all geological time, of what may be called the framework of the existing Continents, on the one hand, and of the *real* Oceanic basins on the other. According to this view, the repeated changes which have unquestionably occurred at various periods in the distribution of sea and land, have been generally produced by elevations and subsidences, for the most part of very moderate amount, in portions of elevated areas in the original crust of the earth, which occupied the general position of our existing Continents; the upheaval of lofty mountain-chains, and the formation of very deep local troughs, in which long successions of sedimentary deposits have been formed, having taken place in parts of those originally elevated areas, especially near their margins. The far larger Oceanic basins on this view, occupy areas of the crust which were originally depressed by an abrupt border, many thousands of feet beneath the continental platforms; and, like them, had a nearly uniform level, until disturbed by local upheavals and depressions occasioned by forces subsequently generated during the progressive contraction of the molten sphere within—these upheavals and depressions, when considerable vertically, being usually limited in area, and only breaking the general uniformity of bottom-level as the elevation of the Ural chain interrupts the uniformity of the great plain of north-east Europe and northern Asia.

I. Now the first consideration to which I would draw your attention, is the enormous disproportion which we now know to exist between the *depth* of the real Ocean-floors beneath the sea-level, and the *height* of the Land elevated above it; which, when taken in connection with the relative *areas* of the existing Sea and Land, seems to render it highly improbable that interchanges extending over large portions of the earth's surface could ever have taken place between them.—The proportion which the *area* of the existing Land bears to that of the Sea may be conveniently stated as about 1 to $2\frac{3}{4}$, or as 4 : 11; so that, if the entire surface of the globe were divided into fifteen equal parts, the Land would occupy only four of these, or rather more than a quarter, whilst the Sea would cover eleven, or rather less than three-quarters. But when we compare the *volume* of

Proportional Area of Sea to Area of Land, 11 to 4, or 2¾ to 1.

VERAGE DEPTH OF OCEANIC AREA 13,000 FEET
VERACE ELEVATION OF LAND, 1,000 FEET

PROPORTIONAL MASS OF SEA, 11 × 13 = 143 } OR { 36 TO 1
LAND, 4 × 1 = 4



the Land above the sea-level with that of the Water which occupies the Ocean-basins, a far greater disproportion shows itself. For the average elevation of the whole Land of the globe certainly does not exceed 1000 feet;—that of Asia and Africa being somewhat above that amount, while that of America (North and South), Europe, and Australia is considerably below it. On the other hand, the average depth of the Ocean-basins is now known to be rather over than under 2½ miles, and may be taken (for the convenience of a round number) at 13,000 feet. Thus the average depth of the ocean being thirteen times as much as the average height of the land, and the area of the sea being 2¾ times that of the land, *the total volume of the Ocean-water is just thirty-six times that of the Land above the sea-level.*

The Northern hemisphere is pre-eminently the *land* hemisphere, and the Southern the *water* hemisphere; and the distribution of the two components of their respective surfaces, so far from being "capricious" (Lyell), is found to have a remarkable symmetry. It is between lat. 30° and 70° that Water most predominates in the Southern hemisphere; the Southern Ocean forming a continuous girdle around it between Cape Horn, lat. 56° S., and the Antarctic continental platform. On the other hand, it is between lat. 30° and 70° that Land most predominates in the Northern hemisphere, girdling nine-tenths of its circumference between lat. 60° N. and the Arctic Ocean.

The great land-masses of the Northern hemisphere send down three extensions into the Southern, viz. South Africa, South America, and the Papuo-Australian continent; which last may be considered as the southward extension of the Asiatic, being connected with it by a nearly continuous though partly submerged continental platform, of which the peninsula and archipelago of Malaya are the most elevated portions. It is further remarkable that each of these southward extensions is almost entirely detached from its northern land-mass by an intervening sea;—South from North America by the Gulf of Mexico and Caribbean Sea; Africa from Europe and Asia by the Mediterranean and Red Seas; and the Malayan continental platform from south-east Asia by the shallow Yellow Sea, and by those smaller seas, some of them remarkable for their depth, that lie among the great islands of the Malay Archipelago—*the interruption in each case coinciding with an area of great Volcanic activity.*

On the other hand, the vast Oceanic area of the Southern hemisphere sends three great extensions northwards; the Pacific, the Atlantic, and the Indian Oceans, of which the two former are prolonged as far as the North Polar area.

But the existing borders of these Oceans by no means correspond with the borders of their real basins. The deep-sea soundings of the 'Challenger' have brought out this remarkable fact—that the ocean-floors present a uniformity of level which corresponds with that of our most level and extensive Continental plains; so that in long section-lines the differences of depth (when represented on true pro-

portional scale*) show themselves—except in cases of local disturbance—as undulations of scarcely perceptible gradient.

Again, we now know that the borders of these vast depressed areas are generally, if not uniformly, very abrupt; the *sudden* descent from a comparatively shallow bottom to a very deep one, which was first noticed in the line of soundings taken with a view to the laying of the Atlantic Telegraph cable, being not an exceptional but a general fact. Taking this as the *real* border of the North Atlantic Ocean, and looking to the smallness of the gradients presented by its sea-bed (except in the volcanic area of the Azores) until we come upon the like steep inclination at some distance from the American coast-line, which obviously marks the true western border of the oceanic area, we see that the term "basin" is a misleading one; a far truer representation of the Atlantic depression being a flat "waiter" with elevated sides, having an upward bulge along the median line of its bottom. On this view, the shallow band which generally intervenes between the edge of a deep Oceanic depression and the ostensible coast-line, is really to be regarded as a submerged portion of the adjacent Continental platform.

The contrast between the *real* and the *ostensible* borders of the Ocean-basins is nowhere more remarkably exhibited than in the seas which girdle the British Islands. These are all so shallow, that their bed is undoubtedly to be regarded as a continuation of the European Continental platform; an elevation of the north-western corner of which, to the amount of only 100 fathoms, would reunite Great Britain to Denmark, Holland, Belgium, and France, and would bring it into continuity with Ireland, the Hebrides, and the Shetland and Orkney Islands. Not only would the whole of the British Channel be laid dry by such an elevation, but the whole of the North Sea also, with the exception of a narrow deeper channel that lies outside the fiords of Norway. Again, the coast-line of Ireland would be extended seawards to about 100 miles west of Galway, and that of the Western Hebrides to beyond St. Kilda; while a little further west, the sea-bed shows the abrupt depression already spoken of as marking the commencement of the real Atlantic area. A like rapid descent has been traced outside the 100-fathom line in the Bay of Biscay (a considerable part of which would be converted into dry land by an elevation of that amount), and along the western coast of Spain and Portugal, where, however, it takes place much nearer the existing land-border. The soundings of the U.S.S. 'Tuscarora' in the North Pacific, have shown that a like condition exists along the western coast of North America: a submerged portion of its Continental platform, covered by comparatively shallow water, forming a belt of variable breadth outside the existing coast-line; and the sea-bed then descending so rapidly as distinctly to mark the real border of the

* The use of a *vertical* scale very many times as great as the *horizontal*, tends to mask this important fact.

vast Pacific depression. And as similar features present themselves elsewhere, it may be stated as a general fact that *the great Continental platforms usually rise very abruptly from the margins of the real Oceanic depressed areas.*

If, on the other hand, we inquire what would be the effect of a *depression* of the existing Land of northern Europe to the same, or even half that amount, we find that very extensive areas of what is now dry land would be overflowed by sea; the higher tracts and mountainous regions alone remaining as representatives of the Continental platform, to which, nevertheless, the submerged portions equally belong. This, as every geologist knows, has been, not once only, but many times, the former condition of Europe; to which a singular parallelism now shows itself in that great Continental platform, of which the peninsula and islands of Malaya are the most elevated portions. For the Yellow Sea, which forms the existing boundary of south-eastern Asia, is everywhere so shallow, that an elevation of 100 fathoms would convert it into land; while half that elevation would lay dry many of the channels between the Malay Islands, so as to bring them into continuity not only with each other, but with the continent of Asia. And Mr. Wallace's admirable researches on the zoology of this region have shown that such continuity undoubtedly existed at no remote period; its Mammalian fauna being essentially Asiatic. On the other hand, a like elevation would bring Papua into land-continuity with Australia; with which, in like manner, the intimacy of its zoological relations shows it to have been in former connection. The Indo-Malay province is separated from the Papuo-Australian province by a strait, which, though narrow, is so much deeper than the channels which intervene between the separate members of either group, that it would still remain as a fissure of considerable depth, even if the elevation of the two parts of the great area it divides were sufficient to raise each into dry land. The Malayan land-area would, however, be still broken by small Inland Seas of extraordinary depth. One of these, known as the Sulu Sea, which lies between the north-west coast of Borneo and the Philippines, and is elsewhere enclosed by smaller islands and reefs connecting them, ranges downwards to 2225 fathoms. Another, the Celebes Sea, which lies to the west of Borneo between Mindinao and Celebes, has a depth of 2050 fathoms. And the Banda Sea, which lies between the southern part of Celebes and New Guinea, with the islands of Ceram on the north and Timor to the south, has the still more extraordinary depth of 2800 fathoms. A general elevation of a few hundred fathoms would detach these Seas from the two great Oceanic areas which they now help to connect *; and yet they would still remain by far the

* The depth down to which each of them communicates with the Ocean outside, is determinable by its correspondence in temperature. Below the plane of continuity, the temperature of the enclosed Sea remains constant to the bottom (as in the Mediterranean), while that of the Ocean shows a continuous descent.

deepest of the smaller depressions anywhere occurring in Land-areas.

The occurrence of these gigantic pit-holes in this region of extraordinary Volcanic activity has a singular significance; especially when taken in connection with the fact that like depressions of the Ocean-bed which have been elsewhere met with, are also in Volcanic areas. Thus the first of the 'Challenger' soundings which showed a depth (3875 fathoms) greatly exceeding that of the ordinary floor of the Atlantic, was made not far north of St. Thomas's, in what may be regarded as a continuation of that "line of fire" which is so marked in the lesser Antilles. The sounding-wire of the United States ship 'Tuscarora' twice broke, without reaching bottom, in near proximity to the volcanic region of Japan, at depths considerably exceeding 4000 fathoms. And the deepest bottom sounded by the 'Challenger,' 4575 fathoms or 27,450 feet,—which seems to have been a local depression of a sea-bed averaging about half that depth, and was met with on the passage between New Guinea and Japan, not far from the Ladrone Islands,—was also presumably in a line of volcanic disturbance.

Again, the 'Challenger' observations enable it to be affirmed with confidence, that wherever Land shows itself * in the great Oceanic area, forming what are distinguished as "oceanic islands" from those which are merely outlying portions of continental platforms, those islands are all *volcanic;* their elevation having been due to forces acting only in limited spots or over particular lines, and not to any general uplifting of the bottom of the basin. So, on the other hand, the contours of the Deep-sea bed, so far as they have been determined, give no countenance whatever to the notion of such a general subsidence as would have produced the submergence of a great Continental platform in any part of the vast Oceanic area; and this negative conclusion receives striking confirmation (as will hereafter appear) from the entire absence, in the sediments at present in process of deposition at a distance from existing continental land, of any traces of land-degradation.

II. The progress of Geological inquiry has now made it apparent that the movements of elevation that have occurred from time to time in various parts of the Land-areas of the globe, have been the result of forces acting in two different directions—*vertical* and *horizontal.* Extensive platforms, of which European Russia affords a conspicuous example, have been several times raised into land (with alternations of depression) by a force that seems to have operated *directly upward;* and with such uniformity over a vast area, as to have produced very

* As in all the Coral islands in which basal rock shows itself, that rock is Volcanic, the same may fairly be presumed to be the character of the submerged peaks on which those "atolls" rest, above whose level platforms no rocky base now rises.

little change in the relative levels of its different parts. These alternations of depression and elevation have all been apparently of very moderate vertical amount. Over the vast area of Russia, we find, as a rule, that the sediments which have been successively deposited upon it exhibit a most regular stratification, and have undergone little or no metamorphic change; Silurian clay-slates being represented by hardened clay; and Carboniferous limestone showing itself as an aggregate of compacted (foraminiferal) *Fusulinæ.*

On the other hand, a force acting *horizontally* against the margin of a previously level area, will throw it into plications, of which the elevated portions will form mountain-ranges. The strata forming these ranges, which show by their contorted condition the enormous lateral thrust to which they have been subjected, always exhibit more or less of metamorphic change; and this metamorphism is now generally regarded as the effect of heat, acting in conjunction with moisture, and usually under pressure.* The source of this heat is to be found in the very mechanical energy which effects the plication; resistance to which, as in ordinary friction and compression, will cause it to take that converted form. This plicating process acts along definite lines and bands, the width of which is usually small in proportion to the vast area of the wide continental platforms; and thus it happens that notwithstanding the enormous height to which the most elevated peaks may be lifted (Mount Everest 29,000 feet), little is added by Mountain-making to the *average* level of any great continent. But, again, the operation of this lateral thrust is now generally recognized, not merely in the elevation of mountain-ranges, but also in Volcanic action; the fusion of the compressed rocks being, in fact, only a further stage of metamorphism, and being fairly attributable, like it, to the production of heat by the conversion of mechanical force.

III. The recent progress of Physical Astronomy, again—mainly through the application of the Spectroscope to the study of the physical and chemical conditions of celestial bodies in various stages of aggregation—seems now to have placed it beyond reasonable doubt that the earth has cooled down from the state of a molten mass; and the probable effect of the progressive cooling and shrinkage of its interior, upon the conformation of the crust which first solidified around it, have been very carefully worked out by Professor Dana;† an outline

* Professor Hull, the able Superintendent of the Geological Survey of Ireland, has shown that in the level tract of Carboniferous Limestone which there forms a great central plateau, the organic origin of the limestone is very distinct; whilst in these upheaved and contorted strata of the same rock which form the elevated borders of that plateau, the organic origin of the limestone is completely obscured by metamorphic change, which has given it a sub-crystalline texture.

† See the chapters on "Dynamical Geology," in the Second Edition of his 'Manual of Geology' (1875); and, for a fuller exposition of his views, his Memoir in the 'American Journal of Science,' June to September, 1873.

of whose theoretical views will show how entirely they harmonize with the conclusions drawn from inquiry into the present conditions of the great Oceanic areas:—"As the globe has cooled from fusion, it has been all through time a contracting globe; and this contraction of the crust has been the chief agency in determining the evolution of the earth's surface-features, and the successive phases in its long history." "The crust which should form over a melted sphere, as it cooled, would have the size the sphere had at the time. As it thickened downwards by the continued cooling, the added portions would contract; and this would occasion lateral pressure through the crust, which would increase as the cooling and thickening continued." Reasons are adduced by Professor Dana for the belief that the formation of the solid crust would not go on at the same rate all over the sphere; but that some portions of the surface would solidify into a layer several miles in thickness, whilst over other large areas the surface would still be liquid or in a state of only incipient solidification. The level of the latter would be gradually lowered by the contraction of the cooling mass beneath; and the crust of these depressed areas would constitute the Ocean-floors, whilst the elevated areas, rising by abrupt sides from their borders, would remain as Continental plateaux. The study of the geological structure of the North American continent leads Professor Dana to the conclusion that "in its very inception, not only was its general topography foreshadowed, but its great mountain-chains appear to have been begun, and its great intermediate basins to have been defined. The evolution of the grand structure-lines of the continent was thus early commenced, and the system thus initiated was the system to the end. Here is one strong reason for concluding that the continents have always been continents; that while portions may have at times been submerged some thousands of feet, *the Continents have never changed places with the Oceans.*"

The progressive shrinkage of the internal mass, as its cooling proceeds, must produce a falling inwards of the crust formed around it; and the lateral pressure thus exerted through the whole crust will necessitate a yielding somewhere. The *lateral thrust* is likely to be exerted most advantageously from the floors of the depressed Oceanic areas against the sides of the elevated Continental plateaux; and this is borne out by the fact that "the continents have mountains along their borders, while the interior is generally low"; and that "the volcanoes of the continental areas are mostly confined to the sea-borders." Further, "the largest and loftiest mountain-chains, greatest volcanoes, and other results of uplifting and disruptive force, characterize the borders of the *greatest* oceans, showing that the lateral pressure from the direction of the oceans was approximately proportional to the extent of the oceanic basins." Thus, in North America the lofty and massive Alleghanies are raised up on the Pacific side; the minor Appalachian chain on the Atlantic. In South America, the great chain of the Andes, with its lofty volcanoes, is in like contrast with the comparatively insignificant mountains of Brazil. So, on the Euro-

pean side of the Atlantic, the mountains which border the Oceanic basin correspond in scale with those on its western border, rather than with those on the Pacific slope of the American continent. On the western side of the Pacific, on the other hand, the Malayan Archipelago constitutes (as already pointed out) a region of extraordinary volcanic activity; and this is probably the greater on account of the comparative narrowness of this continental plateau, so that it is subject to the lateral thrust of the sea-bed of the Indian Ocean in addition.

But the lateral thrust exerted by these floors, being resisted by the buttresses presented by the continental plateaux, will tend to produce an upward bulging of these floors themselves, especially in their median portion. And this, again, corresponds with fact; such an upward bulging showing itself in the median portion of the bed of the Atlantic, both north and south; while the force which raised this, also manifests itself in the volcanic action which has pushed up the Azores and Tristan d'Acunha in corresponding positions. So, in the North Pacific, we have the remarkable volcanic Hawaian group, occupying the same relative position as the Azores in the North Atlantic; while over the still wider expanse of the South Pacific, there seem to be several of these upward bulgings, that have exploded (so to speak), here and there, in local volcanic action.

I must not follow Professor Dana's masterly hypothesis into further detail, but must content myself with noticing one point which seems to me of singular interest—namely, the explanation he gives of the depression of portions of what he regards as the original continental platforms, over which long series of sedimentary deposits have been formed, of course implying a subsidence of their base to an amount at least equal to their total thickness. The first step in ordinary mountain-making by lateral thrust, is affirmed by Professor Dana to be a *downward* bend of the crust, or "geosynclinal." "In the making of the Appalachians, there was first, under the lateral pressure, a slowly progressing subsidence; it began in, or before, the Primordial period, the commencing era of the Silurian, and continued in progress until the Carboniferous age closed. As the trough deepened, deposits of sediment, and sometimes of limestone, were made, that kept the surface of the region near the water-level; and when the trough reached its maximum, there were 40,000 feet of thickness of stratified rock in it, and this, therefore, was the depth of the trough. The Green Mountains began in a similar subsidence, and at the same time; and the trough was kept full with deposits as it progressed. Such facts are in the history of many, if not all mountains."

The foregoing arguments may be thus combined:—

A. The enormous depth of the Oceanic sea-bed, as compared with the height of the Land above the sea-level, renders it very unlikely that any subsidence of a Land-area should be compensated by such an uplifting of a portion of the Ocean-floor as would raise it above that level. Thus, supposing that all the Land of the globe were to sink down to the

sea-level, such subsidence would be balanced (according to the current idea of compensatory alternation) by an elevation up to that level of a portion of the average Ocean-floor, amounting to no more than 1–36th of its existing area. On the other hand, the sinking of such an area as that of Papuo-Australia (which forms about 1–17th of the existing land-surface) to the depth of the average *Ocean-floor*, would require to balance it an elevation of the whole remainder (13–14ths) of the existing Land to *double* its present average height above the sea-level.

B. Wherever the uniform elevation of an extensive Land-area indicates its upheaval by a force acting *vertically* throughout, the amount of such elevation seems to have been very limited,—no such level area showing itself at any considerable height above the sea. Conversely, there is no adequate reason to believe that any extensive area has ever uniformly subsided beneath the sea-level, to any greater depth than that at which lie the submerged portions of some existing Continental platforms.

C. On the other hand, all *great* elevations, whether rising from Continental platforms or from the Oceanic sea-bed, are clearly attributable to *lateral* thrust; and such are everywhere of very limited extent, forming mountain-chains or high table-lands in Continents, and volcanic islands in the Oceanic area,—in neither case having the least resemblance to continental plateaux. And, conversely, the very deep depressions in which long series of stratified deposits have accumulated, only occur as consequences of the lateral thrust which produces plication, and which elevates mountain-ranges as part of the same operation. Local subsidences of this kind, therefore, give no support to the idea of such vast *general* subsidences, as would be required to create a deep Oceanic depression over any area now occupied by a Continental platform.—Inland seas, in fact, may be regarded as troughs of this kind, which have been formed in regions of extraordinary disturbance, in which the troughs have been formed more rapidly than they can be filled by the accumulation of sediment from the elevations of which they are the complements. The largest of them (the Mediterranean and Central American) may possibly have been *original* breaks in their Continental platforms.

Thus, then, all our knowledge of the existing relations between Continental plateaux and Ocean-basins, and of the forces by which those relations might probably be disturbed, points distinctly to the inference that these relations have never been very different from what they are now. And the entire conformity of the results of this reasoning from the present to the past, with those of Professor Dana's reasoning in the contrary direction from the primal assumption (which no man of science would now call in question) of the Earth's original fluidity, affords strong confirmation of its validity.

I am far from affirming that considerable *local* changes may not have occurred in past epochs, which may have had very important

effects upon the distribution of Plants and Animals ; so that, on the one hand, Land-continuity has been established where there was formerly a complete interruption ; whilst on the other, continents now for the most part separated by Oceanic areas, or islands cut off from neighbouring continents by deep channels, may have been at one time in continuous connection. My contention is that such connections have been formed by the elevation of mountain ridges (terrestrial or submarine) by lateral thrust; and not by the vertical elevation of a great area of sea-bottom into a continental plateau. Thus, there appears to be valid evidence that the surface-connection between North and South America is comparatively modern ; a communication between the Atlantic and Pacific basins having formerly existed where now interrupted by the Isthmus of Darien, the elevation of which probably does not date back further than the early Tertiary period. So, in the North Atlantic, the extension of the European platform to the west of the Shetland Islands, the existence of a ridge at only about 200 fathoms' depth beneath the surface between the Faroes and Iceland, and of another ridge at a greater depth between Iceland and Greenland, renders it not unlikely that at some former period Europe and North America may have had a band of connection along this line. On the other hand, the knowledge we now possess of the configuration of the more southerly part of that Oceanic area, seems to preclude the probability of the former extension of a great continental platform (the hypothetical Atlantis) between Europe and America in the parallel of the Azores. So, as it seems to me, the remarkable relations pointed out by Sir J. D. Hooker between the Floras of New Zealand, Tasmania, and South America, may be accounted for by connecting ridges raised by lateral thrust, without supposing the existence of a vast Antarctic continent now deeply submerged.* And the former connection of Madagascar with the African continent, distinctly indicated by the distribution of animal and vegetable life on the western portion of the island, might easily have been established by an elevation of the bottom of the Mozambique Channel by lateral thrust. There are even indications, in the groups of volcanic islets lying to the north-east of Madagascar, that this great island may have been once in connection through them with the Asiatic continent.

Such limited and local changes, I again repeat, are perfectly consistent with the doctrine of general permanence. And I have now, in conclusion, to show how remarkably this doctrine is confirmed by comparison of the deposits ascertained by the 'Challenger' soundings to be now going on upon the real Ocean-floors, with those in process of formation on the shallow bottoms near land.

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* We still know too little about the configuration of the Sea-bed of the great Southern Ocean, to enable any definite opinion to be at present formed on this point. All that can be said is, that no physical evidence of the former existence of such a connecting continent has as yet been obtained.

IV. The examination which Mr. Murray has made of the samples of the Oceanic deposits brought up by the 'Challenger' soundings and dredgings, affords conclusive evidence, that the floor of the real Oceanic area, unless in the near neighbourhood of the Continental platforms, is not, and never has been, covered with sediments formed by the degradation of the existing land; such sediments being deposited only on the shallow bottoms not far from shore, which (as already pointed out) may be considered as in reality submerged portions of those very platforms, and as not belonging to the true Oceanic area. With the exception of certain patches of clay, which there is strong evidence for regarding as a product of the decomposition of pumice ejected from volcanic vents, all the sediments now in process of deposition on the Oceanic sea-bed are of *organic* origin: a *calcareous* ooze, resembling chalk, being produced by the decomposition of the continually accumulating shells of Foraminifera; and a *siliceous* ooze being formed by the like accumulation of the skeletons of Radiolarians in the warmer zones, and the loricæ of Diatoms in the colder. Although volcanic sand was of course met with over the volcanic areas, *ordinary siliceous sand*, resembling that of our own shores and shallow bottoms, *has nowhere been detected on the deep-sea bottom.* And thus, if this bottom were to be raised into dry land, it would be found entirely destitute of those inorganic sedimentary deposits, which constitute by far the larger part of the succession of stratified formations with which geological inquiry has made us familiar. I can best make obvious to you the full significance of this fact,—which, as Professor Geikie has recently remarked, is of the profoundest interest for geologists and geographers,—by citing the views of that eminent geologist as to the mode of formation of the long succession of stratified rocks, which originated in the deposit of sediments formed by the degradation of pre-existing land. "Among the thickest masses of sedimentary rock—those of the ancient Palæozoic systems—no features recur more continually than the alternations of different sediments, and the recurrence of surfaces covered with well-preserved ripple-marks, trails and burrows of annelids, and polygonal and irregular desiccation-marks like the cracks at the bottom of a sun-dried muddy pool. These phenomena unequivocally point to shallow and even littoral waters. They occur from bottom to top of formations which reach a thickness of several thousand feet. They can be interpreted only in one way, viz. that the formations in question began to be laid down in shallow waters; that during their formation the area of deposit gradually subsided for thousands of feet; yet that the rate of accumulation of sediment kept pace on the whole with this depression; and hence, that the original shallow-water characters of the deposits remained, even after the original sea-bottom had been buried under a vast mass of sedimentary matters." The same he holds to be true of the relatively thin and much more varied formations of later date. So it is evident that the materials of these sedimentary rocks must have been deposited in near proximity to the land by

the degradation of which they were produced. "From the earliest geological times the great area of deposit has been, as it still is, the marginal belt of sea-floor skirting the land." This double process of degradation of old land, and deposit of materials for the new, "belongs to the terrestrial and shallow oceanic parts of the earth's surface, and not to the deep and wide oceanic basins." The 'Challenger' explorations have now furnished absolute proof, that the deposits now in progress on the floors of the ocean-basins have no real analogy among the past sedimentary formations which geological inquiry brings into view. "We now know by actual inspection, that the ordinary sediment washed off the land sinks to the sea-bottom before it reaches the deeper abysses; and that, as a rule, only the finer particles are carried more than a few score of miles from the shore." On the abyssal depths the sedimentary deposit gathers so slowly, that the particles of meteoric iron—the star-dust which falls from outer space—form an appreciable part of it.

"From all this evidence," continues Professor Geikie, "we may legitimately conclude that the present land of the globe, though consisting in great measure of marine formations, has never lain under the deep sea; but that *its site must always have been near land.*" "The present Continental ridges have probably always existed in some form; and as a corollary we may infer that the present deep Ocean-basins likewise date from the remotest geological antiquity."*

It is now nearly eleven years ago, that I first ventured in this place to break ground in regard to a subject, for the discussion of which my previous pursuits might have been thought to give me no special qualification. I then made known the conclusion which had been arrived at by my colleague Professor Wyville Thomson and myself, that no essential change had taken place in the great basin of the North Atlantic since the elevation of the Chalk of Europe and America into dry land; and that the globigerina-ooze now accumulating on its bottom is not a *new* chalk-formation, but a continuation of the *old*, which has there gone on uninterruptedly through the whole of that Tertiary period, during which a long succession of varied formations has been in progress of deposit round the margins of the continental lands. But I somewhat incautiously adopted the expression of my friend, "that we might be said to be still living in the Cretaceous epoch." This brought down a storm of geological indignation on our heads. We were accused by one of our very highest authorities, of attempting to disturb the well-established doctrines of geological succession; and were represented by another as showing a complete ignorance of what a geological "epoch" really meant. When, however, we explained that all we contended for was the persistence of a deep Ocean-basin in the Atlantic area, and the

* Lecture on 'Geographical Evolution,' delivered before the Royal Geographical Society, March 24, 1879.

continued formation of globigerina-ooze on its bottom, from the Cretaceous epoch, through the whole Tertiary period, down to the present time, our accusers began to think our doctrine worthy of consideration; and not many years elapsed, before it came to be generally accepted as (to say the least) not improbable. The progress of Deep-sea research, and my own further reflection on the vast disproportion between the mass of the Land above the sea-level and the volume of the Water beneath it, made me think it probable that this view would bear extension to all the great Ocean-basins.* When I found it advocated, on quite other grounds, by a geologist so distinguished for his combination of vast practical knowledge with profound theoretical ability, as Professor Dana, I naturally felt increased confidence in it. And now that Professor Geikie has formally pronounced it to be in his judgment the only one that is consistent, on the one hand, with the facts revealed by geological inquiry as to the conditions under which the past sedimentary deposits were formed, and on the other with the facts determined by the 'Challenger' observations as everywhere presenting themselves over the real Oceanic sea-bed, I venture to present it to you with some degree of assurance, as a doctrine which is likely to take rank as one of the fundamental verities of Geological Science.

[W. B. C.]

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* See the article *Atlantic* in vol. iii. of the ninth edition of the 'Encyclopædia Britannica.'

WEEKLY EVENING MEETING,

Friday, January 30, 1880.

William Spottiswoode, Esq. M.A. D.C.L. LL.D. Pres. R.S. Vice-President, in the Chair.

John Marshall, Esq. F.R.S. &c.

Proportions of the Human Figure.

(Abstract deferred.)

GENERAL MONTHLY MEETING,

Monday, February 2, 1880.

THE DUKE OF NORTHUMBERLAND, D.C.L. Lord Privy Seal, President, in the Chair.

His Royal Highness PRINCE LEOPOLD, K.G.
was elected an Honorary Member of the Royal Institution.

John Carteighe, Esq. F.C.S.
Frederic Coxhead Mathieson, Esq.
were elected Members of the Royal Institution.

The Managers reported that in December last they awarded the Actonian Prize of One Hundred Guineas to Mr. R. S. Boulger, for his Essay on the Structure and Functions of the Retina in all Classes of Animals, viewed in relation to the Theory of Evolution.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Lords of the Admiralty—Nautical Almanack for 1883. 8vo. 1879.
Governor General of India:—
Geological Survey of India:
Records, Vol. XII. Part 4. 8vo. 1879.
Asiatic Society of Bengal—Proceedings, 1879, No. 8. 8vo.
Journal, Vol. XLVIII. Part I. No. 3. Part II. No. 3. 8vo. 1879.
Brown, James F. Esq. F.C.S.—Apparatus, Past and Present: Engravings. (Sheet II.) 1879.
Agricultural Society of England, Royal—Journal, Second Series, Vol. XV. Part 2. 8vo. 1879.
Author—Ambition's Dream in two Fyttes. New Edition. 16to. 1879.
Accademia dei Lincei, Reale, Roma—Atti, Serie Terza: Transunti, Vol. IV. Fasc. 1. 4to. 1879.
Actuaries, Institute of—Journal, No. 117, and List of Members. 8vo. 1879.
Astronomical Society, Royal—Monthly Notices, Vol. XL. No. 1. 8vo. 1879–80.
Bavarian Academy of Sciences, Royal—Sitzungsberichte, 1879, Heft 3. 8vo.
Bentley, C. S. Esq. (the Author)—A new Theory of the Tides. fol. (P 12) 1879.
British Architects, Royal Institute of—1879–80; Proceedings, No. 1. 4to.
Transactions, Nos. 1, 2, 3. 4to.
Address of the President. 4to. 1879.
Buckler, George Esq. (the Author)—Colchester Castle a Roman Building. 8vo. 1876–9.
Cambridge Observatory, Syndicate—Astronomical Observations, 1861–5. 4to. 1879.
Chemical Society—Journal for Dec. 1879, Jan. 1880. 8vo.
Cogswell, Charles M.D. F.L.S. M.R.I.—Major Francis Duncan: History of the Royal Regiment of Artillery. 3rd Edition. 2 vols. 8vo. 1879.
Dax: Société de Borda—Bulletins 2e Série, Quatrième Année: Trimestre 4. 8vo. Dax, 1878.

Editors—American Journal of Science for Dec. 1879, Jan. 1880. 8vo.
Analyst for Dec. 1879, Jan. 1880. 8vo.
Athenæum for Dec. 1879, Jan. 1880. 4to.
Chemical News for Dec. 1879, Jan. 1880. 4to.
Engineer for Dec. 1879, Jan. 1880. fol.
Horological Journal for Dec. 1879, Jan. 1880. 8vo.
Iron for Dec. 1879, Jan. 1880. 4to.
Journal for Applied Science for Dec. 1879, Jan. 1880. fol.
Nature for Dec. 1879, Jan. 1810. 4to.
Telegraphic Journal for Dec. 1879, Jan. 1880. 8vo.
Franklin Institute—Journal, Nos. 648, 649. 8vo. 1879.
Geographical Society, Royal—Proceedings, New Series. Vol. I. No. 12. Vol. II. No. 1. 8vo. 1879–80.
Geological Institute, Imperial, Vienna—Verhandlungen, 1879, Nos. 10–13, 8vo.
Jahrbuch: Band XXIX. No 3. 8vo. 1879.
Abhandlungen: Band VII. Heft 3. fol. 1879.
Harlem, Société Hollandaise des Sciences—Archives Néerlandaises. Tome XIV. Liv. 4, 4, 5. 8vo. 1879.
Mechanical Engineers, Institution of—Proceedings, Oct. 1879. 8vo.
Melbourne University—Calendar, 1879–80. 12mo.
Meteorological Office—Report of the Proceedings of the International Meteorological Congress at Rome, 1879. 8vo. 1879.
Rev. J. S. Perry's Report on the Meteorology of Kerguelen Island. 4to. 1879.
Negretti and Zambra, Messrs.—B. A. Gould: Uranometria Argentina; Brightness and Position of fixed Stars within 100 degrees of the South Pole. 4to. With Atlas. Buenos Ayres, 1879.
Newton, A. V. Esq. (the Author)—Patent Law and Practice. New Edition. 16to. 1879.
North of England Institute of Mining and Mechanical Engineers—Transactions, Vol. XXVIII. (1878–79.) 8vo. 1879.
Ord, Wm. Miller M.D. M.R.I. (the Author)—On the Influence of Colloids upon Crystalline Form and Cohesion. 8vo. 1879.
Pharmaceutical Society—Journal, Dec. 1879, Jan. 1880. 8vo.
Photographic Society—Journal, New Series, Vol. IV. Nos. 2, 3. 8vo. 1879.
Preussische Akademie der Wissenschaften—Monatsberichte: Aug., Sept., Oct., 1879. 8vo.
Rankin, George C. Esq.—Encyclopédie, ou Dictionnaire Raisonné des Sciences, des Arts et des Métiers, par une Société de Gens de Lettres, mis en ordre et publié par M. Diderot; et quant à la Partie Mathématique, par M. D'Alembert. Troisième Edition. A Genève et à Neufchatel. 39 vols. 4to. 1778–9.
Royal Society of London—Proceedings, Nos. 197, 198, 199. 8vo. 1879.
St. Bartholomew's Hospital—Hospital Reports, Vol. XV. 8vo. 1879.
Symons, G. J.—Monthly Meteorological Magazine, Dec. 1879, Jan. 1880. 8vo.
Telegraph Engineers, Society of—Journal, Part 28. 8vo. 1879.
Tuson, Professor R. V. (the Editor)—Cooley's Cyclopædia of Practical Receipts. Parts 15, 16 (last). 8vo. 1879.
Saxon Society of Sciences, Royal—Abhandlungen: Band XX. Nos. 2, 3. 4to. 1879.
University College, London—Catalogue of General Library. Vol. III. 8vo. 1879.
Van der Mensbrugge, G.—Sur la Surface des Liquides en Mouvement. (K 103) 8vo. 1879.
Verein zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, 1879: Heft 1. 1880: Heft 1.
Victoria Institute—Journal, No. 51. 8vo. 1879.
Watherston, E. J. Esq. (the Author)—Our Railways: Should they be Private or National Property? (K 103) 8vo. 1879.

WEEKLY EVENING MEETING,

Friday, February 6, 1880.

Warren De La Rue, Esq., M.A. D.C.L. F.R.S. Secretary and Vice-President, in the Chair.

William Huggins, D.C.L. LL.D. F.R.S. *M.R.I.*

The Photographic Spectra of the Stars.

In the year 1863 my friend Dr. William Allen Miller exhibited on the screen in this room a photograph of the spectrum of the star Sirius, which we had taken the evening before in my observatory. The images of stars in the telescope had already been photographed as points, but this was the first time that their rays after dispersion by a prism had recorded themselves upon a photographic plate. For certain instrumental reasons, the photographs which we then took did not possess sufficient purity of the spectrum to give them a scientific value.

Several researches in other directions to which I subsequently devoted myself, prevented me for some years from resuming this inquiry, until a few years ago, when I took up the subject again. I purpose this evening to give an account of this recent work, and of the results which have come out of it.

Our common notion of light is limited not by the actual extent of range of the radiations of a luminous body, but by the power of our eyes to see them. Of the long range of radiations which comes from highly heated matter, the sun for example, only a small portion falls within the power of the eye. Beyond the extreme violet, where visibility ends, a great range of shorter vibrations beats upon the eye, and we know it not. So on the other side below the red all consciousness of light fails us; but here another sense, that of the feeling of heat and warmth, enables us still to know that a radiated influence from the hot body is coming upon us. These two invisibles, the ultra-violet and the ultra-red, though they cannot stimulate our eyes directly, can make themselves known to us mediately, through certain actions on other bodies.

One of these is the disturbing influence they exert on delicately balanced salts of silver, which we call their photographic power. This action was regarded as so exclusively the property of the ultra-violet portion of the spectrum, that these rays have been distinguished by the names, "chemical rays," "photographic rays." Quite recently,

however, Captain Abney, by the discovery of a new molecular condition of silver bromide, has brought the whole of the other end of the spectrum, the ultra-red, within the power of the photographic plate. He has, I believe, taken the photograph of a kettle of boiling water in the dark by means of its own radiation.

This evening we shall have to do exclusively with the ultra-violet portion of the spectrum.

In the years 1865 and 1869 I had the honour to bring before this Institution the results of the observations of Dr. Miller and myself on the visible spectra of some of the stars. These eye observations embraced a range of vibrations extending from a little below C in the red to about G in the blue. The recent researches, to which I now at once proceed, begin where the eye observations ended, about G, and carry our knowledge of the stellar spectra beyond O, and in some cases beyond S, in the ultra-violet.

We shall, perhaps, underrate the importance of a knowledge of the ultra-violet spectra of stars, if we regard these photographs as simply adding so much in length to the visible spectrum, for there are reasons why a knowledge of this part of the spectrum may be of exceptional value to us.

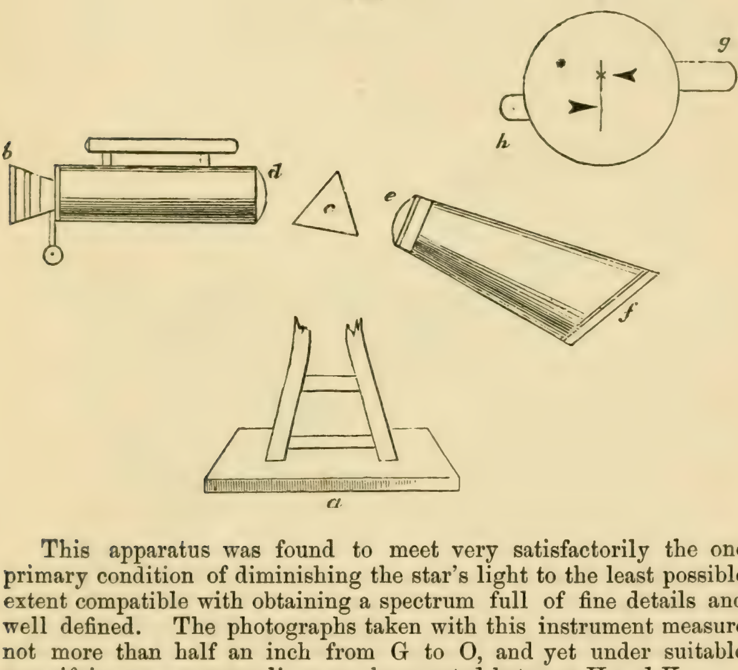
I shall describe first, in some little detail, the instrumental methods by which the very great difficulties which present themselves in so delicate an inquiry were successfully overcome. The two principal difficulties with which the inquirer is at once brought face to face, are the feebleness of the star's light after dispersion by a prism, and the circumstance that the stars are in apparent motion, arising from the earth's rotation.

It was therefore necessary to do two things, first, to obtain a sufficiently pure and detailed spectrum with the least possible loss of light, and secondly, to devise some method by which the star's image could be kept absolutely invariable in position within a very narrow slit.

After passing the limit of the visible spectrum, the transparency of glass diminishes rapidly, until at length it becomes opaque to the rays of very high refrangibility; for this reason it was necessary to avoid altogether the use of this substance. A telescope of the reflecting form, in which the light is received upon a metallic speculum, was employed. This instrument has a speculum of 18 inches diameter. The spectrum apparatus must also contain no glass. There were two substances available, Iceland spar and quartz, both of which are very transparent to this part of the spectrum. Quartz is harder and takes a higher polish and was used for the lenses, but its dispersive power is so small that more than one prism would have been needed, introducing loss of light and other drawbacks, if this substance had been employed. Iceland spar possesses a much higher dispersive power; it is, indeed, about equal to moderately dense flint glass. One prism of this substance of 60°, which was beautifully cut for me by Mr. Hilger, was found to be sufficient for the purpose.

The apparatus is represented in this diagram (Fig. 1). It is mounted on a base plate *a* with bevelled edges, which enables it to be accurately adjusted at the end of the telescope. The prism is at *c*. The image of the star is brought upon the slit *b*. The light is rendered parallel by lens *d*; it passes through the prism, and is then, by a second lens of quartz, made to converge and form an image on the photographic plate *f*, which is inclined so as to bring a considerable part of the spectrum to focus upon the plate.

Fig. 1.



This apparatus was found to meet very satisfactorily the one primary condition of diminishing the star's light to the least possible extent compatible with obtaining a spectrum full of fine details and well defined. The photographs taken with this instrument measure not more than half an inch from G to O, and yet under suitable magnifying power seven lines can be counted between H and K.

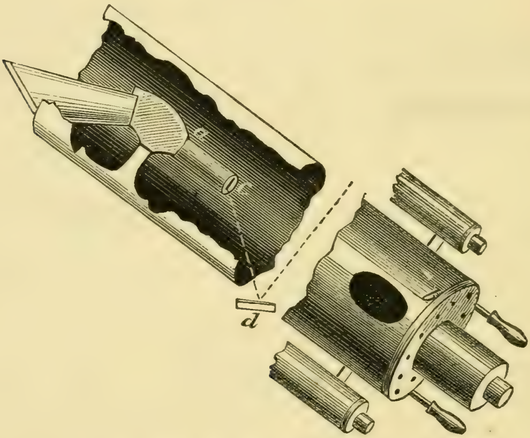
The second important difficulty was to find a ready means of bringing the luminous point, into which the star's light is gathered up by the mirror, accurately upon any part of the very narrow chink, the $\frac{1}{350}$ part of an inch, through which the light has to enter the spectrum apparatus, and further to maintain the star's image precisely within the same part of this chink during the whole time of exposure of the photographic plate, which might be as long as one hour or even more.

The telescope was, of course, mounted upon an equatorial stand—that is, one in which the axis of motion is placed parallel to the earth's axis of rotation,—so that the telescope when kept in suitable motion

by clockwork will remain invariably pointed to any star, notwithstanding that the rapidly rotating earth is carrying the telescope and the observer round with it. This clock motion is one of exceptional excellence, due to the inventive skill of Mr. Grubb, being furnished with a secondary control by means of a pendulum in electrical connection with a standard clock. But even these instrumental arrangements, although of exceptional excellence, were not delicate enough. It was found necessary to supplement them with a method of continuous supervision and control by hand.

In this diagram (Fig. 2) you have represented a portion of the reflecting telescope, which is of the Cassegrain form. The small mirror was removed, and the spectrum apparatus accurately adjusted

Fig. 2.



by its sliding base plate, so that the slit was brought precisely to the principal focus of the large speculum. Now over this slit is placed a highly polished silver plate *c*, with a narrow opening rather larger than the slit.

The next point was to fix on the side of the telescope a small mirror *d*, by which artificial yellow light could be thrown upon the plate. One point further. The great speculum has a central hole; now behind this, in place of the usual eyepiece, is fixed a small Galilean telescope or opera-glass.

Now if the observer directs the telescope to a star, and then looks into this small telescope, he sees before him the silver plate and the slit within the opening by means of the artificial illumination, and also at the same time the star's image as a bright point somewhere on the plate. It is then easily within the observer's power to bring the star's image exactly upon any desired part of the slit. In the figure at *i*, Fig. 1, you have represented what the observer sees. The star's

image being rather larger than the width of the slit, its place, even when upon the slit, can be seen. If, therefore, the observer keeps his eye fixed upon the star's image during the whole time of exposure, half an hour, one hour, or it may be two hours, he can instantly correct by hand any small irregularities of the motion of the telescope, and so maintain the star's image invariably fixed upon the slit.

Further, it was necessary to obtain the photographs under such conditions that it should be possible afterwards to determine with accuracy the value in wave lengths of the positions in the spectrum of the stellar lines.

For this purpose the slit was provided with two small shutters, as represented at *h* and *g*, Fig. 1. One of these only remains open while the photograph of the star is taken.

When the exposure is finished this shutter is closed. The other can then be opened, and a second spectrum upon the same plate for the purpose of comparison taken. It may be the solar spectrum reflected from the moon, or the spectrum of a known star, or a terrestrial spectrum, or the apparatus may remain until the following day, and then the solar spectrum be taken upon the plate directly.

Afterwards, from these comparison spectra, by the aid of a suitable measuring apparatus attached to a microscope, the wave lengths of the stellar lines were determined. And for this purpose use was made of the excellent map of M. Cornu of the ultra-violet, and of his determinations, and those of Mascart, of the wave lengths of the lines of cadmium, aluminium, and zinc. Various photographic methods were tried, but the great sensitiveness which may be given to gelatine plates, as well as the great advantage of employing plates in a dry state, led to the exclusive use of this method of photography.

I was about to complain of how few nights sufficiently fine for this work present themselves during a whole year—they may be counted upon the fingers—but I forbear when I remember that, notwithstanding the terrible drawbacks of our climate, no country contributes more largely than our own to the advance of astronomy.

Before proceeding to the results of my work, I will endeavour to make visible to you some portion of the ultra-violet part of the spectrum.

Besides their photographic power, there is another mode of action by which the ultra-violet rays may make themselves visible to us. There are some substances which absorb these very rapid vibrations, and then give back the energy they have received, in the form of vibrations which are sufficiently long to come within the power of the eye. They transform the invisible energy into visible light. This property of fluorescence is possessed in a high degree by sulphate of quinine, and by æsculin, a substance which exists in the bark of the horse-chestnut. I have a small screen which has been brushed over with a solution of this substance.

Professor Dewar has kindly placed at my disposal one of his electric-arc crucibles. I cannot forbear congratulating Professor

Dewar on having inaugurated so fruitful a method of spectroscopic investigation. Instead of the usual optical arrangement of glass, I have substituted a lens of quartz, and a prism of Iceland spar, similar to that which I have used in my star work.

I will now ask Mr. Cottrell to throw first upon the usual screen the visible spectrum. Even now, when no glass is used, you see how brilliant are the blue and violet parts of the spectrum. The part of the spectrum we shall have to do with in the stars lies for the most part beyond. Now, if this prepared screen be held beyond, you see that the invisible energy is translated for us into characters which the eye can read. In the crucible we have the vapours of calcium and aluminium, and we now see, not merely the ultra-violet light, but the bright lines of these substances in this part of the spectrum.

I now proceed to the results which have come out of this work.

In 1865 I exhibited on the screen several coloured drawings of spectra taken from the observations of Dr. Miller and myself in illustration of the different kinds of spectra which the stars present. It is desirable that I pass three or four of them in review before I exhibit the photographic spectra corresponding to them.

The first diagram represents the spectrum of Sirius. The spectrum of this star may be taken as typical of the stars which shine with white light. Most of the photographs belong to this class of star. Very early Dr. Miller and myself called attention to the distinctive characteristics of the spectra of stars of this class. The great distinguishing feature of their spectra consists of three or four very broad and intense lines. By a method of direct comparison we found three of these lines to coincide with lines of hydrogen. The first corresponds to C of the solar spectrum, the second with solar F, and the third with a line of hydrogen near G. This last line near G appears as the first strong line of the photographic spectrum. There are, indeed, numerous very fine lines also present, but these are so delicate as to be seen, fitfully only, except under the most favourable conditions. We satisfied ourselves of the double line of sodium at D, the least refrangible of the magnesium group, and a line at E—a line of iron—and some others. This class includes the largest number of bright stars. The spectra of the different stars of this class are chiefly distinguished from each other by the greater or less breadth and diffuseness of these lines of hydrogen, and also by various degrees of strength and visibility of the finer lines.

I will now show you the spectrum of another class of stars of which the light is tinged with yellow. This spectrum is that of the star Aldebaran. This class includes our sun. In this star the lines of hydrogen are reduced to about the proportion they possess in the solar spectrum. The other lines of the spectrum are no longer fine and difficult to see. Here we have in full the triple line of magnesium. I now show the spectra of two stars of a different class indeed, but in both cases the light is orange. I will not stop to describe these spectra, but pass to one more class, the stars in the light of

which the predominant colour is less refrangible. These stars are of a full red colour.

Now we return to the class of white stars. [The original photographs were exhibited on the screen.] As this photograph is a negative, of course the black lines are represented by transparent spaces and the continuous spectrum by a dark band. We shall be able, therefore, better to study the peculiarities of the spectrum if we substitute for it a positive taken by direct superposition. Here (Fig. 3) the dark and light are not reversed. The circumstance, which is so marked as to compel us to give it first attention, is the distinctly symmetrical character of this strong group of lines. When the negative is examined under suitable conditions of illumination, twelve lines may be counted. As the refrangibility increases, the lines diminish in breadth and the distance between any two lines is less as the refrangibility of the lines increases. It is also of importance to notice that the spectrum does not end with them. Beyond the last of the group of lines the continuous spectrum runs on far beyond S in the ultra-violet. The point where the group ends is between M and N.

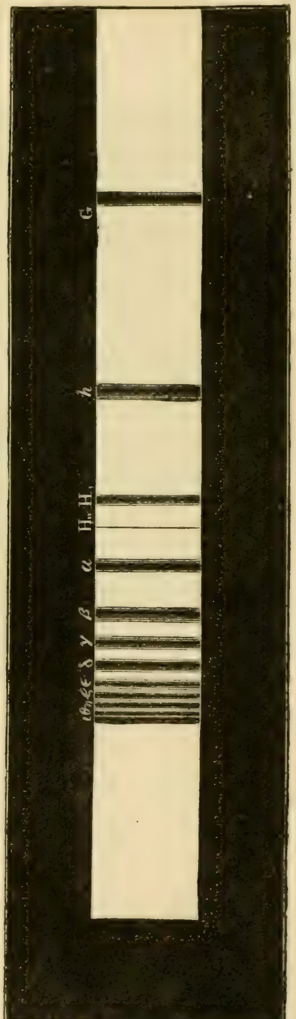
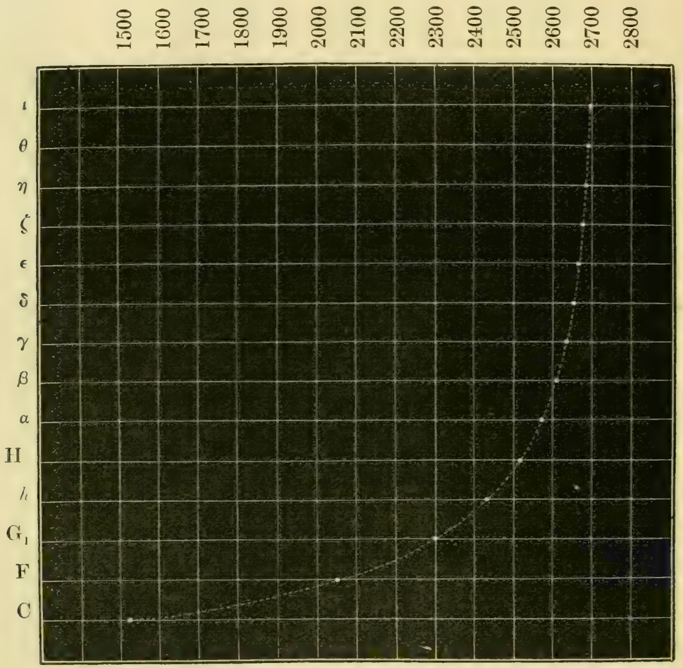


Fig. 3.

The first in order of refrangibility of these lines is the well-known line of hydrogen near G, which you saw in the visible spectrum of the star. The second of these lines is also a line of hydrogen, coincident with h of the solar spectrum. The next line coincides in position with the strong line H of the solar spectrum. But where is H_2 or K? It is represented by this very thin line, which is barely recognziable. You remember how narrow a slit was used, and that if this were a photograph of the solar spectrum, some seven lines or more would be clearly visible in this space. We shall now be able to

study the spectrum better by a reference to this diagram (Fig. 4), in which the lines are put down according to their wave lengths. The two lines H and K, as is well known, coincide with two lines of calcium, and we attribute them usually to the vapour of that substance.

FIG. 5.



The remarkable behaviour of these lines in the stars was pointed out by me at the end of 1876. A few months previously, Mr. Lockyer suggested that photographs of the brighter stars might show a modification of this character, and that if such were the case, it would support his view as to the dissociation of the vapour of calcium in the hottest stars. In a subsequent paper to the Royal Society Mr. Lockyer explained in more detail his views of the dissociation of the terrestrial elements, and also the bearing of his views in connection with the different kinds of visible star spectra. I wish also here to acknowledge the kindness of Professor Dewar, who permitted me to witness the experiments conducted by himself and Professor Liveing. I saw the lines of calcium corresponding to H and K in the emission spectrum of that substance vary in relative brightness until for a moment the line corresponding to H alone remained.

Are this thick line of the star spectrum and the thin line at K really due to calcium?

Now, beyond these lines there is another pair of strong lines in the calcium spectrum more refrangible. The question arises, are these represented in the star's spectrum by any strong lines? They are not. Two strong lines are near them, but do not coincide with them.

Again, in the photographic spectrum of hydrogen there is a line at the position of H. It is seen in Mr. Capron's photographic spectra. It is also present in my own photographs. Mr. Lockyer called attention to the coincidence of this line with H in December, 1879.

In this diagram the photographic spectrum is completed by the addition of the strong lines in the visible one, and laid down according to the scale of wave lengths. Of these the first four are certainly lines of hydrogen; the fifth, H, coincides both with calcium and hydrogen. I suspect in some of my photographs of hydrogen fine lines at the positions of several of the more refrangible lines of the stellar group.*

That all the lines of this remarkable group are members of a common physical system becomes very highly probable indeed, if we convert the wave lengths into their reciprocals, the wave frequencies, and then plot them down, as is done in this diagram (Fig. 5). It then becomes evident that they lie on, or very near, a definite curve, a state of things which we could not suppose to happen by chance. Mr. Johnstone Stoney, in a letter to me, remarks on this point:—

"The question whether they lie actually or only near a definite curve is, if I mistake not, of great significance in the theory. If they lie on a curve, obeying any exact mathematical law, their connection must, I think, be attributed to their corresponding to the consecutive overtures of some vibrating system. If, on the other hand, they lie near but not on the curve, this circumstance would support the hypothesis (which seems to accord with other facts) that the visible lines are members of a long series of harmonics, most of the members of which are invisible, those which are seen being those whose positions chance nearly to fulfil a definite state of things which I have

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* Since this discourse was given, Dr. H. W. Vogel has called my attention to a paper on the "Spectrum of Hydrogen" in the 'Monatsbericht der Königl. Acad. der Wissenschaften zu Berlin,' July 10, 1879. In this paper Vogel pointed out coincidence of a line of hydrogen with H; and among the lines given by him are three others which agree with stellar lines. Vogel's wave lengths for these lines are—

3968 H	3834
3887	3795

The wave lengths of the twelve typical lines are—

H near G	4340	δ	3767·5
h	4101	ϵ	3745·5
H	3968	ς	3730
α	3887·5	η	3717·5
β	3834	θ	3707·5
γ	3795	ι	3699

February 28, 1879.

shown to exist in some acoustic arrangements, and which where it exists exalts the intensity of the harmonics whose positions nearly fulfil the requisite condition. I converted the wave lengths into wave frequencies. . . . I think it must be accepted that the lines do not lie on, but near a definite curve. This appears to be corroborated by finding that H_1 and G_1 (hydrogen line near G) are connected harmonially, these rays being exactly the 35th and 32nd harmonies of a vibration whose fundamental is $\frac{\tau}{72 \cdot 003}$ (τ being the time in which light travels a millimetre in air) . . . ."

Under these circumstances one is led to regard the whole series of lines as due to hydrogen. In this connection it may be stated that Messrs. Dewar and Liveing find that the line of calcium K is more easily reversed than the line at the position of H.

This spectrum of Vega may be taken conveniently as typical of the whole class of white stars, so that in our consideration of the other stars of this class we shall consider the distinctive features peculiar to each, as modifications, or departures, from this common typical form. To facilitate these comparisons I have distinguished the typical lines by the letters of the Greek alphabet, beginning with the line more refrangible than H.

In this map (Fig. 4) I have arranged the spectra of five other stars of the white group in their order of change, approximately at least, from the spectrum of Vega.

I will point out some of the directions in which these changes show themselves, and I will then exhibit upon the screen the photographs themselves of these stars.

There are principally three directions in which the changes take place:—

1. In the breadth and greater or less marginal diffuseness of the typical lines.

2. In the presence or absence of K, and, if present, in its breadth and intensity relatively to H.

3. In the number and distinctness of other lines in the spectrum.

Now in these stars we see modifications in these three directions, a successive diminution of breadth of the typical lines, and of the nebulosity at the edges; the lines become at the same time narrower and defined at the edges.

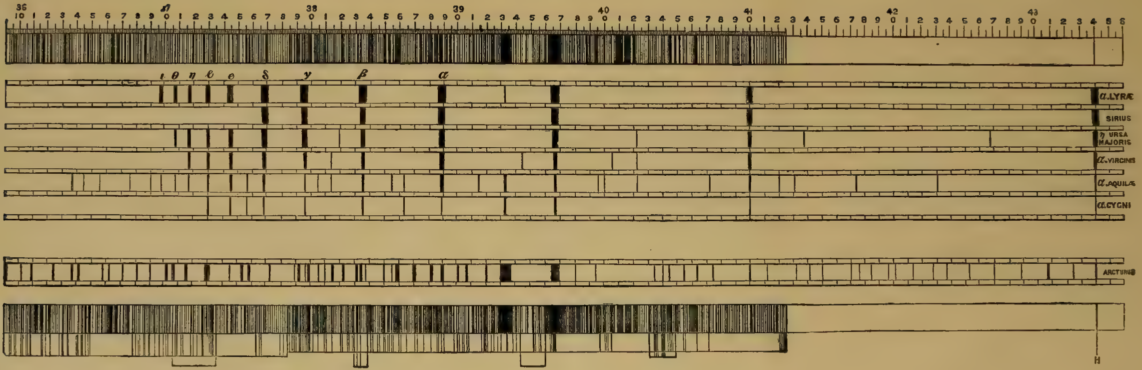
In Sirius the lines are about the same thickness as in α Lyræ, and the line corresponding to K of about the same fineness.

In the next star, α Ursæ Majoris, we have the same typical group, but the lines are less broad and rather more defined at the edges. There is no fine line at the position of K, but some other lines make their appearance.

The star next in order is α Virginis. Here the typical lines are still narrower and more defined. K is stronger relatively to H, and numerous lines are visible beyond the last of the typical group.

In the spectrum of α Cygni the typical lines are still narrower

FIG. 4.



36
38
39
40
41
42
43
ι θ η ε e δ γ β α
α.LYRÆ
SIRIUS
η URSA MAJORIS
α.VIRGINIS
α.AQUILÆ
α.CYGNI
ARCTURUS
H

and more defined. The line at K is nearly as broad as H, and there are other lines present.

In the last spectrum of the map, that of Arcturus, we come to that of a star of another order, which includes the solar type, but this star appears to be further removed than the sun is, in the order of change from the typical form, as we meet with it in Vega and Sirius. Here the typical lines are no longer present as a strong group. The line at K is stronger relatively to H than it is in the solar spectrum. The spectrum is crowded with fine lines, and in the visible part resembles the solar spectrum, but beyond H the lines are more intense and differently grouped.

We cannot resist the feeling that we have here to do with a star which has departed farther from the condition in which Vega now is than our sun has yet done.

The question presents itself—Have we before us stars of permanently different orders, or have we to do with some of the life-changes through which all stars pass?

Does the sun's position somewhere before Arcturus in the order of change indicate also his relative age?

On these points we know nothing certainly. If I may give some play to the scientific use of the imagination, I would ask you to imagine an inhabitant from some remote part of the universe seeing for the first time an old man with white hair and wrinkled brow, to ask, Was he born thus? The answer would be, No; in this child, this youth, this man of mature age, you see some of the life-changes through which the old man has passed. So, giving play to the scientific imagination, there may have been a time when a photograph of the solar spectrum would have presented the typical lines only which are still in Vega. At a subsequent period these would have been narrower and more defined, and other lines would have made their appearance. And if we allow this scientific imagination to project these Friday evenings into the far future, the lecturer, clad it may be in the skin of a white bear, may have to describe how the spectrum of the then feeble sun has already passed into the class of spectra which now distinguishes the stars which shine with red light.

There remain only two other points. In 1865 I described the method of observing the spectrum of a planet compared directly with the solar spectrum under *similar conditions* of terrestrial atmosphere. The planet is observed in the early evening, when the light from the sky is bright enough to give a spectrum. With a long slit one sees a broad spectrum of the sky, and then upon it the brighter spectrum of the planet. Making use of this method, spectra were taken of the planets Venus, Mars, and Jupiter.

I will now exhibit upon the screen the spectrum of Venus. This broad spectrum is that of the light from the sky. The narrow stronger spectrum is that of the planet Venus. You see line corresponds to line, and that there are no modifications or additions which indicate a planetary atmosphere.

The same is true of the planets Mars and Jupiter. These two last-named planets do show indications of atmospheric absorption in the visible part of the spectrum. Similar photographs taken of different small areas of the moon under different conditions of illumination are negative as to any lunar atmosphere. It must not be supposed that such observations are necessarily antagonistic to the existence of a lunar atmosphere. They simply tell us nothing as to its existence.

There are many other directions in which the photographic arrangements I have described may be doubtless successfully employed. I hope to photograph any lines that may exist in the ultra-violet part of the spectra of the gaseous nebulæ. The apparatus will give us the spectra of different portions of a sun-spot. It may enable us to determine the difference of velocity in the line of sight of two stars; and also we may record by it the sun's rotation by the alteration in refrangibility of the lines of the spectra of opposite limbs.

One of the great charms of the study of Nature lies in the circumstance that no new advance, however small, is ever final. There are no blind alleys in scientific investigation. Every new fact is the opening of a new path. As the description of a first step in a new and broad highway, I venture to hope the last hour's discourse has not been wholly wanting in interest.

[W. H.]

WEEKLY EVENING MEETING.

Friday, February 13, 1880.

Warren De La Rue, Esq. D.C.L. F.R.S. Secretary and Vice-President, in the Chair.

W. H. Preece, Esq. C.E. M.R.I.

The Telegraphic Achievements of Wheatstone.

Dr. Johnson said of Oliver Goldsmith—

> "Nihil erat quod non tetigit;
> Nihil quod tetigit non ornavit."

Some are inclined to think that the great literary giant of one hundred years ago thought more of the roundness of his periods than of the facts they clothed, but a greater man than Samuel Johnson said of a greater man than Oliver Goldsmith—in fact, our well-beloved Faraday said of Charles Wheatstone that there was nothing he touched that he did not adorn.

Wheatstone's familiar form was very well known to the old *habitués* of this theatre. Whenever either of his favourite subjects, light, sound, or electricity, was under discussion, his little, active, nervous and intelligent form was present, eagerly listening to the lecturer. He was no lecturer himself, yet no one was more voluble in conversation. At explaining any object of his own invention, or any apparatus before him, no one was more apt, but when he appeared before an audience and became the focus of a thousand eyes, all his volubility fled, and left him without a particle of that peculiar quality which enables an individual with confidence to come before a critical audience, such as is represented by the members of this Institution, to develop scientific facts or describe apparatus. This defect proved fortunate, for it was the cause of Wheatstone obtaining the aid of the greatest lecturer of the age, and the annals of this Institution bear record of many a Friday evening being occupied by Faraday expounding the "beautiful developments," as he called them, of Wheatstone.

Up to the year 1834 Wheatstone devoted his time almost entirely to the investigation of sound. In that year he was appointed

Professor of Natural Philosophy at King's College, and then commenced that career of electrical investigation, that wave of success on whose crest he moved until it broke on the shore "of that country from whose bourn no traveller returns." The many electrical inventions by Wheatstone which were brought forth after 1834 were derived more or less from his telegraphic achievements. He was not an electrical investigator; no one could be while Faraday existed. He was essentially a practical man, who applied the discoveries of others to the wants and purposes of mankind. Somebody has said that nothing is easier than the discovery of yesterday. Nothing is more difficult than the discovery of to-morrow! The telegraph itself was no new idea. The scroll of time bears the names of Lomond, Le Sage, Sœmmering, Ronalds, Ampère, Schilling, and others, but it was only in the year 1837 that all these fingerposts led up to that point where two men met who made telegraphs practical.

In 1837 Cooke, Morse, Steinheil, and Wheatstone all focussed the labours of previous inventors, and gave the starting point from which telegraphy became what it is. Cooke and Wheatstone went hand in hand. Wheatstone was the brilliant, fertile, ingenious man of science. Cooke was the sanguine, energetic, practical man of business. When Cooke came to England from Heidelberg he was (through Roget and Faraday) brought into contact with Wheatstone, and he found that Wheatstone had cracked the Columbus egg—he had discovered the possibility of bridging over space. All previous attempts to apply electricity to useful purposes had failed, from the difficulty of obtaining sufficient force at a distance to be productive of effect. By applying the laws of Ohm to the facts of Ampère and Oersted, Wheatstone succeeded in finding the proper basis for arranging wires and magnets in such proportions as to produce evident effects. The electrical effects utilized for telegraph purposes are very numerous. The one upon which Cooke and Wheatstone worked was the simple fact that whenever a current of electricity passes in the neighbourhood of a magnet, such as the mariner's compass, that magnet was deflected; and Wheatstone arranged five mariners' compass needles in a horizontal row, each needle deflecting when a current of electricity was sent along the wire to which it was attached, deflecting to the right or left according to the direction of the current. Such deflections or beats to the right or left represented symbols, combinations of which could be translated into letters and words.

Here is a five-needle hatchment-shaped instrument, made on the principle I have just explained. This was the original kind of speaking telegraph instrument. It soon became apparent that five needles were not required to form symbols to represent all the letters, and a four-needle instrument was introduced; and practice and experience, the great utility tests, proved that when one or other of these four needles became faulty or unworkable, communication could still be readily kept up on the remaining needles. Thus it was soon found

that a double-needle instrument was capable of meeting all requirements, and here before you are instruments of the original design on the double-needle principle. This one of an elegant architectural design was made for use in the New House of Parliament about 1850. Before you are the first two double-needle instruments that were ever used, and they are greatly prized for their historical value. This kind of instrument met the same fate as its predecessors, and was superseded by a single-needle instrument which gave out its signals quite as efficiently as its earlier brethren possessing a greater number of needles. This form has remained in use to the present day, and may be seen at any railway station in the country. It is used to a greater extent than any other kind of telegraph instrument, there being at the present moment at least 10,000 employed by railway companies, and 3500 by the Post Office. It is an instrument of simple construction, but I will not detain you by explaining details. You can easily see that when I press down a pedal the needle is deflected; if it be the right-hand pedal then the needle goes to the right, and if the left-hand pedal it goes to the left, and one deflection to the left and one to the right represents the letter A, one to the right and three to the left B, four beats, viz. one right, one left, one right and one left C, and so on through the alphabet.

Wheatstone saw the necessity of doing away with the trouble of acquiring familiarity with this kind of instrument, and set to work to introduce one which could be understood by anyone in a moment's acquaintance. This, of course, meant representing the ordinary letters of the alphabet without requiring translation from signals. He produced an instrument which printed the ordinary letters, but the mechanical complexity of its arrangement compelled him to abandon it. He then thought of a permanent alphabet on a dial which revolved in front of an open window or around which the indicating needle could revolve and point out the exact letters sent. This proved more practicable, and the result was the alphabetical instrument, such as I now show you. The indicating portion works in a very similar manner to the second hand of an ordinary clock. In a clock the hand makes a slight pause at each point representing a second on its dial, and proceeds by jerks round and round the dial, being stopped at each second by the cogs of a wheel. The hand or indicator of an alphabetical instrument works round the dial containing the alphabet just in the same way, but is controlled by a piece of mechanism which only answers to the current being sent. Each current moves the mechanism to which is attached the indicating needle, and each movement jerks forward the needle one step as it were. To cause the needle to make one revolution round its dial we will suppose requires fifty-two steps (really it is many more). Then if it is made to wait or rest at every second step, and opposite that step a letter is placed, it is easy to see that in one revolution all the letters may be successively indicated. The indicator is caused to rest or wait by the depression of small keys placed round the dial of

the sending portion of the instrument opposite to the printed letters. The whole apparatus is based on one of the most remarkable discoveries of the century, that by which Faraday found it possible to obtain electricity from a magnet. I have here the identical apparatus that Faraday himself used in developing this beautiful idea. It was not an accident (none of Faraday's discoveries were accidental), but it was the result of continued self-education, scientific training, experience, and observation, and forms one of the most interesting episodes in the scientific history of this country. Faraday, by taking this magnet and placing over it a soft iron armature, around which wire is wound, was able to produce electricity on moving the magnet backwards and forwards. He thus caused currents to flow in the wire round the armature, which produced sparks as you see I now do. The following lines were written upon this experiment:—

"Around the magnet Faraday
Was sure that Volta's lightnings play,
But how to draw them from the wire?
He took a lesson from the heart.
'Tis when we meet, 'tis when we part,
Breaks forth the electric fire."

Wheatstone applied this fact first of all to produce artificial alarums. Here is an instrument (which old electricians call a "thunderpump") containing an arrangement similar to that used by Faraday, the magnet being caused to move by the pressure of a handle. The ends of a wire round the armature are joined to an alarum, and on my pressing down the "pump" handle you hear the bell ring, which is caused by the electricity produced, and sent along the wire by the moving magnet. The bell might be fifty miles away, but the effect would be the same. The same idea was adopted for an A B C instrument, and a wheel (like a ship's steering wheel) was made to send a current every revolution. A case is marked off with the letters of the alphabet, and the wheel is made to stop opposite the letter sent. This apparatus was used in 1840. It was too cumbrous for practical adoption, and so fell out of use. From then to 1858 little or nothing was done, but in that year Wheatstone introduced quite a novel feature in the manufacture of his instruments in the way of making them of the most perfect mechanism, and as accurate in their fittings as chronometers; and to enable this to be done he fortunately succeeded in acquiring the assistance of perhaps one of the finest mechanical geniuses and workmen that England has had for many a long day; and that is Mr. Stroh, whose name I need not say is well known to this Institution. Here is a specimen of the improved alphabetical transmitter. When the handle is turned, currents of electricity are produced which when the keys opposite the letters are pressed down, go to the other end of the wire, to which is attached the receiver or indicator, the needle of which moves according to the currents sent. (The method by which the indicating needle revolved was explained by the use of a large model with toothed wheel and

springs; and when the actual mechanism of the sending portion of the alphabetical instrument was reflected on a screen the principle of the instrument was clear.) I have shown you the primitive instrument of 1840, and its improved though cumbrous form of 1858; and here is its elegant representative of 1880, which is joined up to a wire between this room and the Central Telegraph Station, and by which we will have a little conversation. There is scarcely a portion of this instrument that is not an improvement on the earlier forms, and some of the improvements have passed through many stages before reaching their present perfection. The original principle adopted by Wheatstone remains, but the teachings of practice and observation showed practical defects which have been removed, and so brought the instrument to what you now see. (Several items of general news were received on the alphabetical instrument from the Central Station.) While this instrument is slow, it is sure, and, comparing its pioneer of 1840 to a cart-horse, may be said to be a fine racer. It is very useful for private purposes or at outlying offices where little business is done; and many thousands of them are so employed.

Having succeeded so far in obtaining simplicity, Wheatstone turned his attention to the practicability of sending telegraphic signals by machinery without the aid of the hand in manipulation, and thus increasing the capacity of wires for carrying messages. Bain in England, Siemens in Germany, and others had been working in a similar field, and in 1858 the genius of Wheatstone, combined with the mechanical ingenuity of Mr. Stroh, developed an entirely new system of automatic telegraphy on the principle of the Jacquard loom. A paper ribbon was passed through a piece of mechanism consisting of three keys with cutting punches, which, when pressed on the paper, perforated it according to the key depressed. The centre key cut a continuous row of holes, which were used to push forward the paper. The left-hand key cut two holes directly opposite each other, and represented the left-hand beat of a needle, or the dot of the Morse alphabet. The right-hand key cut two holes, one above and one below the middle row, but in a slanting direction from left to right, and represented the right-hand beat of a needle, or the dash of the Morse alphabet. The paper so perforated was then passed through the automatic transmitter, the action of which I can, perhaps, make clear by the use of a model.

Wheatstone's automatic instrument transmits a succession of currents of electricity in opposite directions, and if no paper were interposed to prevent these currents going except at the proper time, this succession of currents would be continually transmitted. (A model of the transmitter was explained in detail.) So that when no holes present themselves to the rocking prongs for the currents to pass through, nothing goes to line; but if two holes, representing a dot, present themselves, then a current passes, and a dot is produced at the receiver; and so, if the holes representing a dash admit the

prongs, a dash is transmitted to the receiver. Each reversal of the current forms a dot, and if the reversal is stopped, a dash is formed; and it is by controlling these reversals with the perforated paper that reversals are sent at the proper time to make the required signals. I have an actual apparatus fitted up here in connection with the Central Station, and we will set it going and see what news is passing on the wire on which the instrument has been placed. At the present moment I may tell you that this wire is transmitting intelligence to distant offices, and our instrument is placed on it so that by setting it going we can see for ourselves what is passing. (Odd items of news were read off by the assistant.)

When Wheatstone invented this apparatus it was only workable at an average rate of seventy or eighty words a minute, but scientific training, observation, thought, and care have resulted in improving upon this so much that, on making inquiry yesterday morning, I found that one of our Wheatstone instruments was actually working at the rate of 180 or 190 words a minute. When the Queen's Speech was transmitted to the country last week it was sent to several towns in five minutes, and as it contained 800 words, the average rate was 160. To long distance places it is not possible, from various causes, to maintain this high rate without the insertion of a translator or repeater, which receives what I may call the almost exhausted signals, and sends them on reinvigorated to their destination. Until recently the rate of working between London and Cork was sixty words a minute, but, by inserting a repeater at Haverfordwest, this speed was doubled. The repeater is a complicated instrument, and I will not attempt to describe it. Its function, reduced to simple words, is, that it receives the currents from London, and transmits fresh ones to Cork. Repeaters are being generally introduced on our long circuits. To apparatus not on Wheatstone's principle repeaters can also be applied, and this has been done even in such complicated machinery as the duplex apparatus.

Wheatstone's great achievements were the needle, alphabetic, and automatic instruments. The first telegraph was erected on the Camden incline, and is now called the fossil telegraph, though it only dates as far back as 1837. It consisted of five wires inserted in wood, and five needle instruments were fitted at each end. On recent operations going on where this old line was laid, portions of it were dug up, and I have a piece now before you.

Here is a photograph of an original document referring to the early needle telegraph. It is now reflected on the screen, and you see that in the year 1842, " under the special patronage of Her Majesty and H.R.H. Prince Albert, the public are respectfully informed that this interesting and extraordinary apparatus, by which upwards of fifty signals can be transmitted 280,000 miles in one minute, may be seen in operation daily (Sundays excepted), from 9 a.m. till 8 p.m. at the telegraph office, Paddington, and telegraph cottage, Slough.

Here is another placard which was distributed all over London at the same time as the previous one, and which speaks of the telegraph in the same eulogistic terms:—"The galvanic and electric telegraph, Great Western Railway, may be seen in constant operation daily [one would think we were going to the Polytechnic] (Sundays excepted);" and goes on to say that "by this powerful agency murderers have been apprehended, thieves detected, and lastly (which is of no little importance), the timely assistance of medical men has been procured in cases which would otherwise have proved fatal." There are some of the brilliant ideas thought of Wheatstone's telegraph in 1842. Everyone knows of the enormous development of the telegraphs. In 1870 the commercial part of the business was transferred to the Government, and at that time the business done in four weeks represented 554,000 messages. In the four weeks just expired it was 1,900,000. In the metropolis alone, while the number of messages of all sorts dealt with in four weeks in 1870 amounted to 130,000, in the four weeks just passed there were 726,000. It is very curious to note, in quoting these figures, that the high figure of the past few weeks is to a large extent owing to the tremendous fogs we have had, which were the cause of a marvellous increase in telegraph business. Pecuniarily, therefore, from this point of view, fogs are not objectionable. The traffic for two days at the Central Station in February, 1870, was 14,000; during the past week the average has been 40,000.

But it is in the transmission of news where Wheatstone's telegraphic achievements have proved of such marvellous benefit. In 1871 there were distributed to the different papers copies of messages, some 2000 words long, others as short as 10 words, a total of 32,000. In 1879 they amounted to nearly 50,000. The number of words delivered in one week in 1871 was 3,598,000; in 1879 they amounted to nearly 6,000,000, which means 300 millions for the year, or 15,000,000 columns of 'The Times.' There is not a town in the United Kingdom possessing a daily newspaper that is not in direct communication with London for news purposes, and by this means every man receives at his breakfast table the latest item of news, Parliamentary or general, just as readily as we do in London. And all this is done by the Telegraph Department with the Wheatstone apparatus. In 1870 there were only six wires used for special press purposes, now there are twenty-four. Besides the million words sent a day, there are newspapers in Glasgow, Dublin, and Edinburgh that rent wires for themselves, fitted up with different kinds of apparatus, by which they transmit all the debates of the Houses, &c.

In 1870 the number of Wheatstone alphabetical instruments was 1200, now 5000 are in use. There are now 151 circuits worked by the Wheatstone automatic apparatus, in 1870 there were only eight. This system has proved its superiority for the rapid despatch of news, and, in time, will no doubt be adopted by all countries employing the telegraph. I have not the slightest hesitation in saying that our

telegraphic apparatus (thanks to Wheatstone) is at the head of that of the world, and my own impression is that the time is not far distant when even America will take advantage of the inventions we are now using.

On the table before you are specimens of the telegraphic achievements of Wheatstone which are worthy the attention of those who are interested in the subject.

One of the chief characteristics of Wheatstone was his extreme devotion to science. I doubt whether anyone ever gave himself up so completely to science, in every shape or form. He was not a philosopher, nor was he a deep investigator; but he was essentially an experimenter, and designer of delicate apparatus. The chief merits of his apparatus were their wonderful originality, their refined beauty, their marvellous fecundity, and their eminent adaptability for the purposes for which they were designed. I told you he was no lecturer, nor was he a prolific writer; but he was an unrivalled conversationist, and those who had the pleasure of his conversation could never forget the lucidity with which he explained his apparatus. His bibliographical knowledge was almost incredible. He seemed to know every book that was written and every fact recorded, and anyone in doubt had only to go to Wheatstone to get what he wanted. His power of deciphering puzzles was marvellous—it was instinct. A secret despatch of Charles I., that puzzled everybody since the days of that monarch, was placed into Wheatstone's hands, and was almost instantly explained. His mind, as I told you, was eminently practical. His powers over the forces of Nature are shown by his telegraphic achievements in the beauty of the apparatus before you. The elegance of the design of everything Wheatstone accomplished must always maintain him in the very first rank of the wonderful geniuses of this wonderful century.

[W. H. P.]

WEEKLY EVENING MEETING,

Friday, February 20, 1880.

William Spottiswoode, Esq. M.A. D.C.L. Pres. R.S. Vice-President, in the Chair.

The Rev. H. R. Haweis, M.A.

Old Violins.

[Among other violins exhibited, were two by Stradiuarius—one belonging to the Emperor of Russia, the other lent by his Royal Highness the Duke of Edinburgh—a Gaspar di Salo bass, found in the late Tarisio's bedroom with his corpse. The South Kensington Museum, and Messrs. Hart, Adam, Amherst, Hill, Enthoven, Cox, &c., also lent valuable instruments.]

The lecturer began by saying that the collection of violins and basses now before the audience, weighing but a few ounces each, represented several thousands of pounds worth. The variety in shape and style of the viol tribe, ancient and modern, showed the inexhaustible fascination it possessed over the mind.

"I deal to-night," he said, "with the construction, the history, and the sound of the violin. To begin with the wood. At Brescia makers use pear, lemon, and ash; at Cremona, maple, sycamore, and, of course, pine. The wood came into the markets of Mantua, Brescia, Cremona, Venice, Milan, from the Swiss Southern Tyrol, unlimited in supply, often mighty timbers of great age—plentiful then, scarcer now. The makers had their pick; they tested it for intensity and quality. Cut strips of wood and strike them, you will see how they will vary in musical sound. When a good acoustic beam was found the maker kept it for his best work. In Joseph Guarnerius and Stradiuarius the same pine tree crops up at intervals of years. A good maker will patch and join and inlay to retain every particle of tried timber. Old wood is oddly vocal. As I sat in my room surrounded by these instruments I could not cough or move without ghostly voices answering me from the sixteenth, seventeenth, and eighteenth centuries; and even the old-seasoned backs and bellies of unstrung violins are full of echoes."

Taking a violin and tearing it open, the lecturer continued:— "The violin is made of fifty-eight or seventy pieces. It is a miracle of construction. It is as light as a feather and as strong as a horse. Wood about as thick as a half-crown, by exquisite adjustment, resists for centuries a pressure of several hundred-weight. The belly of soft deal, the back of hard sycamore, are united by six ribs of sycamore, supported by twelve blocks with linings. The sound-bar running obliquely under the left foot of the bridge is the nervous system of the violin, the sound-post supporting the bridge is the soul, through it pass all the heart-throbs or vibrations generated between the back and the belly; on its position depends mellowness, tightness, or intensity of sound. The prodigious strain of the strings is resisted first by the arch of the belly, then by the ribs, strengthened with the upright blocks, the pressure among which is evenly distributed by the linings which unite them, and lastly by the supporting sound-bar and sound-post and back."

After describing the other parts of the instrument, Mr. Haweis alluded to the Cremona varnish, which he described after Mr. Charles Reade as probably a heterogeneous varnish, first of oil with gum in solution, then of colour evaporated in spirit. "A red and a yellow gum appear to have been used and combined. Although it was said that the secret was now lost, Dod, as late as 1830, who employed the Fendts and Lott and always varnished with his own hand, had the receipt for something very like the Cremona varnish; and, lately, Mr. Perkins has not only analyzed the varnish of Joseph Guarnerius and found amber in it, but has himself produced varnish of an extraordinary quality.

"The supreme interest of the violin is not far to seek. It lies not only in its simplicity, beauty, strength, subtlety, and indestructibility, which fit it for the cabinet of the collector, but it is the king of instruments in the hands of the player. It combines accent with modification of sustained tone. The organ has sustained tone without accent, the piano accent without sustained tone, the violin accent and sustained tone modified at will. Within its limits it is scientifically perfect; it has all the sensibility and more than the compass, execution, and variety of the human voice. The violin is not an invention, it is a growth; it has come together, it is the survival of the fittest. On the screens you see its rough elements, which had to be collected from the rebek, the crowth, and the rotta or guitar tribe. About the eleventh century an instrument of the viol tribe emerged with frets, but 150 years were required to get rid of these marplots before even a step towards the true viol could be made. Before the end of the fourteenth century viols were made in great profusion of every size and shape—the knee viol, the bass viol, viol de Gamba, of which certain South Kensington specimens are before you. But the rise of the true violin tribe begins with the rise of modern music. About the time when Carissimi and Monteverde—1585–1672—discovered the true octave and the perfect cadence, part singing received

a new impulse; the human voice was discovered to fall naturally into soprano, contralto, tenor, and bass, and viol instruments being adapted to these four divisions, the violin, tenor, bass, and later contrabasso, before me, gradually separated themselves from the confused *nebulæ* of viols behind me, and shone out clearly as the true planetary system of the musical firmament."

After illustrating the qualities of the violin, tenor, double-bass, and violoncello, Mr. Haweis alluded in detail to the schools of Brescia and Cremona. "Although here is an antique Duiffoprugcar (1520), the great Italian creators of the violin date, not from Mantua or Bologna, but from Brescia. Gaspar di Salo, 1560–1610, brought down the tubby German viol and struck a more elegant outline and proportion. He was almost the inventor of violin sound; beneath his flattened bellies and rounded backs the muffled sob began to vanish and the tone is loud and full. Maggini, 1590–1640, carried on the flat form, lowering his ribs; his tone is somewhat crisper and sweeter than Gaspar. The Maggini model passed into the hands of Andreas Amati, 1520–80, who had had ample opportunity as a contemporary maker of old viols to study the Brescian models, and while adopting their gaping sound-holes and drooping corners, reverted to the raised model, and thus retarded the triumph of the Cremona sound. It may be that the new loud fiddles seemed harsh to the monks, and wanting in mellowness after the soft old viols; but the charm of power once intuitively grasped by the Brescians, along with the flatter model, only wanted the intelligence of Jerome Amati, who again brought down his violin bellies, leaving his brother Anthony in the old ways. Still the violins by the brethren, Jerome dominating, are highly prized. Unfortunately they brought in the scoop on either side of the bridge, weakening the belly, and weakening if (as it is said) sweetening the tone. The later Amati, however, narrowed the Brescian sound-holes, thus retaining and prolonging their vibrations. Nicholas Amati, 1596–1684, who never quite shook off the scoop, by inventing the 'grand' pattern, a long-shaped instrument with pointed corners, paved the way for his great pupil, Antonius Stradiuarius."

After a brief account of Stradiuarius, Mr. Haweis alluded to his four periods, which, he said, ran into each other. "For thirty years this extraordinary man was content to work under the acknowledged influence of N. Amati. In 1668 he sets up for himself, but copies Nicholas till 1686; from 1686–94 his form fluctuates, but inclines to the earlier Brescian model (not in the corners), grows flatter, corners bold and full of character. In 1687 he makes the long or rather narrow model, which he did not adhere to. In 1700–3 he enters on his golden period after countless experiments. The last trace of the Amati scoop has disappeared. Some of his finest violins of the 'grand' pattern were made 1720–25. They have all the grace and boldness of a Greek frieze drawn by a master's hand. The arch of the belly, not too flat nor too much raised, is the true natural curve

of beauty; on each side the undulating lines, as from the bosom of a wave, flow down and seem to eddy up into the four corners, where they are caught and refined away into these inimitable angles. The scroll is strong and elegant, the sound-holes exquisitely cut. The varnish is not hard and silicate, but mellow as amber or sunlit water. There is a violin of 1736, bearing date and name; it was made in the master's ninety-second year. He made down to the last, but latterly seldom signed his work. Alas! that has been since done for him by thousands who would be at pains to make even a respectable tub."

Mentioning Carlo Bergonzi as the chief pupil of Stradiuarius, Mr. Haweis alluded briefly to the other Italian schools of Venice, Naples, &c., and then passed to the French school, dwelling on Pique and Lupot, 1758–1824; the German school, showing a specimen of Jacob Steiner, 1684, but slightly touched with the Cremonese influence; the "English Amati" Banks, Foster, and Duke, and calling attention to the fact that while France clave to Cremona from the first, England adopted the popular German Steiner, for nearly 100 years before returning to the Italian model.

In the course of the lecture His Royal Highness the Duke of Edinburgh's fine Stradiuarius, 1728, made by the master for Count Platen, given by him to the present Duke of Cambridge's father, and by the present Duke to His Royal Highness of Edinburgh, was exhibited—and another which, His Royal Highness had informed Mr. Haweis, had belonged to the Emperor Alexander II., and was, in fact, the property of the Russian Royal Family.

After noticing the reasons of the Cremonese supremacy as consisting in the selection and arrangement of wood, obedience to certain curves and thicknesses, which would vary endlessly according to the acoustical properties of each piece, the wood being cut thicker when soft and thinner when hard, the varnish, the sunny climate, the workmanship, and the lapse of years, Mr. Haweis closed the lecture by some practical illustrations on the sound of the violin, playing a few passages on several violins to illustrate their different qualities.

WEEKLY EVENING MEETING,

Friday, February 27th, 1880.

Sir W. Frederick Pollock, Bart. M.A. Vice-President, in the Chair.

F. J. Bramwell, F.R.S. M. Inst. C.E. *M.R.I.*
Past President of the Institution of Mechanical Engineers.

Sequel to the 'Thunderer' Gun Explosion.

It will probably be in the recollection of most of my hearers of to-night that in January, 1879, actually on the 2nd of that month, one of the 38-ton guns in the fore turret of the 'Thunderer' burst explosively, resulting unhappily in the death of many of those engaged in the working of the guns; and that, upon intelligence of this disaster being received in England, a Committee was appointed to inquire into the cause of the explosion.

That Committee, of which I was Assessor, met at Malta (where the 'Thunderer' was then lying), and reported. After the report was published, I, with the full assent of the Admiralty, delivered in this Institution, on the 13th of June last, a lecture, the title of which was "The 'Thunderer' Gun Explosion," while that of the present lecture is the "Sequel to the 'Thunderer' Gun Explosion."

I will call your attention to the diagram model (1) I exhibited at the last lecture, showing the gun in its turret, and will just re-state that the bore was 12 inches, the length of the tube 16½ feet, or 198 inches; that the gun was made of an internal steel tube, surrounded by four wrought-iron coils; that the powder used in it was known as P or Pebble powder, of which samples are on the table; of this powder two differing quantities are employed in the cartridges, the one, the "full charge," weighs 85 lbs., with this is used a common shell, which when empty weighs with its gas-check 590 lbs.; the other quantity, 110 lbs., is the "battering charge," and this is used with a Palliser chilled shell, which when empty weighs with its gas-check 700 lbs.

Diagrams (2) and (3) show the cartridges, with their projectiles and gas-checks.

The two guns in the after turret of the 'Thunderer' were also of 12 inches bore, but were only 35 tons in weight, the difference of 3 tons being due to the fact that the after turret guns were 3 feet shorter than the guns of the fore turret. This extra length of the

fore turret guns rendered it practically impossible to load them by hand from within the turret, and thereupon it became necessary to resort to some other means. The plan adopted was that of depressing the muzzle of the gun (when run in) to an angle of about $11\frac{1}{2}°$, and this enabled the muzzle to be presented to a tubular opening, or loading tube, which when the gun was in this position formed a continuation of the bore. This opening was made through the wall of the turret, below the level of the firing port, and so low down as to be between the main deck and the battery deck.

When the gun was thus depressed, the cartridge was lifted up and inserted into the loading tube, the projectile was placed on a carriage, which was raised by hydraulic pressure to the line of the loading tube, and then a telescopic hydraulic rammer, which had been previously used as a sponger-out, was caused to move outwards against the projectile to drive it off the carriage, and to send it and the cartridge up the bore of the gun. The projectile was prevented from running down the gun, in case of the ship rolling, by means of a papier-mâché disc wad placed upon the rammer head, and sent up the gun with it. Hydraulic apparatus was also used for running the gun out, and for running it in, when, as at drill, there was no recoil, and, to supplement the running in, due to the recoil, when the gun was fired. This same hydraulic arrangement served also to absorb any excess of recoil.

Prior to the meeting of the Committee at Malta, and not only subsequent to the Report of the Committee, but up to within the last few weeks, many persons believed that the explosion of the gun was due to an air space being left between the cartridge and the shot, or to one between the shot and the wad; and it was suggested, so far as regards the air space between the cartridge and the shot, that it might have been caused by the wad failing to act efficiently, and by the shot slipping down the inclined bore of the gun towards the muzzle. Such a supposition those who had the advantage, as I had, of investigating into the matter, knew to be without foundation, our experiments having conclusively proved that the inclination at which the gun lay was just that of the angle of repose of the shot, and that practically the shot had no tendency, or but the slightest tendency, to move downwards. This was shown by the fact that the upward pressure of the testing apparatus we employed, even when unweighted and when exerting a pressure not exceeding 8 lbs., was more than sufficient in all cases to retain the shot in its place, while the friction of the wad not only amounted to this 8 lbs., but far exceeded it, indeed it was such as to need the exercise of the pressure given by the hydraulic rammer to readily send the wad in. With respect to the other suggestion, that an air space existed between the projectile and the wad, there was conclusive evidence to show that no such space could have existed at the time of the explosion, because it was demonstrated, beyond the possibility of doubt, that after the wad socket, by which alone the rammer could withdraw the wad, had become detached from the wad disc, there were repeated rammings which had sent the disc home against the shot.

This same evidence was equally fatal to another class of suggestions as to the cause of the explosion, namely that the wad had become canted in front of the projectile, which, on the gun being fired, had passed over the wad and converted it into a wedge of such character as to burst the gun.

Reverting to the air space suggestion, the Committee in their Report showed that even if air spaces had existed, the result would be to diminish the pressure of the powder generally, although in some instances, and under particular circumstances, a local pressure might be developed upon a narrow band of the circumference of the bore in the neighbourhood of the base of the projectile. A pressure, however, of this kind would not be such as to endanger the gun, having regard to the limited area over which it would act, and to the fact that this limited area, besides having its own inherent strength, would receive aid from the metal both in its rear and in its front, that in the rear being exposed to but a comparatively slight pressure, while that in the front would be exposed to scarcely any pressure at all.

Further, there had been the experiments on Sir Wm. Palliser's gun and the experiments by the Armstrong and Whitworth Committee made long ago, which had shown that air spaces were not sources of danger to a gun. Moreover, at the time of the inquiry at Malta we had before us the fact of an air space of 4 feet having existed in the 100-ton gun on the occasion of one of the trials at Spezzia; and by the time I had delivered my last lecture, we had had, as I showed by the diagrams, experiments made by Captain Noble on a 10-inch gun chambered to 12 inches. In these last experiments the air spaces varied from 2 feet to 6 feet. The general result was the lowering of the pressure, but with the special result, however, when Rifle Large-grain powder was used in lieu of the Pebble powder, of generating the local pressure which I have said may be produced under certain circumstances. All these experiments show that, whether the local pressure was generated or whether it was not, the gun was uninjured.

Other suggested causes of the explosion were, that the gun was unfitted for ordinary use, because the materials were bad, or that it had been injured by previous firing.

The Committee, as you may remember, reported, for good reasons, as it appears to me, against all the preceding suggestions, and for equally good reasons—the best of all reasons—the evidence afforded by the fragments of the gun itself—gave it as their unanimous conviction, that the explosion had resulted from the gun being fired while two charges were in it.

This judgment of the Committee, I am glad to say, commended itself at the time to many of those who were in possession of the whole of the facts and who duly considered and weighed them; but we now know that a large number of professional men, well acquainted with the practice of gunnery generally, but not taking the pains to inquire into the special circumstances attending upon the working of the gun in the fore turret of the 'Thunderer,' were opposed to

the conviction of the Committee. They urged that, for a gun to be inadvertently double-loaded, those who were working the gun must be assumed to be ignorant of whether they had fired it or not, and this they said was on the following grounds an absurdity: that it involved their not knowing whether the gun had recoiled or not, for the recoil would have been evidence that the gun had not missed fire, and the want of the recoil evidence that it had missed fire, and it was impossible there could have been any doubt upon this question of recoil. Further, some of them urged that it must have been known by the noise of the report whether the gun had gone off or not; and, lastly, they all urged that any body of men must have noticed that the rammer used did not send the second shot to the position that it ought to have occupied if the gun had been empty, but that it stopped several feet short of this.

All these were very pertinent observations upon guns fired singly and unprovided with hydraulic running in and out gear, and with hydraulic telescopic loading gear; but they were inapplicable to, and, in the proper, but not the conventional, sense of the word, were impertinent to, the matter under consideration.

Let me briefly recapitulate the facts attending upon the loading and firing of the 'Thunderer' gun.

Two 38-ton guns in one turret, electrically coupled up to a firing key placed in the conning tower, to which key also the two guns in the after turret were coupled up. On the depression of this key by the officer in the conning tower (without any act on the part of those in the turrets), it was intended that all four guns should be simultaneously discharged to give a broadside. These electric discharges are by no means certain in their action. At the very broadside under consideration, one of the guns in the after turret was most certainly not fired off, although its electrical fuse was exploded. I say most certainly, because after the steps had been taken that were rendered necessary by the explosion in the fore turret, the charge of this gun in the after turret was extracted. There is therefore no certainty that because the key in the conning tower was depressed, both guns in the fore turret must have been discharged.

With respect to the men in the turret knowing by the sound, or concussion, that the gun had been discharged, all those who have been present in a turret when a single gun of this size has been fired, and again, when two guns have been fired, will agree in saying, that the effect upon the ear of the firing of one gun is such, that the explosion of the charge of the second gun does not add to it, and that thus it is impossible to tell whether one or two guns have gone off.

Next as regards the inward run of the gun due to recoil. In a manually worked gun that part of the inward run which is not so made has to be laboriously performed by hand, and it would be soon known whether the gun had not come in some part of its run by recoil; if it had not, this would at once give evidence that the charge had not exploded. But with hydraulically worked guns, the valve

lever is quite properly, and to save time, put into the in-running position directly the gun is supposed to have been discharged, and in a very few seconds the whole work of running in is accomplished; and whether it has been done by hydraulic power alone, or by that power aided by recoil, no one who does not happen to be closely observing the first part of the inward run of the gun could possibly tell.

Then with respect to the rammer giving an indication, the large joint of a hydraulic telescopic rammer having a greater area than the smaller one, it follows, almost as a matter of necessity, that on the water pressure being turned on, the large joint starts first on the journey and carries the rammer head with the smaller joint up into the bore of the gun, the smaller joint not starting to run out from the large one until the large one has made the whole of its traverse, and has come to rest; and thus it is that, in the absence of some indicator, there is nothing to tell those who are working the rammer to what distance the rammer head carried on the small joint goes up the bore, before it is stopped by the projectile and cartridge in the gun.

Thus concussion, recoil, position of rammer—all means that persons acquainted with the working of ordinary guns would rely on as affording information of the non-explosion of the first charge—fail when one is considering the question of double-loading of guns such as those in the fore turret of the 'Thunderer,' when fired in pairs by electricity.

From the very outset it was felt by the Committee of investigation, that notwithstanding their strong and unanimous conviction as to the cause of the explosion, based upon the evidence they themselves had heard and the investigations they had made of the fragments of the gun, there was great reason to fear that the public, and it might be some artillerists, including naval officers, not having the opportunity, or not caring to incur the necessary labour, to closely examine into the facts and evidence, would be likely to reject the conclusion arrived at by the Committee, and would hold the belief that the gun had exploded because the gun was dangerously near the exploding point even when used in the ordinary manner; and if this were true with respect to artillerists and naval officers, still more likely was it to be the case with reference to the gunners and sailors, whose education and training in all probability, as a rule, would not be such as to enable them to weigh the evidence and to follow the arguments based upon that evidence (even if they had access to it) and thus to come to a proper conclusion upon a question such as this.

It was under these circumstances that the Committee which sat at Malta expressed their hope that the Government would devote the fellow gun of the one which burst, to trials of all suggested causes, and finally to the firing with the double charge, so as to prove, both negatively and affirmatively, that the Committee were correct in the conclusion to which they had come.

On the occasion of my previous lecture on this subject, I stated my earnest desire that the Government would see fit to accede to that recommendation of the Committee, although I trusted it was not needed for the satisfaction of the service at large; but the time that has elapsed since that lecture, has brought evidence, that there were many who needed, to re-assure them, the indisputable proof which the double-loading could alone afford.

I am glad to say, as we now all know, that this proof has been given, and that the gun, after having been experimented with in the modes which I shall have occasion to detail to you presently, was finally double-loaded and fired; and I for one desire to tender my most sincere thanks to those in authority for having pursued this course, and I hope that everyone in this room will feel that in thus acting the Government were doing no more than they were bound to do, bearing in mind the fact that officers and men must of necessity carry out the orders given to them, wholly irrespective of the risk attending upon their obedience. It seems to me impossible for any right-thinking man to dissent from the proposition, that under such a state of things it is the duty of those who are placed in a position of authority—authority such that the orders they give cannot be disputed—to satisfy those who are compelled to obey, and to convince them by practical proof that in that obedience they incur no risk arising from the imperfection of the weapons put into their hands.

I hope that this expression of satisfaction will be general among those whose opinion is worth having, because it is easy to see that the time is coming when there will be those who will say, "Why was this waste made of a valuable gun? The Committee had stated the cause of bursting. The members of it who were in England and its Assessor said there is no doubt as to the result of the double-loading; and if this were so, and it was a foregone conclusion that double-loading would burst the gun, why was the gun wasted?" Probably among these objectors will be found those who by that time will have forgotten, as by this time they are beginning to forget, they ever denied that double-loading could burst a gun, and asserted that the bursting must have been due to some other cause.

I once heard an eminent chemist, lecturing on the history of a great discovery, say there were generally three stages through which all new truths have to pass. First, It is absurd, and cannot be. Second, Why should it not be?—it is as likely as not. Third, Why do you take up our time by labouring a conclusion with which every sensible man must agree, and always has agreed. Or—shortly, "We told you so."

I have not the least doubt but that we shall find these or similar remarks will be made in reference to the 'Thunderer' gun explosion. We know they have been made as regards the result of the experiments on one of the suggested causes. Already the statement is, as regards air spaces, "that nobody expected anything to happen from them—the experiments were made to amuse the gallery." Wad-

wedging, with the exception of one or two still faithful adherents, is now discarded as a most unlikely cause: "Absurd to suppose that a wrought-iron or steel gun could be burst with a piece of brown paper!" And with respect to the double-loading, I doubt not before many weeks are over it will be found that there is nobody who did not know from the very first that if a gun were double-loaded and fired it must burst. It appears to me that if it were only to bring persons into this condition of mind, the gun has been well expended, because if the truth be generally accepted and believed in, it matters little whether persons remember that this desirable state of mind has been arrived at against their previous convictions, or whether they delude themselves into the condition of believing that they never had any other opinion upon the subject, than that double-loading would burst the gun, and that nothing else would. Whatever may be the origin of the state of their minds, it is for the good of the service that the universal settled conviction should be, that the gun was strong enough for ordinary use, and was only burst by an extraordinary use, which in all probability never will recur, and which can be made a matter practically impossible to recur.

Other of the former suggested causes of the explosion, such as the insufficiency of the gun to withstand the effects of exploding a single cartridge, fired in the ordinary manner, arising either from badness of materials, or of workmanship, or from the gun having been injured by previous use, have practically been abandoned, and need not occupy your time for one moment. Neither need we devote much consideration to a novel suggested cause, which does great honour to the ingenuity of its author. He says, "When pushing in a drawer of a chest of drawers, if you push it on one side it sticks." I am afraid the gentleman's furniture must have been defective. "Now apply that reasoning to the projectile in the gun; if you push that upon one side that would also stick, and therefore the gun would burst." Therefore he reflects, "If I can but find some sufficient cause why the projectile may have been pushed more on one side than on the other, my homely chest of drawers has enabled me to solve this great problem." He no doubt had read those exhortations "to keep your powder dry," and it occurred to him that if dry powder were a good explosive, damp powder must be a less good explosive; thereupon he suggests that if one side of the cartridge, say the upper side, happened to contain a stratification of damp powder, while the other side had dry powder, that the one side of the shot would be impelled with a pressure greater than that which was acting on the other, the shot would jam, and the gun would burst.

I will now call your attention to the tables, Diagrams (4) and (5), which show the results of firing the gun with air spaces between the cartridge and the base of the projectile. As you will see, as many as seven experiments were made with each of the quantities of powder, which on all occasions was P, or pebble. The first experiment was made without any air space: in the subsequent experiments the pro-

jectile was moved further and further from the cartridge until an air space of 10 feet was left.

You will observe that the pressure, as indicated by the crusher gauge, decreased as the space increased, so that with the 110-lb. charge and the Palliser shell, instead of a pressure of 21·8 tons per square inch when there was not any air space, there was a pressure of only 6·2 tons per square inch when that air space was increased to 10 feet, and with the 85-lb. charge and the common shell instead of a pressure of 20·2 when there was not any air space 1·0 ton only was exerted when the air space was increased to the 10 feet.

The velocity of the shot and the recoil of the gun, it will also be observed, diminished as the air spaces increased.

These experiments entirely corroborated those which had been made by Captain Noble prior to my last lecture.

Although in the foregoing experiments with pebble powder burning comparatively slowly, no excess of pressure was attained, because the general diminution of pressure, owing to the enlarged space the gases had for their expansion, more than compensated for any increase due to ram action, yet it is perfectly possible by the use of a small grain, or highly inflammable powder, to set up a local pressure on the base of the shell precisely on the principle on which a water ram acts, and precisely on the same principle as that in which in the year 1870 I showed to the Institution of Mechanical Engineers how steam might be caused to deliver a greater pressure than that which prevailed in the boiler from which the steam came.

The question arose in the following manner. The late M. Le Chatelier had devised a mode of arresting railway trains without the use of breaks, by means of that which has become known as the "Contre Vapeur" system. This consisted in (as we engineers say) putting the engine into "back gear," so that the pistons instead of being propelled by the steam were converted into pump buckets forcing the steam back into the boiler. You may say, as many did say, that this amounted to nothing more than that which every engine driver does as a last resource to avoid collision—reverse the engine; but the reversal of an engine while in rapid motion is with an ordinary locomotive never resorted to except in dire necessity, because there is great danger of scoring the cylinder and the piston-rod, and of burning the packing in the stuffing boxes. This arises from the rise of temperature in the cylinders, due to the conversion of the stored-up energy of the train into heat. Le Chatelier was aware of this difficulty, and provided against it by admitting a small jet of water into the cylinders; this water was converted into steam, which, in common with the steam that came from the boiler, was pumped back into it.

The plan has had considerable use in France, but has never made its way in this country.

In the year 1869, however, Mr. Beattie, the then locomotive engineer of the London and South-Western Railway, fitted one of their engines for the purpose of trial, and I had the opportunity of

experimenting with it in order to lay the results of the experiments before the Institution of Mechanical Engineers. In making these experiments, I found to my astonishment that the pressure of steam in the cylinder was greater than that in the boiler from which the steam had set out. Had the difference been only two or three pounds I should have attributed it to the excess needed to force the steam back into the boiler, but the observed difference was as much as 30 lbs., and moreover it was obvious that the true limit of pressure had not been reached, but that the further rise of the indicator had been stopped, the spring then in the indicator having been compressed to the full extent of its range. My first impression was that the indicator was out of order, although in its immediately previous use, on the engine when running in forward gear in the ordinary manner, it had accorded so completely with the pressure of steam in the boiler as to render such an assumption very improbable; but on testing the indicator it was found to be quite accurate. I then had to cast about for the cause of the phenomenon of the excess, which was revealed by the indicator diagrams, enlarged copies of which diagram (6) are on the wall. To explain this cause I must refer you to the skeleton diagram (7) of the locomotive. From this it will be seen that the steam was taken from one end of the boiler, and was then conducted by a pipe the whole length of the boiler to the cylinders. When working in reverse gear the steam is suddenly admitted from the boilers into the cylinder when the piston is about half-way along the cylinder; as a result the steam is set in very rapid motion in the long pipe, and then upon the cylinder being filled, its motion is resisted, and the stored-up work in the weight of steam travelling at the high velocity along this pipe is sufficient to cause the pressure to rise in the cylinder to such a point above the pressure in the boiler as will absorb the "work" in the steam in the pipe.

I think I shall be able to illustrate that which I mean by the little apparatus I have here. This is a gas-holder, now, however, filled with air, giving, as you will see by the gauge, a statical pressure of nine inches of water, which I must ask you to accept as the equivalent of the pressure of the steam in the boiler. From the gas-holder a horizontal pipe (the equivalent of the long pipe in the boiler) proceeds. This pipe terminates in a vessel which is the equivalent of the cylinder in the locomotive. The pipe is shut off from the gas-holder by a stop-cock, and is shut off from the pressure gauge, placed at its end, by a little valve opening outwards towards that gauge, the gauge itself deriving its pressure from another pipe having a small hole of connection. On the sudden opening of the stop-cock, I think you will find that the air on rushing along the pipe and filling the vessel at the end of it, will not be content with producing a pressure in the vessel equal to that in the holder, but will by virtue of the stored-up work in the air in motion produce a pressure in the vessel sufficiently higher than that in the holder to open the valve against the gas-holder pressure, and to raise the water in the

gauge to a higher point than that at which it is now standing, although that point, be it remembered, is that which indicates the pressure of the air in the holder.

This simple experiment and those made in the locomotive illustrate the ram action of an elastic fluid, and the manner in which a local pressure at the base of the shot may be set up when, with a suitable air space, a very quick burning powder is used. But it would be found if we were to apply pressure gauges, that this pressure is extremely local, and as I have said, would not cause injury, because, it is resisted by the strength of the ring of metal against which it presses, and that ring is aided to resist it by the metal on each side of this area of local effort; which last-mentioned metal is not itself subjected to the local pressure, and therefore has a surplus of strength to aid its neighbour.

I will now call your attention to Diagram (8) showing the experiment which was made to test the value of the other suggested cause of explosion, namely, an air space between the projectile and the canted wad, over which wad it was assumed the point of the projectile might pass, thus converting the wad into a wedge and bursting the gun. All that I have to say about this experiment is that the effect was absolutely nil, as was also an experiment made with a similar air space, but with the wad not canted.

With respect to the non-injurious effect of air spaces, many persons have asked me, "How do you distinguish between the bursting of a sporting gun from a little snow being in the barrel when the gun is fired, and the bursting or non-bursting of the 38-ton gun with an air space? Are not these two states of things similar, and if the sporting gun bursts in consequence of the air space between the charge and the snow, why should not the 38-ton gun burst with its air space?" The answer is, that the sporting gun is not burst by the air space but by the snow. I have here two barrels which have been purposely burst in the manner I will describe to you.

Diagram (9) shows these barrels in their burst condition.

One of them, as you will see, is split open by a longitudinal split of some length. This was effected by placing a plug of wax at the point *a a* shown on the drawing, and firing a bullet against that plug. The pressure required to put the wax instantly into motion at a velocity equal to that of the bullet was, of course, infinite, but, as infinite pressures cannot be obtained, a compromise was arrived at between the shot and the wax. The shot retained part of its energy, and moved forward at a reduced velocity; the wax moved forward with the same reduced velocity; but as even this velocity could not be obtained instantaneously and without heavy pressure, the first effect of the impact, on the wax, was to cause it to expand laterally, and thereby to burst the gun.

The second barrel, you will see, has a ring bulge, which illustrates very strikingly that which I have been saying as regards local pressure at the base of a projectile. This ring bulge was obtained by placing

a bullet in the barrel, at the point *b b*, and by then firing at the bullet a pellet of wax. The wax meeting the base of the bullet was subjected to the pressure requisite to put the bullet into motion at the velocity, whatever it was, at which the bullet was then moving, and that pressure was sufficiently great over the small area shown to bulge the gun in the manner in which you see it.

There should be nothing difficult to understand in this fact of the bursting power of a small stationary object of a character such as, under pressure, to behave in the manner of a fluid, if we consider how the heaviest shot, moving at the highest velocity, will, on striking a yielding material like water, if they are of the appropriate form, be deflected from their lines of flight, and caused to assume an entirely different course. Especially is this the case with pointed projectiles. I was told that the pointed shot from the vessel the 'Huascar' when fired against the 'Amethyst' were many of them aimed very well as regards horizontal direction, but, fortunately for the 'Amethyst,' they fell a few feet short, and struck the water some little distance before reaching the vessel, with the result that they were deflected, and passed completely over the 'Amethyst,' doing her no harm whatever.

Let me ask you to suppose that up to the present time no shot had ever been fired so as to come into contact with water (and that no one had ever played "Ducks and Drakes"), and the question were put to any of you what effect would such contact have upon the flight of a 600-lb. shot moving at a velocity of a quarter of a mile a second. Do you not think you would have attempted to parody Stephenson's celebrated answer when asked what would happen if one of his locomotives were to run against a cow, and have said it would be "a bad thing for the water," and would have done so because apparently obviously a body so mobile that the hand of a child may disturb it at will, must be powerless to interfere with the flight of such an object as the shot of a 38-ton gun, or indeed of any gun. But we know from experience that it can deflect that flight, as I have just instanced in the case of the 'Amethyst,' and if the shot be of the appropriate shape, deflect it to a most serious extent; the reason being, as is now clear to all of us, that the inertia of the bulk of water that must be set into motion with the requisite speed to allow of the passage of the shot is such as to produce a resistance so great that if it be applied in any other direction on the shot than that of its axis it will cause a departure from the line of flight, and thus when the element of time is taken into account a mobile material like water may be as efficacious in diverting the direction of a shot as would be a steel-faced armour plate itself.

Similarly, the small piece of snow, which could be readily removed from the bore of the gun by the little finger, produces a lateral pressure when struck and sought to be put into motion by the rapidly moving shot, sufficient to burst the barrel of a sporting gun.

If a cylindrical shot could be made of some material so hard, that,

on being fired against a similar stationary shot, placed near the muzzle of a barrel, it would not split, and yet would not be so soft as to expand on the collision between the two cylinders taking place, the result would be that the barrel would not be injured.

After the air space and the wedge wad experiments, nothing remained but the firing of the gun with double-loading. I ought to have said that more than one person urged the authorities to try the double-loading before the canted wad experiment was made, on the ground that as the canted wad would burst the gun, there would be no gun left to fire double-loaded. The authorities, however, having confidence in the Report of the Malta Committee, did not accede to this request, as they felt assured that after the double-loading there would be nothing but fragments with which to experiment.

To guard against accident very considerable precautions had to be taken. These were most thoroughly carried out by the Royal Engineers, and were in every way successful. The gun, provided with a hydraulic cylinder recoil gear, was contained within a cell constructed of upright timber sides, and a timber roof; against the sides sandbanks were formed, and the roof was loaded with many thousand bags of sand. A transverse opening was left just at the rear of the gun from side to side of the cell, and above this opening two ventilating shafts were placed. The cell projected about 20 feet beyond the muzzle of the gun. There was then an opening of 4 feet, and beyond that another cell filled up solid with sand, into which the projectiles, and any splinters of the gun that went forward, were to be received. Diagram (10) shows the arrangement.

On the morning of the 3rd of this month, all preparations being complete, the gun was loaded, first with 110 lbs. charge of P. powder, then with a Palliser shell and gas-check, the shell being empty, then a disc wad. This wad fitted so tightly into the gun as to require a mallet to insert it. It was rammed home with a rammer worked by eight or ten men, and when in place a mallet was used on the end of the rammer.

Then a full charge, 85-lb. cartridge, of similar powder was put into the gun. Then a common shell with its gas-check, but empty, was inserted, and then another disc wad, which was similarly rammed home. When the whole charge was in the gun I measured from the front of the muzzle to the front of the disc of the wad, and I found the front of that disc to be $84\frac{1}{2}$ inches from the muzzle, or exactly fair with the front of the 1 B coil. This leaves $113\frac{1}{2}$ inches of the bore as the space occupied by the two shells, the two cartridges, and the two wads: allowing for the circumstance of the points of the shells penetrating the holes in the wads, and for the fronts of the cartridges being within the rim of the gas-checks, and for the front of the hinder wad being indented into the rear of the front cartridge, it will be found that the cartridges must have been occupying 3 to 4 inches less than the length nominally allotted, thus clearly showing that there was no defect in the hand ramming as practised on this occasion, such

as would cause the loading to differ from that which was effected by the hydraulic apparatus.

The loading being complete, those present at the experiment retired about two hundred yards. The gun was then fired. The report was not very remarkable; but it must be borne in mind that the gun was so thoroughly enveloped in the cell that the sound was, of necessity, much deadened. There was a very large volume of smoke, obviously more than would have occurred from an ordinary charge. Some planks which were laid across the space between the two cells were blown into the air, and these were all the indications exterior to the chamber, that were given.

On entering the cell it was at once seen that the gun was utterly destroyed, the breech part with the trunnion alone being left in position. The rest, with the projectiles, had either penetrated the sand in front, or was lying in fragments about the cell, the sides of which were scored.

The gun, or rather the remains of it, had recoiled the full distance of about 4 feet, and the carriage was hard up against the wooden blocks which had been put there as a final stop, and these blocks had their ends indented into the transverse timbers, showing that the pressure had been very large. Moreover, it was clear from the condition of the rear end of the cell that the pressure on the hydraulic apparatus had been such as to burst the cylinder which, unlike the cylinder on board the 'Thunderer,' was not provided with safety valves, and to drive the fluid (the oil) out of the cylinder, the inside of the cell at this part being literally anointed with the contents of the cylinder. Subsequent examination has shown that the end of the hydraulic cylinder had given way and had been opened out around more than half of its circumference.

I now propose to show you on the screen photographs of certain of the principal portions of the recently exploded gun and of its shells.

I will, however, first ask your attention to a photograph, Diagram (11), of the companion gun (the one that burst on board the 'Thunderer') as it appears now when put together in the Arsenal at Woolwich.

The first of the photographs, Diagram (12), of the gun recently burst, represents a front view of the remainder of the hinder part of the gun. You will see that the whole in advance of the breech piece has disappeared; that the steel tube is broken off in a jagged manner at about this point; that the front end of the C coil has been torn away, and that this coil itself is split from end to end on that which is the right-hand side of the gun when viewed from the muzzle end.

The front part of the steel tube is expanded, as the other tube was, from 12 inches diameter to about $12\frac{1}{2}$ inches, this expansion, as before, being due almost entirely to stretching in the grooves, and the front of the breech piece has again been bell-mouthed by that expansion. The rear end of the steel tube is unchanged in dimension,

and is absolutely without flaw for 3 feet 6 inches from the rear end; at this point some of the cracks can be traced, but it must be remembered that these are cracks which do not originate here, but which terminate here, their point of origin having been far forward or under the 1 B coil.

The second of these photographs, Diagram (13), represents the hinder part of the gun (but to a smaller scale), and—laid out in order, so far as it has been possible as yet to determine the order—the fragments of the 1 B coil, those of the front of the C coil, those of the B tube, and those of the steel barrel; from this photograph you will see that the 1 B coil has been burst, not only in several places longitudinally, but also transversely at about the middle of its length, or just over the point to which in all probability the front charge had moved at the time of explosion. You will also observe that the front part of the steel tube remains as a complete cylinder; this and some of the pieces immediately in its rear are on the table, and you will see from them that they have been ploughed into and deeply indented by a cylindrical body of the bore of the gun. I have no doubt whatever but that this body was the broken Palliser shell.

I will now ask your attention to the third of these photographs, Diagram (14), which shows the ruins of the rear shell, the Palliser, which, with its gas-check, is on the table. The fourth photograph, Diagram (15), exhibits the front part of the common shell, with its gas-check, and part of the fragment. I much regret that some extremely remarkable pieces of the rear of this shell have not been included in the photograph; they are however on the table and afford very considerable information. I find I have omitted to state that crusher gauges were put in the steel tube at its base, were inserted into the base of the Palliser shell and into that of the common shell; these gauges, before being put in, had been set to show no pressure below 36 tons. The gauges at the base of the tube, and at that of the Palliser shell, record that this pressure of 36 tons was not exceeded, in all probability was not reached; but the gauge at the base of the common shell tells a very different story. I will ask you to refer to Diagram (16), which shows a crusher gauge in its working condition, and then to compare with it Diagram (17), which shows the change that had been made in the gauge in the base of the common shell. The piece of copper, the "crusher," was yielding under the pressure, and had already collapsed as far as 40 tons, when the bottom of the shell was blown inwards, and in being so blown in, was jammed between the walls of the steel tube and the outside of the crusher gauge; the pressure was so enormous that the cast iron of the common shell has received on it a print of the rifling of the gun, and the cylinder containing the pressure gauge has been contracted upon the steel piston so as to nip it and to stop its further descent, and has thus unhappily prevented our obtaining the true record of the pressure which did prevail. It has been suggested that this driving in of the base of the common shell was due to a blow from the Palliser shell behind it, but most

certainly this could not have taken place until the bursting of the gun had suffered the gases of the forward charge of powder to escape; and further, the remains of the common shell, with its crusher gauge and its gas-check, make it clear that no such contact took place. If the Palliser shell had struck the common shell fairly, it would have broken the rear end of the common shell crusher gauge to pieces. There is not a mark upon it, while if it had struck eccentrically, as under the circumstances it well might, then the blow must have rent the gas-check of the common shell, and that again is without a mark; it is clear, therefore, that the base of the common shell was driven in by the excessive pressure of the explosion of the first charge, a pressure due to causes to which I will shortly allude.

It is now of course beyond dispute that double-loading of the 'Thunderer' 38-ton gun will burst it, and it is equally beyond dispute that the air space and canted wad trials did not burst this gun. This being so, it seems to me to be idle to now suggest, that although double-loading has burst the gun, and spaces and canted wads have failed to do so, and although there was conclusive evidence afforded by the condition of the socket of the wad used on board the 'Thunderer' that the wad could neither have been withdrawn to make an air space, nor could it have been canted, nevertheless the explosion on board the 'Thunderer' did take place from a canted wad and did not take place from double-loading. I will not pay you the bad compliment of supposing that you want this point further enlarged upon by me, but with your permission I will briefly allude to a criticism which has been made. It has been said, "Well, the gun has burst from being doubly loaded. This is a proof that the gun is not what it should be, because if double-loading could happen in practice, how much more likely is it to happen in the heat of action? and if a gun will not stand such a contingency as this, it is not a proper gun to be employed."

This is a taking sort of statement, but is to my mind one that could not be made by any person who had considered the subject. No one suggests that the hinder part of the 38-ton gun, the part which has to sustain the effect of the single charge, is unnecessarily strong. In fact there are some who would like to say it is too weak; but let us take it that the gun should be as strong as it is at that part to properly withstand the charge. If this is to be so, then evidently, if the effect of the explosion of the front of the two charges were no greater than that which arises from firing it as a single charge (and I shall have presently to show you that it is very much greater), even then the gun ought to be made as large for 11′ 2″ of its length as it is now made for 7′ 5″ of its length. This addition of 3′ 9″ in length of the extra thickness required would add 7 tons to the weight of the gun, but the extra thickness must not stop here, because the portion in advance of the front charge of the two must be as strong as that in advance of the charge when in its proper place. Therefore the gun must be increased in dimension for the whole of its length forward.

In fact it must be made as shown in Diagram (18). This would have the result of adding 12 tons to the weight of the gun, or in other words to provide against an extremely remote probability—a probability which, small as it is, can be diminished, as the Committee pointed out, by a simple appliance, until the probability comes to the verge of an impossibility—it is suggested that the 38-ton gun should be made into a 50-ton gun, while only retaining the efficiency of a 38-ton gun.

These considerations have I trust convinced you of the practical impossibility of attempting to make a 38-ton gun fit to carry two charges placed in the position which they would occupy when the gun is double-loaded, even assuming that the forward charge in that position exploded with no more effect than it would have produced if placed at the base of the barrel, and fired in the ordinary manner. But everything points to the conclusion that the forward one of the two charges when fired explodes with a violence far in excess of anything that would result from its being fired as a single charge.

Assuming, as is most probable, that the ignition takes place from the flash of the hinder charge, and from that alone, this flash would certainly proceed along every one of the rifle grooves, so that in nine equal spaces round about the circumference and for the whole length of the cartridge would ignition take place, instead of its occurring at one point only, that of the firing tube of the ordinary charge, and further when the powder was thus fired it would be highly compressed by the forward movement of the hinder projectile.

The specific gravity of gunpowder without any air space whatever may be taken at $1\frac{3}{4}$ times that of water, or 16 cubic inches to the lb. The space allowed for its combustion is 30 inches to the lb., but by the time the cartridges are rammed home this is perhaps contracted to $27\frac{3}{4}$ inches, or in other words the space then allowed for a given weight of powder is thus just equal to that which would be required to contain an equal weight of water.

Captain Noble on one occasion exploded in a close vessel, powder, when the space it occupied was such as to represent the density of $1\frac{1}{5}$ times that of water. The result was the raising of the pressure from 43 tons, which such powder had when fired in a close vessel giving a space of 30 inches per lb., to 59 tons, so that the diminution of space from that which occurs with the gravity of $\cdot 925$ to that which occurs with the gravity of $1 \cdot 2$ was sufficient to make an increase in pressure of 37 per cent.

But one knows that the front charge of powder when acted upon by the rear shot urged forward by a pressure of some 2500 tons would be so driven together as to obliterate practically all air space, and thus to bring the powder up to its full specific gravity of $1\frac{3}{4}$ times that of water. The result of an explosion from powder thus compressed I do not pretend to offer you in tons, but looking at what happened from the firing of powder of $1\frac{1}{5}$ times the density of water, it is clear the pressure must be enormous. In a small gun the

pressure would be much reduced. Let us imagine the cartridge to be made up of a number of parallel cylinders of powder, each cylinder being 1 inch in diameter, and of the whole length of the cartridge, and assume that the projectile is, in length, say $2\frac{1}{2}$ times the diameter of the bore of the gun. Then, in a 6-inch gun, the projectile would be 15 inches long, and each of the imaginary cylinders of powder of 1 inch diameter would find the expansion of the gases arising from its combustion opposed by the inertia of a cylinder of iron 1 inch diameter by 15 inches long. But if the gun were 12 inches diameter of bore and the shot $2\frac{1}{2}$ diameters in length, then each cylinder of powder of 1 inch diameter would find the expansion of the gases arising from its combustion opposed by the inertia of a cylinder of iron of 30 inches long. This circumstance should be borne in mind when considering the results arising from cases of double-loading which have from time to time occurred in comparatively small cannon.

Further, as pointed out by Mr. Osborne Reynolds, now nearly a year ago, it is not impossible that the front charge may have been ignited by the rise in temperature caused by the conversion of a portion of the energy in the rear projectile, into heat. It is by no means difficult to ignite gunpowder in this way.

The bursting charge of a Palliser shell, for instance, consists of powder without any fuse or detonating composition to ignite it; but by the striking against an armour plate the velocity of the shell is so materially reduced, that the powder contained within it continuing to rush forward, strikes the front end of the shell, and by this mere stoppage generates sufficient heat to ignite itself.

I have here an apparatus similar to one used by Professor Abel many years ago to investigate this question. Professor Abel, I am glad to say, is present with us and will exhibit it to you. It consists simply of a falling weight, the effect of which is received upon a small brass plate lying upon a thickness of one-twentieth of an inch of powder, having an area of about one-fifth of a square inch; the weight is 50 lbs., and by falling from a height of 12 feet it explodes the powder.

I may mention that a gunpowder pile-driving engine has been used. I have not seen it in operation, but according to what I have read of it, a charge of powder being placed in a cavity in an iron cap on the pile and being struck by the falling monkey, is thereby exploded, driving the pile downwards and the monkey upwards; and then a fresh charge being inserted before the monkey has time to descend, is in its turn exploded; and in this way the pile is driven.

As you see, explosion can be obtained by the mere effect of a small falling weight, and it does not seem improbable, looking at the immense energy in the rear of the front charge of powder, that enough heat may be generated to raise that powder to the point of combustion, either just in the rear of the cartridge, causing a local raising of the temperature and the ignition of the powder at this part, or what

would be the most destructive of all, producing throughout the mass a rise of temperature to the inflaming point, and thus causing a detonation under the following circumstances:—Powder compressed to its fullest specific gravity, powder heated up all through to the point of explosion, and powder ignited at every portion of its bulk at the same moment.

With respect to the heating, I find I have not called your attention to the importance—the twofold importance—of this. The first way in which it is important is, that heating renders the chemical changes which occur when the powder is inflamed more rapid, and thereby causes the explosion to be more violent than it would be if the temperature were not raised. The second way in which the heating up is important is owing to the fact, that, when powder explodes, the effect of the explosion is due, not alone to the expansion which arises from the conversion of the solid into a gas, but very largely to the further expansion which arises from the heating up of the gas produced. Anything therefore which abstracts heat from the products of the explosion, diminishes the expansion of the gas, and thereby diminishes the effect produced. Now, assuming the inflaming point of gunpowder to be about 660 degrees, and assuming it to be of the temperature of the atmosphere, or say 60 degrees, the whole weight of gunpowder has to be heated 600 degrees before it attains the point of combustion, and this heat is abstracted from that which would otherwise go to augment the bulk of the gases. I am not speaking now of the heat requisite to convert the solid into a gas, for that must be expended in any event, but I am speaking of the heat needful to raise the solid from the ordinary temperature of the air to the temperature of ignition.

We have again to thank Professor Abel for an experiment which will show you the effect on the burning of powder of the rapid abstraction of heat.

There are here two similar pieces of pebble powder. One has been cooled down to freezing, the other has been heated up to about 350°. On firing them you will see that while the first burns slowly, the second burns more rapidly.

Another mode of showing the effect arising from the abstraction of heat was devised by Professor Abel many years ago. There is here an exhausted receiver, containing powder at the ordinary temperature. On igniting it by a wire heated electrically up to whiteness, it will be found that the powder, instead of exploding, will burn in the immediate neighbourhood only of the wire; indeed, if we can succeed in throwing the image on the screen, the powder will appear to be boiling, and producing a vapour; and it will not be until after such an interval that the production of gas has partially restored pressure in the receiver, or until air is admitted, that the residue of the powder will explode, the reason being that in the vacuous condition existing here, the gas generated expands with great readiness, and takes up the heat rapidly, and this goes on until, as has

Diagram 1

Diagram 2

BATTERING CHARGE PALLISER SHELL

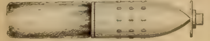


Diagram 3

FULL CHARGE COMMON SHELL

Diagram 4

REPORT OF THE FIRING AT THE ROYAL GUN FACTORY PROOF BUTTS OF No2 12 INCH 38 TON GUN FROM "H M S THUNDERER" WITH PALLISER SHELLS

Proof Butts 22nd Dec. 1879

Distance of Base of Projectile from Cartridge	Charge of P. Powder: Weight lb.	Charge of P. Powder: Density	Projectile weight including Gas Check lb.	Observed Velocity at 100 feet (f. per second)	Muzzle Velocity (f. per second)	Pressures tons per square inch: Base of Cartridge	Pressures tons per square inch: Base of Projectile	Recoil of Carriage on Slide (ft. in.)
Nil	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]

Diagram 5

REPORT OF THE FIRING AT THE ROYAL GUN FACTORY PROOF BUTTS OF No2 12 INCH 38 TON GUN FROM "H M S THUNDERER" WITH COMMON SHELLS

Proof Butts 22nd Dec. 1879

Distance of Base of Projectile from Cartridge	Charge of P. Powder: Weight lb.	Charge of P. Powder: Density	Projectile weight including Gas Check lb.	Observed Velocity at 100 feet (f. per second)	Muzzle Velocity (f. per second)	Pressures tons per square inch: Base of Cartridge	Pressures tons per square inch: Base of Projectile	Recoil of Carriage on Slide (ft. in.)
Nil	[illegible]	[illegible]	[illegible]	[illegible]	1418	[illegible]	[illegible]	[illegible]
[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]

Diagram 6.

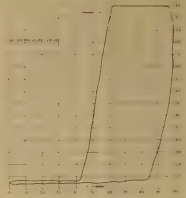


Diagram 7.

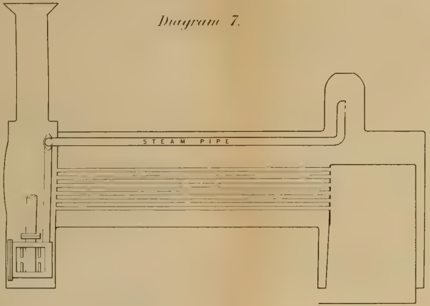


Diagram 8.

REPORT OF THE FIRING AT THE ROYAL GUN FACTORY PROOF BUTTS OF No 2 12 INCH 38 TON GUN, FROM "H.M.S THUNDERER" WITH COMMON SHELL.

Proof Butts 16th Jany 1880.

Distance of Base of Projectile from Cartridge	Charge of P Powder		Projectile weight including Gas Check	Observed Velocity at 162 feet	Muzzle Velocity	Pressures tons per square Inch		Recoil of carriage on Slide	Remarks
	Weight lbs	Density	lbs oz	ft per second	ft per second	Base of Cartridge	Base of Projectile	ft in	
Nil	85	21 57 1 130	592 6	1598 1403	1409	20 7	17 6	3 7½	Wad placed 5 feet in front of Shell and at an angle of 45

Diagram 9.

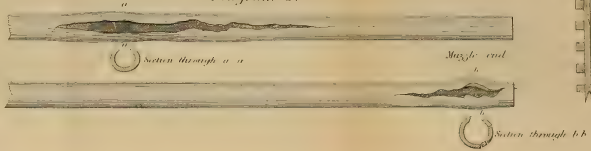


Diagram 10.

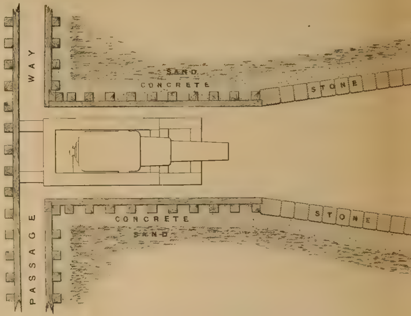


Diagram 10.

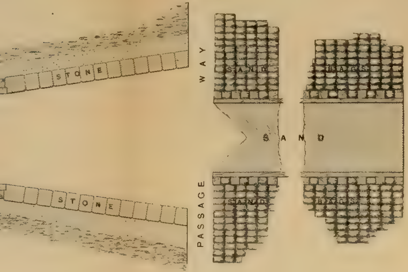


Diagram 16.

Crusher Gauge before firing

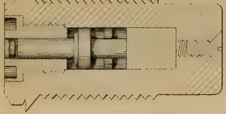


Diagram 17.

Crusher Gauge after firing

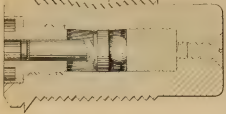


Diagram 18.

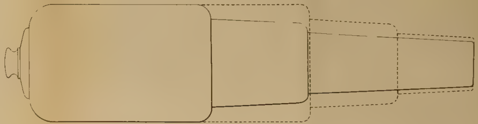


DIAGRAM 11.

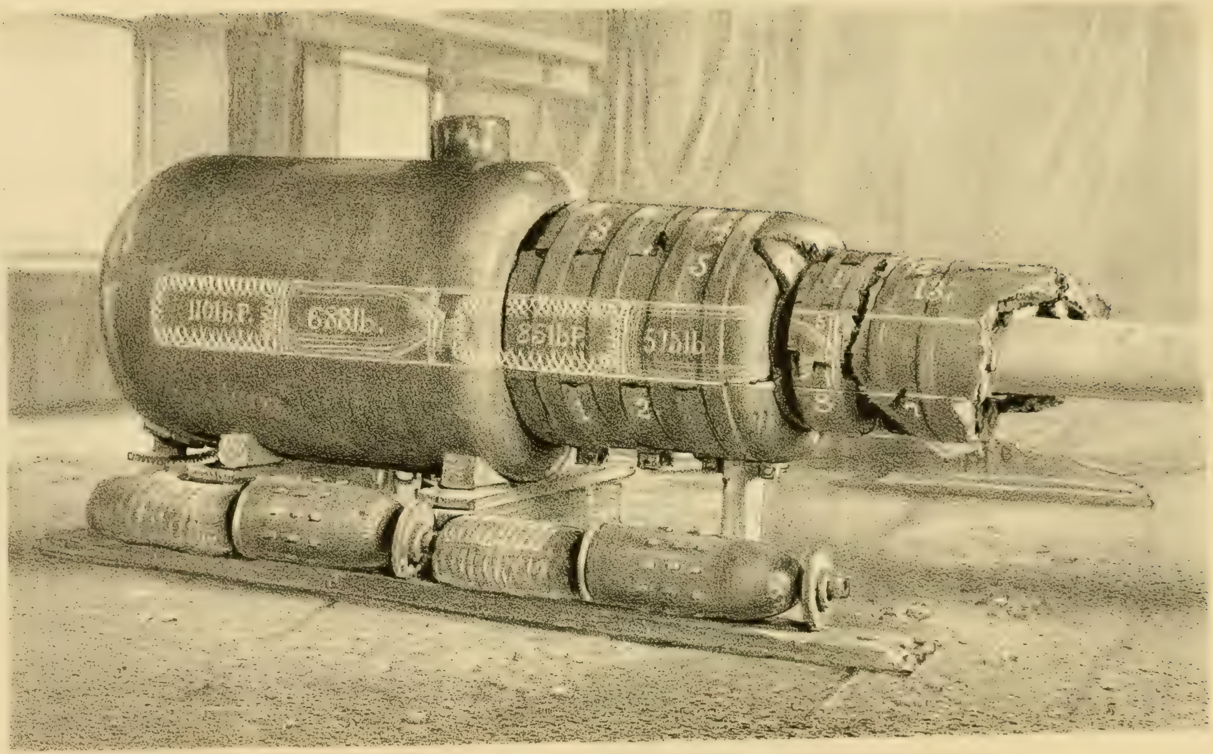


DIAGRAM 12.

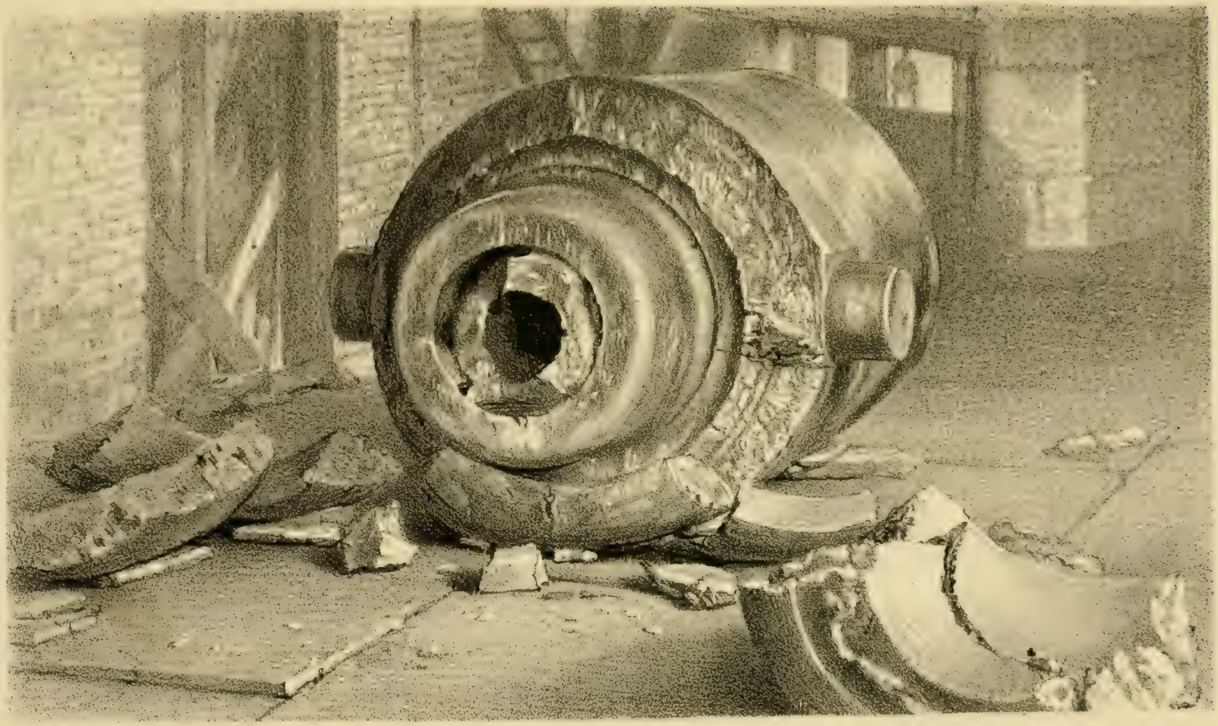
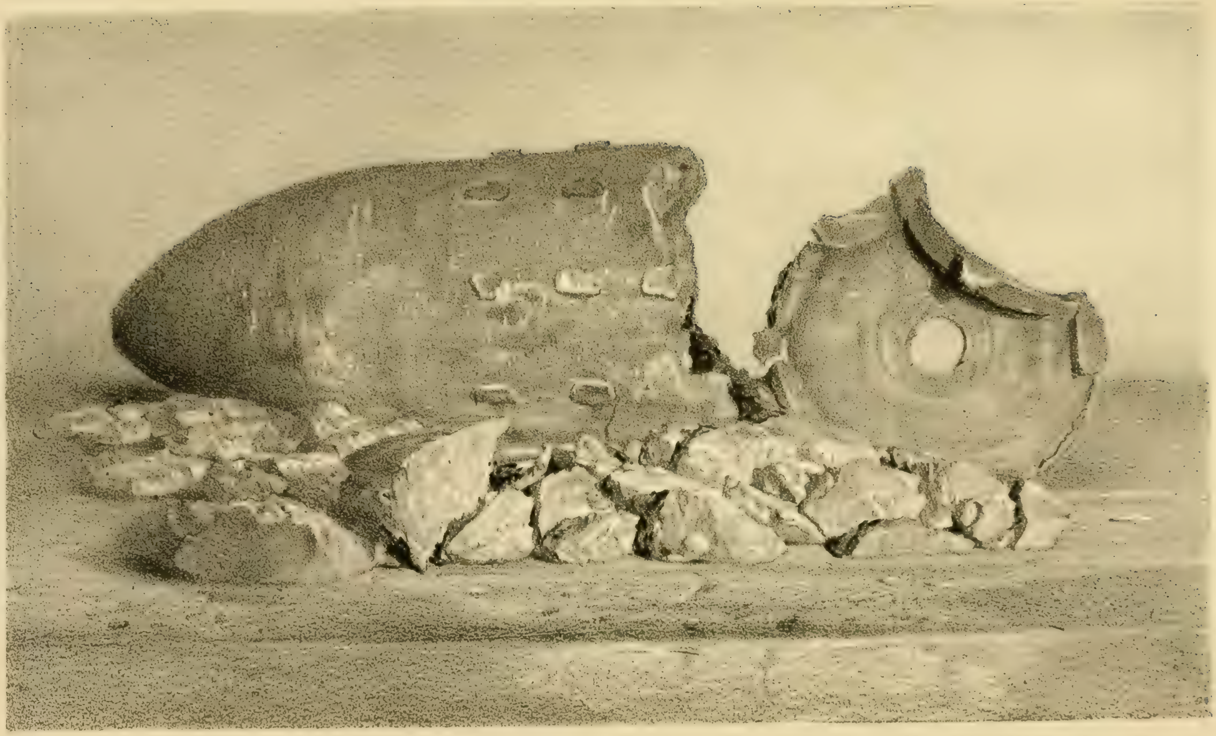


DIAGRAM 13.

DIAGRAM 14.

DIAGRAM 15.



been said, the accumulation of the gas in the receiver or the admission of air has destroyed the vacuum, and then the expansion not being so great, and the absorption of the heat being correspondingly less, the powder explodes.

I trust that the public, and above all the officers and men who are to work guns such as these on board the 'Thunderer,' are now thoroughly convinced that these guns are safe, when properly used, and are only unsafe when, by almost a miracle of ill luck, they are double-loaded. And looking at the large number of guns of this design of large weights and calibres which are provided for our turret ships and our fortifications, it is in the highest degree necessary that the absolute safety of these guns should be thoroughly established. They have cost much money, they have involved a large expenditure of time, and they are the guns on which, at the present moment at all events, the country has to rely for its safety in the event of war.

But it may well be, looking at the fact of the increase in the length of guns which is taking place, owing to the difference in the nature of the powder used, that muzzle loading will cease to be practicable with guns of the largest dimensions, and that breech loading will be employed. If this be so, then all danger of a double charge is done away with.

There are, of course, many attendant difficulties, and care will have to be exercised as well with a breech-loader, as with a muzzle-loader, and in fact one can conceive, that it will require some practice, or some special provision, before those who load breech-loading guns can be taught to believe, that the shot has to be put into the gun point foremost, and that it has to precede the powder, and not to follow it. But this question of breech-loaders is one upon which I must not further enter; at all events, not this evening.

It only remains for me to thank the authorities for their kindness in allowing the specimens from the broken gun to be exhibited here to-night, and to thank you for the attention with which you have listened to that which is, of necessity, to a very considerable extent, a twice-told tale.

[F. J. B.]

GENERAL MONTHLY MEETING,

Monday, March 1, 1880.

Edward Frankland, Esq. D.C.L. F.R.S. &c. Manager, in the Chair.

The following Letter was read:—

"Buckingham Palace, *Feb. 5th*, 1880.

"Sir,

"His Royal Highness Prince Leopold begs you will intimate to the Members of the Royal Institution his sense of the honour they have done him in electing him a Member. Faithfully yours,

"R. H. COLLINS,
"*Comptroller.*"

Forster Fitz-Gerald Arbuthnot, Esq.
Richard Claude Belt, Esq.
Shelford Bidwell, Esq. M.A. LL.B.
James Crichton-Browne, Esq. M.D. LL.D. F.R.S.E.
Henry G. Bunbury, Esq.
Miss Isabella Clerk,
Vicat Cole, Esq. A.R.A.
Alfred Kingsford Coles, Esq.
Frederick Thomas Jennings, Esq.
Alfred Lloyd, Esq. B.A. F R.G.S.
William Mansell MacCulloch, Esq. M.D.
Miss Louisa Millar,
Miss Isabella Milne,
Major H. C. Roberts,
Isaac Seligman, Esq.
Mrs. Isaac Seligman,

were elected Members of the Royal Institution.

The following Arrangements for the Lectures after Easter were announced:—

Professor Huxley, LL.D. F.R.S.—Two Lectures on Dogs, and the Problems connected with them; on Tuesdays, April 6, 13.

Robert Henry Scott, Esq. M.A. F.R.S. Secretary to the Council of the Meteorological Office.—Four Lectures on Wind and Weather; on Tuesdays, April 20, 27, and May 4, 11.

John Fiske, Esq.—Three Lectures on American Political Ideas viewed from the standpoint of Universal History; on Tuesdays, May 18, 25, and June 1.

Professor Tyndall, D.L.C. F.R.S.—Six Lectures on Light as a Mode of Motion; Theories of Colour; on Thursdays, April 8 to May 13.

T. W. Rhys Davids, Esq.—Three Lectures on the Sacred Books of the Early Buddhists; on Thursdays, May 20, 27, and June 3.

James Sully, Esq.—Three Lectures on Art and Vision; On Saturdays, April 10, 17, 24.

Professor Henry Morley.—Five Lectures on the Dramatists before Shakespeare, from the Origin of the English Drama to the Year of the Death of Marlowe (1593); on Saturdays, May 8 to June 5.

The Special Thanks of the Members were returned to Mr. Frederick B. Garnett for his present of a Portrait of Dr. Thomas Garnett, the first Professor of Natural Philosophy and Chemistry in the Royal Institution (1799–1801).

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Lords of the Admiralty—Greenwich Observations for 1877. 4to. 1879.
Cape of Good Hope Astronomical Observations for 1876. 8vo. 1879.
British Museum Trustees—Fac-Similes of Ancient Charters, Part IV. fol. 1878.
Accademia dei Lincei, Reale, Roma—Atti, Serie Terza: Transunti: Tome IV. Fasc. 2. 4to. 1879.
Adams, Professor W. G. M.A. F.R.S. (the Author)—Measuring Polariscopes. (Phil. Mag. Oct. 1879.)
Bakerian Lecture. (R. S. Proceedings, 1875.)
Action of Light on Selenium. (Phil. Trans. Vol. CLXVII.) 1876.
Antiquaries, Society of—Proceedings, Second Series, Vol. VIII. No. 1. 8vo. 1880.
Astronomical Society, Royal—Monthly Notices, Vol. XL. No. 3. 8vo. 1880.
Ball, Professor R. S. LL.D. F.R.S. (the Author)—Thirteen Mathematical Papers. 4to and 8vo. 1869–78.
British Architects, Royal Institute of—1879–80: Proceedings, No. 9. Transactions. No. 4, 5. 4to.
Chemical Society—Journal for Feb. 1880. 8vo.
Editors—American Journal of Science for Feb. 1880. 8vo.
Analyst for Feb. 1880. 8vo.
Athenæum for Feb. 1880. 4to.
Chemical News for Feb. 1880. 4to.
Engineer for Feb. 1880. fol.
Horological Journal for Feb. 1880. 8vo.
Iron for Feb. 1880. 4to.
Journal for Applied Science for Feb. 1880. fol.
Nature for Feb. 1880. 4to.
Telegraphic Journal for Feb. 1880. 8vo.
Frankland, Edward, Esq. Ph.D. D.C.L. F.R.S. M.R.I. (the Author)—Water Analysis for Sanitary Purposes, with Hints for the Interpretation of the Results. 16to. 1880.
Franklin Institute—Journal, No. 650. 8vo. 1879.
Geneva: Société de Physique—Mémoires. Tome XXVI. Partie 2. 4to. 1879.
Geographical Society, Royal—Proceedings, New Series. Vol. II. No. 2. 8vo. 1880.
Geological Society—Quarterly Journal, No. 141. 8vo. 1880.
Heneage, Charles, Esq. M.R.I. (the Translator)—Journey in the Caucasus, Persia, and Turkey in Asia. By Lieut. Baron Max von Thielmann. 2 vols. 12mo. 1875. (Another copy, presented by Mr. Murray, the Publisher.)
Liverpool Polytechnic Society—Journal, various Nos. 8vo. 1879–80.
London Corporation—Catalogue of Library: 15th Supplement. 8vo. 1879.
Manchester Geological Society—Transactions, Vol. XV. Parts 10, 11. 8vo. 1879.
Mathieson, Frederick C. Esq. M.R.I. (the Author)—New Map of the Railway System of England and Scotland. 1 Jan. 1880. (On Roller.)

O. K. (*the Author*)—Russia and England from 1876 to 1880, with a Preface by J. A. Froude. 8vo. 1880.

Pharmaceutical Society—Journal, Feb. 1880. 8vo.

Photographic Society—Journal, New Series, Vol. IV. No. 4. 8vo. 1880.

Physical Society of London—Proceedings, Vol. III. Part 3. 8vo. 1880.

Preussische Akademie der Wissenschaften—Monatsberichte: Nov. 1879. 8vo.

Royal Society of London—Proceedings, No. 200. 8vo. 1880.

Royal Society of New South Wales—Journal of Proceedings, Vol. XII. 8vo. 1879.

Sandys, R. Hill, Esq. M.A. (*the Author*)—In the Beginning: Remarks on Certain Modern Views of Creation. 2nd Edition. 16to. 1880.

Schäfer, E. A. Esq. (*the Author*)—Some Teachings of Development. (K 103) 8vo. 1880.

Society of Arts—Journal for Feb. 1880. 8vo.

Squire, Peter, Esq. F.L.S. M.R.I. (*the Author*)—Companion to the Latest Edition of the British Pharmacopœia. 12th Edition. 8vo. 1880.

St. Petersbourg, Académie des Sciences—Bulletins, Tome XXVI. Nos. 12, 13, 14; Tome XXVII. No. 1. 4to. 1879.

Symons, G. J.—Monthly Meteorological Magazine, Feb. 1880. 8vo.

United Service Institution, Royal—Journal, No. 103. 8vo. 1879.

Verein zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, 1880: Heft 2.

WEEKLY EVENING MEETING,

Friday, March 5, 1880.

George Busk, Esq. F.R.S. Treasurer and Vice-President, in the Chair.

H. N. Moseley, Esq. M.A. F.R.S.

Deep-Sea Dredging and Life in the Deep Sea.

(This lecture has been published in full in 'Nature.')

After some account of the physical conditions under which life occurs in the deep sea, the lecturer described the improved apparatus for deep-sea dredging, which has been introduced since the 'Challenger' Expedition by Mr. Alexander Agassiz. A steel wire rope with a hemp core only $1\frac{1}{16}$ inch in circumference is used. Three or four hauls can be made with it in a day in depths in which only one would be made by the 'Challenger.' A reversible trawl is used in the place of the old net, which was useless if it fell on its back on the bottom.

All animal and plant life originated in the sea in shallow water, and spread thence on to the land and into the depths. Only one plant exists in the depths, a parasitic fungus which infests corals. The absence of sunlight excludes others. Many genera of all groups of animals and even not a few species range from the shores down to very great depths. One species of coral ranges from 30 to 2900 fathoms. There are, however, certain well-marked deep-sea forms which are not now met with in shallow water, unless in Polar regions.

There being scarcely any difference in the physical conditions of life from a depth of 500 fathoms downwards, the deep-sea fauna exhibits no zones of distribution in depth. Its upper limit varies much in position, approaching shallow water in the higher latitudes, but even in some places in the tropics coming within 300 or possibly 100 fathoms of the surface. It is impossible to recognize a geological deposit as having been formed in the deep sea, from the nature of its fossil contents.

The deep-sea fauna is world-wide in its distribution, there being

no barriers to hinder migration. Mr. Agassiz has dredged off the West Indies and the east coast of North America nearly all the types obtained by the 'Challenger' all over the world.

It is probably as correct to say that deep-sea animals have colonized the shallower waters of the Polar regions as to say that Arctic animals have occupied the deep sea elsewhere.

There seems to be a close relation between the pelagic fauna and deep-sea fauna. There are surface-swimming representatives of most deep-sea forms. Rhizopoda both with calcareous and arenaceous tests live abundantly in the deep sea, and Mr. Henry Brady concludes that the Globigerina mud is formed by animals living at the bottom, the pelagic representatives of these bearing but a small proportion to the main mass.

The most important question now remaining to be solved with regard to deep-sea life is the range of life at the various depths between the surface and the bottom of the ocean. At present nothing is known with regard to this matter. Possibly there is a wide zone between the surface and bottom devoid of life. The lecturer described a net which he had devised, by which this question could be determined. By means of an electrical arrangement its mouth can be opened and closed at will at any depth. Mr. Agassiz intends to make use of it or some similar appliance. Some deep-sea animals possibly pass their early stages of existence at the sea surface.

The deep-sea basins being very probably of the remotest geological antiquity, it is remarkable that scarcely any ancient animals occur amongst the deep-sea fauna. Almost all the most ancient forms known are from comparatively shallow water; for example, Heliopora, Limulus, Lingula, Trigonia, Nautilus, Amphioxus, lampreys, ganoids, and Cestracion. The deep-sea fish are of modern origin, allies of the cod, salmon, and angler. Scarcely a single animal of first-rate zoological importance has been found in the deep sea.

The deep sea was probably uninhabitable in early geological times, being highly charged with salts and gases in solution and mud in suspension, the results of its primitive superheated condition.

The deep-sea animals must depend for sight entirely on the phosphorescent light of themselves or others. The sense of touch is probably mainly relied on by them. Investigations on their organs of hearing might give valuable results. None have as yet been made.

The deep-sea animals are more abundant towards the upper limit of their range; the ultimate source of their food is the sea surface, or derived from the land and shore. Their food is probably always most abundant near coasts.

Some animals are dwarfed, others enlarged by deep-sea conditions of life. A Pycnogonid was obtained measuring two feet in length.

The usual parasites have accompanied their hosts into deep waters.

Some deep-sea animals are brilliantly coloured, having retained colours which were effective in shallow-water ancestors; but colours

are often useless to the animals possessing them as such, and may be merely accidental properties of chemical compounds required for other physiological purposes. A large bottle full of a solution of the bright red colouring matter of a deep-sea Pentacrinus was exhibited to the audience, and the light was also thrown on the screen through cells containing the solution. It is green when alkaline, red when acid, and yields well-marked absorption-bands in the spectrum, which were shown upon the screen. Other colouring matters of deep-sea animals yield well-defined absorption spectra, and are hence easily identified. In several instances the lecturer has proved them to be identical with those of allied shallow-water or pelagic forms.

The lecture was illustrated by numerous photographic figures thrown upon the screen.

[H. N. M.]

WEEKLY EVENING MEETING,

Friday, March 12, 1880.

The Duke of Northumberland, D.C.L. LL.D. Lord Privy Seal, President, in the Chair.

C. William Siemens, Esq. D.C.L. LL.D. F.R.S. *M.R.I.*

The Dynamo-Electric Current and some of its Applications.

(Abstract.)

The lecturer commenced with a reference to Faraday's great discovery of the magneto-electric or induced current, which was first shown to the members of the Royal Institution in 1831. So slight and instantaneous was the current, that although Faraday had from *à priori* reasoning arrived as early as 1824 at the conclusion that such a current must be set up in a coil surrounding an armature, when the latter was forcibly severed from a permanent magnet, seven years elapsed before he could detect that current with the instruments then at his command.

It was further shown that although each induced current was feeble and only instantaneous in its action, it differed from a galvanic current in the important particular that it was the immediate outcome of the expenditure of mechanical force, and that by repeating the operation of severance by suitable mechanical arrangements a rapid succession and an aggregation of these currents could be directed through a metallic conductor, and produce in it all the phenomena of a continuous current of great magnitude. The single current revealed by Faraday's original experiment might be likened to the single drop of rain, which, though impuissant by itself, was, when repeated often enough in its fall upon an elevated plateau, capable of giving rise to streamlets and streams, until at last a mighty river and a source of power, such as the Falls of Niagara, might be produced. It was shown by experiment that the single current resulting from the forcible severance of an armature from its magnet was capable, if directed through the coils of another armature in contact with its own magnet, of effecting its severance from the same; and that the force expended bore a definite relation to the force obtained in moving the second armature. In viewing this experiment by the light of advanced science it followed that in this way the conversion of mechanical force into electric current, and

from electric current back into mechanical force, was clearly demonstrated.

The utilization of the induced current had, however, been a work of much time and thought on the part of those who followed in the wake of the great discoverer. One of the first attempts to utilize the magneto-electric current in telegraphy was made by Wheatstone in 1844, when he brought out his magneto-electric step-by-step instrument. But, notwithstanding the great ingenuity displayed in the same, the current induced was found practically insufficient to move the receiving instrument with a sufficient degree of certainty.

An important step towards aggregating magneto-electric currents was made by Dr. Werner Siemens in 1856, who constructed an armature resembling in section a double-headed rail, or double I, into the hollow of which the insulated wire was wound longitudinally. In mounting this armature upon bearings, and giving it a rapid rotation between the poles of a series of permanent magnets, an accumulative effect was produced through the simultaneous action of each permanent magnet in setting up a current in one and the same coil; thus a succession of currents was set up, which, when directed by means of a commutator into an outer metallic circuit, was capable of producing a continuous current of considerable power. A magneto-electric step-by-step instrument constructed on this principle was shown in operation, and also a more powerful arrangement of the same description for exploding mines, and for igniting platinum wire. It was also shown that in turning the handle of such a machine, and connecting its leading wires to another similar machine, motion was set up in the latter, and sufficient force was obtained to work a ventilator with considerable effect.

The magneto-electric machines of Holmes and Wilde were next passed in review, which it was shown marked a further step towards the attainment of powerful effects by the accumulation of magneto currents when steam power was employed for their production.

The dynamo-electric principle attributable to Werner Siemens and Wheatstone was next adverted to, and the first machine constructed on this principle by the lecturer, and brought by him before the Royal Society in 1867, was shown in operation. This machine differed from magneto-electric arrangements in the substitution of electro-magnets for permanent or steel magnets, which electro-magnets were excited by the current produced by the rotation of the helix or armature of the machine itself. The advantage of the machine consisted in the accumulative action it evolved, giving rise to currents of considerable magnitude which were strictly proportionate to the mechanical power expended.

The adaptations of this accumulative principle by Professor Pacinotti, by Gramme, by von Heftner-Alteneck, and others, were alluded to, leading up to a recent modification of the dynamo-electric machine by the lecturer, by which a further increase in the strength of current and improved steadiness of action could be realized. The

form of the machine was not materially altered by this change, which consisted in so arranging the wire on the rotating helix and on the exciting electro-magnets, that the maximum current produced for the power expended was attained when the outer resistance was such as was usually required.

Amongst the applications of the dynamo-electric current, the lecturer showed in the first place that of the transmission of power, illustrating this portion of his subject by working a circular saw, receiving its motion from a dynamo-electric machine (constructed according to the modified plan alluded to) placed in the basement of the Royal Institution and receiving motive power in its turn from a gas engine. It was shown that by such an arrangement 60 per cent. of the engine power expended could be utilized at another place, and that thus natural sources of power, such as waterfalls, might be made available for supplying motive power at distances even of twenty or thirty miles; or power might be transmitted to the depths of mines and collieries by the establishment of a stout leading-wire connected with an electro-motor on the bank.

The lecturer next described a novel application of the dynamo-electric current for the propulsion of tramway cars upon railways, by preference upon elevated railways. Dr. Werner Siemens had made such an application very successfully last year in connection with a Berlin Exhibition, and the experiment would very shortly be repeated at the Crystal Palace. One of the carriages composing the train was fitted with an ordinary dynamo machine, and another similar machine was worked on terra firma by engine power. A central rail or conducting rope was introduced for the conveyance of the current, the return circuit being completed through the side rails, and the person in charge on the train could by moving a handle start and stop the train as required. The tractive force was considerable, and increased with the resistance, amounting in ascending an incline to 200 kilogrammes, and falling to 70 or 80 kilogrammes on level ground. From thirty to forty persons were conveyed easily at a speed of from ten to twelve miles an hour.

Dr. Siemens explained that whenever a current was passed through a conductor, a loss was incurred varying as the square of the intensity of the current and as the resistance encountered, but that what was loss of current when the object was the simple transfer of electrical energy might be turned into a gain where light and heat were to be produced. Platinum and iridium were notoriously bad conductors, and on putting a piece of wire of these metals into a circuit they become hot and luminous, as was well known. It would readily be conceived that the greater the electrical resistance in any one point of a circuit the greater must be the luminosity produced, and Sir Humphry Davy had shown as far back as 1810, before the Royal Institution, that the greatest local resistance, and the highest degree of heat and luminosity, could be produced in the electric arc between two carbon electrodes placed a short distance apart.

The electric light was therefore no novelty; but the interest attaching to it at the present time was entirely due to the comparatively cheap rate at which the electric current could be produced by the expenditure of mechanical energy resulting from the combustion of coal, whereas formerly zinc had to be consumed or burnt in the galvanic battery. Much ingenuity had been displayed of late in devising electric lamps and electric candles, the various devices proposed having for their object to produce a steady action. These might be divided into two classes—the *glow lights*, and the *regulators* of the electric arc. It was shown that glow lights furnished the most simple solution of the problem, but could never rival the arc in economy of result, because the intensity of the latter could be made to approach that of solar light, whereas glow lights were limited in intensity to the fusing or dispersing point of the conductor employed.

It had been proved that even in the electric arc the major portion of the rays emitted were heat rays, but in the best glow light probably not more than 2 or 3 per cent. were rays of high luminosity, all the rest being lost as regards the effect to be produced. It was also shown that greater efficiency could be obtained from a powerful arc than from divided arcs, and that therefore the development of electric lighting should be sought in the direction of creating powerful centres of light, and not in its subdivision.

Electric light, properly applied, was much cheaper than gas light, but was not likely to supplant the latter for purposes where great subdivision was indispensable; besides which, gas was essentially a heating agent, and would find ever-increasing application in that direction.

The sensitiveness of gas shares to the announcements of mere varieties of glow lights showed that the principles upon which electric lighting depended were not sufficiently appreciated. The regulation of the electric arc to the varying conditions of current and to imperfections in the carbons was, however, a matter of practical difficulty, which admitted of an almost unlimited number of solutions. The question was which practical combination was at the same time the most simple and efficient. The lecturer had himself worked out several solutions, one of which recommended itself by its absence of all clockwork arrangement in making the advancing carbons abut against a fixed metallic stop. Other solutions might, however, have their particular advantages, but it was not the purpose of his present lecture to enter upon a consideration of such details of arrangement.

The electric light, if properly carried into effect, was a cheap light. By burning, for instance, a thousand cubic feet of gas in burners, and consuming the same quantity in a gas engine, giving motion to a dynamo-electric machine feeding an electric light, it could be shown, as the lecturer had done before the House of Commons Committee on Lighting by Electricity, that about twenty times the luminous effect

would be produced in the latter case. In practical working, the difference would not be so great, owing to the imperfect arrangements as yet adopted; but he could show, from actual experience extending over several months, that electric illumination if applied to halls, and places of a certain magnitude, was three or four times cheaper than gas lighting.

The application of the dynamo-electric current next dealt with was that of the fusion of metals and other substances. Sir Humphry Davy had as early as 1810 obtained in the electric arc sufficient heat to decompose potash, and Professor Dewar in experimenting with the dynamo-electric current had shown recently that in his lime tube or crucible several of the metals assumed the gaseous condition as shown by the reversal of the lines in his spectrum, thus proving that the heat obtained by him was not much inferior to solar heat.

The lecturer had experimented for some time with an electric furnace, not with a view of attaining these extreme degrees of heat, but rather with the practical object of melting such refractory materials as platinum, iridium, and steel in considerable quantities. He was led to these experiments by the consideration that a good steam engine converted nearly 15 per cent. of the heat energy residing in coal into mechanical effect, and that by the dynamo-electric machine nearly 80 per cent. of that mechanical energy could be converted into electric energy. If this could be expended without loss within an electric furnace or crucible, 12 per cent. of the total energy residing in the coal would be conveyed to the refractory material to be melted at any degree of temperature required, and such a result would far exceed in economy that of the best furnaces yet constructed.

In the small furnace placed before the meeting, the positive electrode (of iron) entered from below a crucible containing the metal to be melted, whereas the negative electrode was in the shape of a rod of carbon, or of a metal tube cooled by a current of water, which, descending through the crucible cover, was attached by means of a lever to a solenoid regulator. The crucible was packed in charcoal or other non-conducting material contained in a copper vessel to prevent loss of heat, and so great was the heat accumulated within the crucible, that in the course of about twenty minutes a kilogramme of broken files was completely melted. The arrangement was such that it could easily be applied upon a larger scale, and electric fusion had the great advantage that the access of the atmosphere and of the products of combustion to the substance under treatment was entirely prevented.

Another application of the electric arc which the lecturer thought might ultimately assume an important character was to horticulture. Having experimented with an electric light of 1400 candle power in his own greenhouses at Sherwood, near Tunbridge Wells, he had arrived at the conclusions that electric light promoted the formation of chlorophyll, starch, and cellulose in plants, and could be made

nearly equal to daylight in its beneficial effects upon them; that the plants did not require a time of rest by night, but became all the more vigorous if put under the influence of day and electric light alternately without intermission, and that the development of flowers and the ripening of fruit would be greatly accelerated and improved through the action of electric light. Various plants, including mustard, carrots, peas, roses, lilies, and strawberries with the fruit partially developed were exhibited, a portion of which had been exposed to daylight only, another to electric light only, being kept in the dark during the daytime, and a third portion to day and electric light, which clearly justified the conclusions already stated.

The cost of electric light in this application, if steam had to be used for its production, would no doubt be considerable, but not too great probably to prevent its being employed for forcing early vegetables and fruits; but its cost would be quite inconsiderable in situations where water power could be made available.

Other applications of the dynamo-electric current for electro-decomposition, photography, and telegraphy, could only be alluded to by the lecturer, but enough had been said, he thought, to show the extraordinary uses to which Faraday's great discovery made fifty years ago was likely to lead.

[C. W. S.]

WEEKLY EVENING MEETING,

Friday, March 19, 1880.

George Busk, Esq. F.R.S. Treasurer and Vice-President, in the Chair.

Professor Tyndall, D.C.L. F.R.S.

Goethe's 'Farbenlehre.'

In the days of my youth, when life was strong and aspiration high, I found myself standing one fine summer evening beside a statue of Goethe in a German city. Following the current of thought and feeling started by the associations of the place, I eventually came to the conclusion that, judging even from a purely utilitarian point of view, a truly noble work of art was the most suitable memorial for a great man. Such a work appeared to me capable of exciting a motive force within the mind which no purely material influence could generate. There was then labour before me of the most arduous kind. There were formidable practical difficulties to be overcome, and very small means wherewith to overcome them, and yet I felt that no material means could, as regards the task I had undertaken, plant within me a resolve comparable with that which the contemplation of this statue of Goethe was able to arouse.

My reverence for the poet had been awakened by the writings of Mr. Carlyle, and it was afterwards confirmed and consolidated by the writings of Goethe himself. But there was one of the poet's works, which, though it lay directly in the line of my own studies, remained for a long time only imperfectly known to me. My opinion of that work was not formed on hearsay. I dipped into it so far as to make myself acquainted with its style, its logic, and its general aim; but having done this I laid it aside, as something which jarred upon my conception of Goethe's grandeur. The mind willingly rounds off the image which it venerates, and only acknowledges with reluctance that it is on any side incomplete; and believing that Goethe in the 'Farbenlehre' was wrong in his intellectual, and perverse in his moral, judgments—seeing above all things that he had forsaken the lofty impersonal calm which was his chief characteristic, and which had entered into my conception of the god-like in literature—I abandoned the 'Farbenlehre,' and looked up to Goethe on that side where his greatness was uncontested and supreme.

But in the month of May, 1878, Mr. Carlyle did me the honour of calling upon me twice; and I, not being at home at the time, visited him in Chelsea soon afterwards. He was then in his eighty-third year, and looking in his solemn fashion towards that portal to which we are all so rapidly hastening, he remembered his friends. He then presented to me, as "a farewell gift," the two octavo volumes of letter-press and the single folio volume, consisting in great part of coloured diagrams, which are here before you. Exactly half a century ago these volumes were sent by Goethe to Mr. Carlyle. They embrace the 'Farbenlehre,'—a title which may be translated, though not well translated, 'Theory of Colours'—and they are accompanied by a long letter, or rather catalogue, from Goethe himself, dated the 14th of June, 1830, a little less than two years before his death. My illustrious friend wished me to examine the book, with a view of setting forth what it really contained. This year for the first time I have been able to comply with the desire of Mr. Carlyle; and as I knew that your wish would coincide with his, as to the propriety of making some attempt to weigh the merits of a work which exerted so great an influence in its day,* I have not shrunk from the labour of such a review.

The average reading of the late Mr. Buckle is said to have amounted to three volumes a day. But they could not have been volumes like those of the 'Farbenlehre.' For the necessity of halting and pondering over its statements was so frequent, and the difficulty of coming to any undoubted conclusion regarding Goethe's real conceptions was often so great, as to invoke the expenditure of an inordinate amount of time. I cannot even now say with confidence that I fully realize all the thoughts of Goethe. Many of them are strange to the scientific man. They demand for their interpretation a sympathy beyond that required, or even tolerated, in severe physical research. Two factors, the one external and the other internal, go to the production of every intellectual result. There is the evidence without and there is the mind within on which that evidence impinges. Change either factor, and the result will cease to be the same. In the region of politics, where mere opinion comes so much into play, it is only natural that the same external evidence should produce different convictions in different minds. But in the region of science, where demonstration instead of opinion is paramount, such differences ought hardly to be expected. That they nevertheless occur is strikingly exemplified by the case before us; for the very experimental facts which had previously converted the world to Newton's views, on appealing to the mind of Goethe, produced a theory of light and colours in violent antagonism to that of Newton.

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* The late Sir Charles Eastlake translated a portion of the 'Farbenlehre'; while the late Mr. Lewes, in his 'Life of Goethe,' has given a brief, but very clever account of the work. It is also dealt with, in connection with Goethe's other scientific labours, in Helmholtz's Lectures.

Goethe prized the 'Farbenlehre' as the most important of his works. "In what I have done as a poet," he says to Eckermann, "I take no pride, but I am proud of the fact that I am the only person in this century who is acquainted with the difficult science of colours." If the importance of a work were to be measured by the amount of conscious labour expended in its production, Goethe's estimate of the 'Farbenlehre' would probably be correct. The observations and experiments there recorded astonish us by their variety and number. The amount of reading which he accomplished was obviously vast. He pursued the history of optics, not only along its main streams, but on to its remotest rills. He was animated by the zeal of an apostle, for he believed that a giant imposture was to be overthrown, and that he was the man to accomplish the holy work of destruction. He was also a lover of art, and held that the enunciation of the true principles of colour would, in relation to painting, be of lasting importance. Thus positively and negatively he was stimulated to bring all the strength he could command to bear upon this question. The greater part of the first volume is taken up with Goethe's own experiments, which are described in 920 paragraphs duly numbered. It is not a consecutive argument, but rather a series of jets of fact and logic emitted at various intervals. I picture the poet in that troublous war-time, walking up and down his Weimar garden, with his hands behind his back, pondering his subject, throwing his experiments and reflections into these terse paragraphs, and turning occasionally into his garden house to write them down. This first portion of the work embraces three parts, which deal respectively with: Physiological or Subjective Colours, with Physical or Prismatic Colours, and with Chemical Colours and Pigments. To these are added a fourth part, bearing the German title, "Allgemeine Ansichten nach innen"; a fifth part, entitled "Nachbarliche Verhältnisse," neighbouring relations; and a sixth part, entitled "Sinnlich-sittliche Wirkung der Farbe," sensuously-moral effect of colours. It is hardly necessary to remark that some of these titles, though doubtless pregnant with meaning to the poet himself, are not likely to commend themselves to the more exacting man of science.

The main divisions of Goethe's book are subdivided into short sections, bearing titles more or less shadowy from a scientific point of view: Origin of white; Origin of black; Excitement of colour; Heightening; Culmination; Balancing; Reversion; Fixation; Mixture real; Mixture apparent; Communication actual; Communication apparent. He describes the colours of minerals, plants, worms, insects, fishes, birds, mammals, and men. Hair on the surface of the human body he considers indicative rather of weakness than of strength. The disquisition is continued under the headings: How easily colour arises; How energetic colour may be; Heightening to red; Completeness of manifold phenomena; Agreement of complete phenomena; How easily colour disappears; How durable colour remains; Relation to philosophy; Relation to mathematics; Relation to physiology and

pathology; Relation to natural history; Relation to general physics; Relation to tones. Then follows a series of sections dealing with the primary colours and their mixtures. These sections relate less to science than to art. The writer treats, among other things, of: Æsthetic effects; Fear of the Theoretical; Grounds and Pigments; Allegorical, Symbolical, and Mystical use of colours. The headings alone indicate the enormous industry of the poet; showing at the same time an absence of that scientific definition which he stigmatized as "pedantry" in the case of Newton.

In connection with his subject, Goethe charged himself with all kinds of kindred knowledge. He refers to ocular spectra, quoting Boyle, Buffon, and Darwin; to the paralysis of the eye by light; to its extreme sensitiveness when it awakes in the morning; to irradiation—quoting Tycho Brahe on the comparative apparent size of the dark and the illuminated moon. He dwells upon the persistence of impressions upon the retina, and quotes various instances of abnormal duration. He possessed a full and exact knowledge of the phenomena of subjective colours, and described various modes of producing them. He copiously illustrates the production by red of subjective green, and by green of subjective red. Blue produces subjective yellow, and yellow subjective blue. He experimented upon shadows, coloured in contrast to surrounding light. The contrasting subjective colours he calls "geforderte Farben," colours "demanded" by the eye. Goethe gives the following striking illustration of these subjective effects. "I once," he said, "entered an inn towards evening, when a well-built maiden, with dazzlingly white face, black hair, and scarlet bodice and skirt came towards me. I looked at her sharply in the twilight, and when she moved away, saw upon the white wall opposite a black face with a bright halo round it, while the clothing of the perfectly distinct figure appeared of a beautiful sea-green." With the instinct of the poet, Goethe discerned in these antitheses an image of the general method of nature. Every action, he says, implies an opposite. Inhalation precedes expiration, and each systole has its corresponding diastole. Such is the eternal formula of life. Under the figure of systole and diastole the rhythm of nature is represented in other portions of his work.

Goethe handled the prism with great skill, and his experiments with it are numberless. He places white rectangles on a black ground, black rectangles on a white ground, and shifts their apparent positions by prismatic refraction. He makes similar experiments with coloured rectangles and discs. The shifted image is sometimes projected on a screen, the experiment being then "objective." It is sometimes looked at directly through the prism, the experiment being then "subjective." In the production of chromatic effects, he dwells upon the absolute necessity of *boundaries*—"Gränzen." The sky may be looked at and shifted by a prism without the production of colour; and if the white rectangle on a black ground be only made wide enough, the centre remains white after refraction, the

colours being confined to the edges. Goethe's earliest experiment, which led him so hastily to the conclusion that Newton's theory of colours was wrong, consisted in looking through a prism at the white wall of his own room. He expected to see the whole wall covered with colours, this being, he thought, implied in the theory of Newton. But to his astonishment it remained white, and only when he came to the boundary of a dark or a bright space did the colours reveal themselves. This question of "boundaries" is one of supreme importance to the author of the 'Farbenlehre'; the end and aim of his theory being to account for the coloured fringes produced at the edges of his refracted images.

Darkness, according to Goethe, had as much to do as light with the production of colour. Colour was really due to the commingling of both. Not only did his white rectangles upon a black ground yield the coloured fringes, but his black rectangles on a white ground did the same. The order of the colours seemed, however, different in the two cases. Let a visiting card, held in the hand between the eye and a window facing the bright firmament, be looked at through a prism, then supposing the image of the card to be shifted upwards by refraction, a red fringe is seen above and a blue one below. Let the back be turned to the window and the card so held that the light shall fall upon it; on being looked at through the prism, blue is seen above and red below. In the first case the fringes are due to the decomposition of the light adjacent to the edge of the card, which simply acts as an opaque body, and might have been actually black. In the second case the light decomposed is that coming from the surface of the card itself. The first experiment corresponds to that of Goethe with a black rectangle on a white ground; while the second experiment corresponds to Goethe's white rectangle on a black ground. Both these effects are immediately deducible from Newton's theory of colours. But this, though explained to him by physicists of great experience and reputation, Goethe could never be brought to see, and he continued to affirm to the end of his life that the results were utterly irreconcilable with the theory of Newton.

In his own explanations Goethe began at the wrong end, inverting the true order of thought, and trying to make the outcome of theory its foundation. Apart from theory, however, his observations are of great interest and variety. He looked to the zenith at midnight, and found before him the blackness of space, while in daylight he saw the blue firmament overhead; and he rightly adopted the conclusion that this colouring of the sky was due to the shining of the sun upon a turbid medium with darkness behind. He by no means understood the physical action of turbid media, but he made a great variety of experiments bearing upon this point. Water, for example, rendered turbid by varnish, soap, or milk, and having a black ground behind it, always appeared blue when shone upon by white light. When, instead of a black background, a bright one was placed behind, so that the light shone, not *on*, but *through* the turbid

liquid, the blue colour disappeared, and he had yellow in its place. Such experiments are capable of endless variation. To this class of effects belongs the painter's "chill." A cold bluish bloom, like that of a plum, is sometimes observed to cover the browns of a varnished picture. This is due to a want of optical continuity in the varnish. Instead of being a coherent layer it is broken up into particles of microscopic smallness, which virtually constitute a turbid medium and send blue light to the eye.

Goethe himself describes a most amusing illustration, or, to use his own language, "a wonderful phenomenon," due to the temporary action of a turbid medium on a picture. "A portrait of an esteemed theologian was painted several years ago by an artist specially skilled in the treatment of colours. The man stood forth in his dignity clad in a beautiful black velvet coat, which attracted the eyes and awakened the admiration of the beholder almost more than the face itself. Through the action of humidity and dust, however, the picture had lost much of its original splendour. It was therefore handed over to a painter to be cleaned, and newly varnished. The painter began by carefully passing a wet sponge over the picture. But he had scarcely thus removed the coarser dirt, when to his astonishment the black velvet suddenly changed into a light blue plush; the reverend gentleman acquiring thereby a very worldly, if, at the same time, an old-fashioned appearance. The painter would not trust himself to wash further. He could by no means see how a bright blue could underlie a dark black, still less that he could have so rapidly washed away a coating capable of converting a blue like that before him into the black of the original painting."

Goethe inspected the picture, saw the phenomenon, and explained it. To deepen the hue of the velvet coat the painter had covered it with a special varnish, which, by absorbing part of the water passed over it, was converted into a turbid medium, through which the black behind instantly appeared as blue. To the great joy of the painter, he found that a few hours continuance in a dry place restored the primitive black. By the evaporation of the moisture the optical continuity of the varnish (to which essential point Goethe does not refer) was re-established, after which it ceased to act as a turbid medium.

This question of turbid media took entire possession of the poet's mind. It was ever present to his observation. It was illustrated by the azure of noonday, and by the daffodil and crimson of the evening sky. The inimitable lines written at Ilmenau—

> "Ueber allen Gipfeln
> Ist Ruh',
> In allen Wipfeln
> Spürest Du
> Kaum einen Hauch"—

suggest a stillness of the atmosphere which would allow the columns

of fine smoke from the foresters' cottages to rise high into the air. He would thus have an opportunity of seeing the upper portion of the column projected against bright clouds, and the lower portion against dark pines, the brownish yellow of the one and the blue of the other being strikingly and at once revealed. He was able to produce artificially at will the colours which he had previously observed in nature. He noticed that when certain bodies were incorporated with glass this substance also played a double part, appearing blue by reflected and yellow by transmitted light.*

The action of turbid media was to Goethe the ultimate fact—the *Urphänomen*—of the world of colours. "We see on the one side Light and on the other side Darkness. We bring between both Turbidity, and from these opposites develop all colours." As long as Goethe remained in the region of fact his observations are of permanent value. But by the coercion of a powerful imagination he forced his turbid media into regions to which they did not belong, and sought to overthrow by their agency the irrefragable demonstrations of Newton. Newton's theory, as known by everybody, is that white light is composed of a multitude of differently refrangible rays, whose coalescence in certain proportions produces the impression of white. By prismatic analysis these rays are separated from each other, the colour of each ray being strictly determined by its refrangibility. The experiments of Newton, whereby he sought to establish this theory, had long appealed with overmastering evidence to every mind trained in the severities of physical investigation. But they did not thus appeal to Goethe. Accepting for the most part the experiments of Newton, he rejected with indignation the conclusions drawn from them, and turned into utter ridicule the notion that white light possessed the composite character ascribed to it. Many of the naturalists of his time supported him, while among philosophers Schelling and Hegel shouted in acclamation over the supposed defeat of Newton. The physicists, however, gave the poet no countenance. Goethe met their scorn with scorn, and under his lash these deniers of his theory, their Master included, paid the penalty of their arrogance.

How, then, did he lay down the lines of his own theory? How, out of such meagre elements as his yellow, and his blue, and his turbid medium, did he extract the amazing variety and richness of the Newtonian spectrum? Here we must walk circumspectly, for the intellectual atmosphere with which Goethe surrounds himself is by no means free from turbidity. In trying to account for his position, we must make ourselves acquainted with his salient facts, and endeavour to place our minds in sympathy with his mode of regarding them. He found that he could intensify the yellow of his transmitted light by making the turbidity of his medium stronger. A single

* Beautiful and instructive samples of such glass are to be seen in the Venice Glass Company's shop, No. 30, St. James's Street.

sheet of diaphanous parchment placed over a hole in his window-shutter appeared whitish. Two sheets appeared yellow, which by the addition of other sheets could be converted into red. It is quite true that by simply sending it through a medium charged with extremely minute particles we can extract from white light a ruby red. The red of the London sun, of which we have had such fine and frequent examples during the late winter, is a case to some extent in point. Goethe did not believe in Newton's differently refrangible rays. He refused to entertain the notion that the red light obtained by the employment of several sheets of parchment was different in quality from the yellow light obtained with two. The red, according to him, was a mere intensification—"Steigerung"—of the yellow. Colours in general consisted, according to Goethe, of light on its way to darkness, and the only difference between yellow and red consisted in the latter being nearer than the former to its final goal.

But how in the production of the spectrum do turbid media come into play? If they exist, where are they? The poet's answer to this question is subtle in the extreme. He wanders round the answer before he touches it, indulging in various considerations regarding penumbræ and double images, with the apparent aim of breaking down the repugnance to his logic which the mind of his reader is only too likely to entertain. If you place a white card near the surface of a piece of plate-glass, and look obliquely at the image of the card reflected from the two surfaces, you observe two images, which are hazy at the edges and more dense and defined where they overlap. These hazy edges Goethe pressed into his service as turbid media. He fancied that they associated themselves indissolubly with his refracted rectangles—that in every case the image of the rectangle was accompanied by a secondary hazy image, a little in advance of the principal one. At one edge, he contended, the advanced secondary image had black behind it, which was converted into blue; while at the other edge it had white behind it, and appeared yellow. When the refracted rectangle is made very narrow, the fringes approach each other and finally overlap. Blue thus mingles with yellow, and the green of the spectrum is the consequence. This, in a nutshell, is the theory of colours developed in the 'Farbenlehre.' Goethe obviously regarded the narrowing of the rectangle of the cylindrical beam, or of the slit of light passing through the prism, which, according to Newton, is the indispensable condition requisite for the production of a pure spectrum, as an impure and complicated mode of illustrating the phenomenon. The elementary fact is, according to Goethe, obtained when we operate with a wide rectangle the edges only of which are coloured, while the centre remains white. His experiments with the parchment had made him acquainted with the passage of yellow into red as he multiplied his layers; but how this passage occurs in the spectrum he does not explain. That, however, his hazy surfaces—his virtual turbid media—produced, in some way

or other, the observed passage and intensification, Goethe held as firmly, and enunciated as confidently, as if his analysis of the phenomena had been complete.

The fact is, that between double images and turbid media there is no kinship whatever. Turbidity is due to the diffusion, in a transparent medium, of minute particles having a refractive index different from that of the medium. But the act of reflection which produced the penumbral surfaces, whose aid Goethe invoked, did not charge them with such discrete particles. On various former occasions I have tried to set forth the principles on which the chromatic action of turbid media depends. When such media are to be seen blue, the light scattered by the diffused particles, and that only, ought to reach the eye. This feeble light may be compared to a faint whisper which is easily rendered inaudible by a louder noise. The scattered light of the particles is accordingly overpowered, when a stronger light comes, not from the particles, but from a bright surface behind them. Here the light reaches the eye, minus that scattered by the particles. It is therefore the complementary light, or yellow. Both effects are immediately deducible from the principles of the undulatory theory. As a stone in water throws back a larger fraction of a ripple than of a larger wave, so do the excessively minute particles which produce the turbidity scatter more copiously the small waves of the spectrum than the large ones. Light scattered by such particles will therefore always contain a preponderance of the waves which produce the sensation of blue. During its transmission through the turbid medium the white light is more and more robbed of its blue constituents, the transmitted light which reaches the eye being therefore complementary to blue.

Some of you are, no doubt, aware that it is possible to take matter in the gaseous condition, when its smallest parts are molecules, incapable of being either seen themselves or of scattering any sensible portion of light which impinges on them; that it is possible to shake these molecules asunder by special light-waves, so that their liberated constituents shall coalesce anew and form, not *molecules*, but *particles;* that it is possible to cause these particles to grow, from a size bordering on the atomic, to a size which enables them to copiously scatter light. Some of you are aware that in the early stages of their growth, when they are still beyond the grasp of the microscope, such particles, no matter what the substance may be of which they are composed, shed forth a pure firmamental blue; and that from them we can manufacture in the laboratory artificial skies which display all the phenomena, both of colour and polarization, of the real firmament.

With regard to the production of the green of the spectrum by the overlapping of yellow and blue, Goethe, like a multitude of others, confounded the mixture of blue and yellow lights with that of blue and yellow pigments. This was an error shared by the world at large. But in Goethe's own day, Wünsch of Leipzig, who is ridiculed

in the 'Farbenlehre,' had corrected the error, and proved the mixture of blue and yellow lights to produce white. Any doubt that might be entertained of Wünsch's experiments—and they are obviously the work of a careful and competent man—is entirely removed by the experiments of Helmholtz and others in our own day. Thus, to sum up, Goethe's theory, if such it may be called, proves incompetent to account even approximately for the Newtonian spectrum. He refers it to turbid media, but no such media come into play. He fails to account for the passage of yellow into red and of blue into violet; while his attempt to deduce the green of the spectrum from the mixture of yellow and blue, is contradicted by facts which were extant in his own time.

One hole Goethe did find in Newton's armour, through which he incessantly worried the Englishman with his lance. Newton had committed himself to the doctrine that refraction without colour was impossible. He therefore thought that the object-glasses of telescopes must for ever remain imperfect, achromatism and refraction being incompatible. The inference of Newton was proved by Dollond to be wrong.* With the same mean refraction, flint glass produces a longer and richer spectrum than crown glass. By diminishing the refracting angle of the flint-glass prism, its spectrum may be made equal in length to that of the crown glass. Causing two such prisms to refract in opposite directions, the colours may be neutralized, while a considerable residue of refraction continues in favour of the crown. Similar combinations are possible in the case of lenses; and hence, as Dollond showed, the possibility of producing a compound achromatic lens. Here, as elsewhere, Goethe proves himself master of the experimental conditions. It is the power of interpretation that he lacks. He flaunts this error regarding achromatism incessantly in the face of Newton and his followers. But the error, which was a real one, leaves Newton's theory of colours perfectly unimpaired.

Newton's account of his first experiment with the prism is for ever memorable. "To perform my late promise to you," he writes to Oldenburg, "I shall without further ceremony acquaint you, that in the year 1666 (at which time I applied myself to the grinding of optick-glasses of other figures than spherical) I procured me a triangular glass prism, to try therewith the celebrated phenomena of colours. And in order thereto, having darkened my chamber, and made a small hole in my window-shuts, to let in a convenient quantity of the sun's light, I placed my prism at its entrance, that it might be thereby refracted to the opposite wall. It was at first a very pleasing divertisement, to view the vivid and intense colours produced thereby; but after a while applying myself to consider them more circumspectly, I became surprised to see them in an oblong form, which according

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* Dollond was the son of a Huguenot. Up to 1752 he was a silk weaver at Spitalfields; he afterwards became an optician.

to the received laws of refractions, I expected should have been circular. They were terminated at the sides with straight lines, but at the ends the decay of light was so gradual, that it was difficult to determine justly what was their figure, yet they seemed semi-circular.

"Comparing the length of this coloured *spectrum* with its breadth, I found it about five times greater; a disproportion so extravagant, that it excited me to a more than ordinary curiosity of examining from whence it might proceed." This curiosity Newton gratified by instituting a series of experimental questions, the answers to which left no doubt upon his mind that the elongation of his spectrum was due to the fact "that *light* is not similar or homogeneal, but consists of *difform rays, some of which are more refrangible than others;* so that without any difference in their incidence on the same medium, some shall be more *refracted* than others; and therefore that, according to their *particular degrees of refrangibility*, they were transmitted through the prism to divers parts of the opposite wall. When," continues Newton, "I understood this, I left off my aforesaid glass works; for I saw that the perfection of telescopes was hitherto limited, not so much for want of glasses truly figured according to the prescriptions of optick authors, as because that *light* itself is an heterogeneous mixture of *differently refrangible rays;* so that were a glass so exactly figured as to collect any one sort of rays into one point, it could not collect those also into the same point, which, having the same incidence upon the same medium, are apt to suffer a different refraction."

Goethe harped on this string without cessation. "The Newtonian doctrine," he says, "was really dead the moment achromatism was discovered. Gifted men, our own Klügel for example, felt this, but expressed themselves in an undecided way. On the other hand, the school which had been long accustomed to support, patch up, and glue their intellects to the views of Newton, had surgeons at hand to embalm the corpse, so that even after death, in the manner of the Egyptians, it should preside at the banquets of the natural philosophers."

In dealing with the chromatic aberration of lenses, Goethe proves himself to be less heedful than usual as an experimenter. With the clearest perception of principles, Newton had taken two pieces of cardboard, the one coloured a deep red, the other a deep blue. Around those cards he had wound fine black silk, so that the silk formed a series of separate fine dark lines upon the two coloured surfaces. He might have drawn black lines over the red and blue, but the silk lines were finer than any that he could draw. Illuminating both surfaces, he placed a lens so as to cast an image of the surfaces upon a white screen. The result was, that when the dark lines were sharply defined upon the red, they were undefined upon the blue; and that when, by moving the screen, they were rendered distinct upon the blue, they were indistinct upon the red. A distance of an inch and a half separated the focus of red rays from the focus

of blue rays, the latter being nearer to the lens than the former. Goethe appears to have attempted a repetition of this experiment; at all events he flatly contradicts Newton, ascribing his result not to the testimony of his bodily eyes, but to that of the prejudiced eyes of his mind. Goethe always saw the dark lines best defined upon the brighter colour. It was to him purely a matter of contrast, and not of different refrangibility. He argues caustically, that Newton proves too much; for were he correct, not only would a dioptric telescope be impossible, but presented to our naked eyes, differently coloured objects must appear utterly confusing. Let a house, he says, be supposed to stand in full sunshine; let the roof-tiles be red, the walls yellow, with blue curtains behind the open windows, while a lady with a violet dress steps out of the door. Let us look at the whole from a point in front of the house. The tiles we will suppose appear distinct, but on turning to the lady we should find both the form and the folds of her dress undefined. We must move forwards to see her distinctly, and then the red tiles would appear nebulous. And so with regard to the other objects, we must move to and fro in order to see them clearly, if Newton's pretended second experiment were correct. Goethe seems to have forgotten that the human eye is not a rigid lens, and that it is able to adjust itself promptly and without difficulty to differences of distance enormously greater than that due to the different refrangibility of the differently coloured rays.

Newton's theory of colours, it may be remarked, is really less a "theory" than a direct presentation of facts. Given the accepted definition of refraction, it is a matter of fact, and not of theoretic inference, that white light is not "homogeneal" but composed of differently refrangible rays. The demonstration is ocular and complete. Having palpably decomposed the white light into its constituent colours, Newton recompounded these colours to white light. Both the analysis and the synthesis are matters of fact. The so-called "theory of light and colours" is in this respect very different from the corpuscular theory of light. Newton's explanation of colours stands where it is, whether we accept the corpuscular or the undulatory theory; and it stands because it is at bottom not a theory but a body of fact, to which theory must bow or disappear. Newton himself pointed out that his views of colours were entirely independent of his belief in the "corporiety" of light.

After refraction-colours Goethe turns to those produced by diffraction, and, as far as the phenomena are concerned, he deals very exhaustively with the colours of thin plates. He studies the colours of Newton's rings both by reflected and transmitted light. He states the conditions under which this class of colours is produced, and illustrates the conditions by special cases. He presses together flat surfaces of glass, observes the flaws in crystals and in ice, refers to the iridescences of oil on water, to those of soap-bubbles, and to the varying colours of tempered steel. He is always rich in facts. But when he comes to deal with physical theory, the poverty and con-

fusion of his otherwise transcendent mind become conspicuous. His turbid media entangle him everywhere, leading him captive and committing him to almost incredible delusions. The colours of tempered steel, he says, and kindred phenomena, may perhaps be *quite conveniently deduced* from the action of turbid media. Polished steel powerfully reflects light, and the colouring produced by heating may be regarded as a feeble turbidity, which, acted upon by the polished surface behind, produces a bright yellow. As the turbidity augments, this colour becomes dense, until finally it exhibits an intense ruby-red. Supposing this colour to reach its greatest proximity to darkness, the turbidity continuing to augment as before, we shall have behind the turbid medium a dark background, which appears first violet, then dark blue, and finally light blue, thus completing the cycle of the phenomena. The mind that could offer such an explanation as this must be qualitatively different from that of the natural philosopher.

The words "quite conveniently deduced," which I have italicized in the last paragraph, are also used by Goethe in another place. When the results of his experiments on prismatic colours had to be condensed into one commanding inference, he enunciated it thus:—"Und so lassen sich die Farben bei Gelegenheit der Refraction aus der Lehre von den trüben Mitteln gar bequem ableiten." This is the crown of his edifice, and it seems a feeble ending to so much preparation. Kingsley once suggested to Lewes that Goethe might have had a vague feeling that his conclusions were not sound, and that he felt the jealousy incident to imperfect conviction. The ring of conscious demonstration, as it is understood by the man of science, is hardly to be found in the words "gar bequem ableiten." They fall flaccid upon the ear in comparison with the mind-compelling Q.E.D. of Newton.

Throughout the first 350 pages of his work, wherein he develops and expounds his own theory, Goethe restrains himself with due dignity. Here and there, there is a rumble of discontent against Newton, but there is no sustained ill-temper or denunciation. After, however, having unfolded his own views, he comes to what he calls the "unmasking of the theory of Newton." Here Goethe deliberately forsakes the path of calm, objective research, and delivers himself over to the guidance of his emotions. He immediately accuses Newton of misusing, as an advocate, his method of exposition. He goes over the propositions in Newton's optics one by one, and makes even the individual words of the propositions the objects of criticism. He passes on to Newton's experimental proofs, invoking, as he does so, the complete attention of his readers, if they would be freed to all eternity from the slavery of a doctrine which has imposed upon the world for a hundred years. It might be thought that Goethe had given himself but little trouble to understand the theorems of Newton and the experiments on which they were based. But it would be unjust to charge the poet with any want of diligence in this respect.

He repeated Newton's experiments, and in almost every case obtained his results. But he complained of their incompleteness and lack of logical force. What appears to us as the very perfection of Newton's art, and absolutely essential to the purity of the experiments, was regarded by Goethe as needless complication and mere torturing of the light. He spared no pains in making himself master of Newton's data, but he lacked the power of penetrating either their particular significance, or of estimating the force and value of experimental evidence generally.

He will not, he says, shock his readers at the outset by the utterance of a paradox, but he cannot withhold the assertion that by experiment nothing can really be proved. Phenomena may be observed and classified; experiments may be accurately executed, and made thus to represent a certain circle of human knowledge; but deductions must be drawn by every man for himself. Opinions of things belong to the individual, and we know only too well that conviction does not depend upon insight, but upon will—that man can only assimilate that which is in accordance with his nature, and to which he can yield assent. In knowledge, as in action, says Goethe, prejudice decides all, and prejudice, as its name indicates, is judgment prior to investigation. It is an affirmation or a negation of what corresponds, or is opposed to our own nature. It is the cheerful activity of our living being in its pursuit of truth or of falsehood, as the case may be—of all, in short, with which we feel ourselves to be in harmony.

There can be no doubt that Goethe, in thus philosophizing, dipped his bucket into the well of profound self-knowledge. He was obviously stung to the quick by the neglect of the physicists. He had been the idol of the world, and accustomed as he was to the incense of praise, he felt sorely that any class of men should treat what he thought important with indifference or contempt. He had, it must be admitted, some ground for scepticism as to the rectitude of scientific judgments, seeing that his researches on morphology met at first no response, though they were afterwards lauded by scientific men. His anger against Newton incorporates itself in sharp and bitter sarcasm. Through the whole of Newton's experiments, he says, there runs a display of pedantic accuracy, but how the matter really stands, with Newton's gift of observation, and with his experimental aptitudes, every man possessing eyes and senses may make himself aware. It may, he says, be boldly asked, Where can the man be found, possessing the extraordinary gifts of Newton, who would suffer himself to be deluded by such a *hocus pocus* if he had not in the first instance wilfully deceived himself? Only those who know the strength of self-deception, and the extent to which it sometimes trenches on dishonesty, are in a condition to explain the conduct of Newton and of Newton's school. "To support his unnatural theory," he continues, "Newton heaps experiment on experiment, fiction upon fiction, seeking to dazzle where he cannot convince."

It may be that Goethe is correct in affirming that the will and prejudice of the individual are all-influential. We must, however, add the qualifying words, "as far as the individual is concerned." For in science there exists, apart from the individual, objective truth; and the fate of Goethe's own theory, though commended to us by so great a name, illustrates how, in the progress of humanity, the individual, if he err, is left stranded and forgotten—truth, independent of the individual, being more and more grafted on to that tree of knowledge which is the property of the human race.

The imagined ruin of Newton's theory did not satisfy Goethe's desire for completeness. He would explore the ground of Newton's error, and show how it was that one so highly gifted could employ his gifts for the enunciation and diffusion of such unmitigated nonsense. It was impossible to solve the riddle on purely intellectual grounds. Scientific enigmas, he says, are often only capable of ethical solution, and with this maxim in his mind he applies himself, in the second volume of the 'Farbenlehre,' to the examination of "Newton's Persönlichkeit." He seeks to connect him with, or rather to detach him from, the general character of the English nation—that sturdy and competent race, which prizes above all things the freedom of individual action. Newton was born in a storm-tossed time—none indeed more pregnant in the history of the world. He was a year old when Charles I. was beheaded, and he lived to see the first George upon the throne. The shock of parties was in his ears, changes of ministries, Parliaments, and armies were occurring before his eyes, while the throne itself, instead of passing on by inheritance, was taken possession of by a stranger. What, asks Goethe, are we to think of a man who could put aside the claims, seductions, and passions incident to such a time, for the purpose of tranquilly following out his bias as an investigator?

So singular a character arrests the poet's attention. He had laid down his theory of colours, he must add to it a theory of Newton. The great German is here at home, and Newton could probably no more have gone into these disquisitions regarding character, than Goethe could have developed the physical theories of Newton. He prefaces his sketch of his rival's character by reflections and considerations regarding character in general. Every living thing, down to the worm that wriggles when trod upon, has a character of its own. In this sense even the weak and cowardly have characters, for they will give up the honour and fame which most men prize highest, so that they may vegetate in safety and comfort. But the word character is usually applied to the case of an individual with great qualities, who pursues his object undeviatingly, and without permitting either difficulty or danger to deflect him from his course.

"Although here, as in other cases," says Goethe, "it is the exuberant (Ueberschwängliche) that impresses the imagination, it must not be imagined that this attribute has anything to do with moral feeling. The

main foundation of the moral law is a *good* will* which, in accordance with its own nature, is anxious only for the right. The main foundation of character is a *strong* will, without reference to right or wrong, good or bad, truth or error. It is that quality which every party prizes in its members. A good will cherishes freedom, it has reference to the inner man and to ethical aims. The strong will belongs to nature and has reference to the outer world—to action. And inasmuch as the strong will in this world is swayed and limited by the conditions of life, it may almost be assumed as certain that it is only by accident that the exercise of a strong will and of moral rectitude find themselves in harmony with each other." In determining Newton's position in the series of human characters, Goethe helps himself to images borrowed from the physical cohesion of matter. Thus, he says, we have strong, firm, compact, elastic, flexible, rigid or obstinate, and viscous characters. Newton's character he places under the head of rigid or obstinate, and his theory of colours Goethe pronounces to be a petrified aperçu.

Newton's assertion of his theory, and his unwavering adherence to it to the end of his life, Goethe ascribes straight off to moral obliquity on Newton's part. In the heat of our discussion, he says, we have even ascribed to him a certain dishonesty. Man, he says, is subject to error, but when errors form a series, which is followed pertinaciously, the erring individual becomes false to himself and to others. Nevertheless reason and conscience will not yield their rights. We may belie them, but they are not deceived. It is not too much to say that the more moral and rational a man is, the greater will be his tendency to lie when he falls into error, and the vaster will be that error when he makes up his mind to persist in it.

This is all intended to throw light upon Newton, but when Goethe passes from Newton himself to his followers, the small amount of reserve which he exhibited when dealing with the master entirely disappears. He mocks their blunders as having not even the merit of originality. He heaps scorn on Newton's imitators. The expression of even a truth, he says, loses grace in repetition, while the repetition of a blunder is impertinent and ridiculous. To liberate oneself from an error is difficult, sometimes indeed impossible for even the strongest and most gifted minds. But to take up the error of another, and persist in it with stiffnecked obstinacy, is a proof of poor qualities. The obstinacy of a man of originality when he errs may make us angry, but the stupidity of the copyist irritates and renders us miserable. And if in our strife with Newton we have sometimes passed the bounds of moderation, the whole blame is to be laid upon the school of which Newton was the head, whose incompetence is proportional to its arrogance, whose laziness is proportional to its self-sufficiency, and whose virulence and love of persecution hold each other in perfect equilibrium.

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* I have rendered Goethe's "gute Wille" by *good* will; his "Wollen," which he contrasts with "Wille," I have rendered by *strong* will.

There is a great deal more invective of this kind, but you will probably, and not without sadness, consider this enough. Invective may be a sharp weapon, but over-use blunts its edge. Even when the denunciation is just and true, it is an error of art to indulge in it too long. We not only incur the risk of becoming vapid, but of actually inverting the force of reprobation which we seek to arouse, and of bringing it back by recoil upon ourselves. At suitable intervals, separated from each other by periods of dignified reserve, invective may become a real power of the tongue or pen. But indulged in constantly it degenerates into scolding, and then, instead of being regarded as a proof of strength, it is accepted, even in the case of a Goethe, as an evidence of weakness and lack of self-control.

If it were possible to receive upon a mirror Goethe's ethical image of Newton and to reflect it back upon its author, then, as regards vehement persistence in wrong thinking, the image would accurately coincide with Goethe himself. It may be said that we can only solve the character of another by the observation of our own. This is true, but in the portraiture of character we are not at liberty to mix together subject and object as Goethe mixed himself with Newton. So much for the purely ethical picture. On the scientific side something more is to be said. I do not know whether psychologists have sufficiently taken into account that as regards intellectual endowment, vast wealth may co-exist with extreme poverty. I do not mean to give utterance here to the truism that the field of culture is so large that the most gifted can master only a portion of it. This would be the case supposing the individual at starting to be, as regards natural capacity and potentiality, rounded like a sphere. Something more radical is here referred to. There are individuals who at starting are not spheres, but hemispheres ; or, at least, spheres with a segment sliced away—full orbed on one side, but flat upon the other. Such incompleteness of the mental organization no education can repair. Now the field of science is sufficiently large, and its studies sufficiently varied, to bring to light in the same individual antitheses of endowment like that here indicated.

So far as science is a work of ordering and classification, so far as it consists in the discovery of analogies and resemblances which escape the common eye—of the fundamental identity which often exists among apparently diverse and unrelated things—so far, in short, as it is observational, descriptive, and imaginative, Goethe, had he chosen to make his culture exclusively scientific, might have been without a master, perhaps even without a rival. The instincts and capacities of the poet lend themselves freely to the natural history sciences. But when we have to deal with stringently physical and mechanical conceptions, such instincts and capacities are out of place. It was in this region of mechanical conceptions that Goethe failed. It was on this side that his sphere of capacity was sliced away. He probably was not the only great man who possessed a spirit thus antithetically mixed. Aristotle himself was a mighty

classifier, but not a stringent physical reasoner. And had Newton attempted to produce a Faust, the poverty of his intellect on the poetic and dramatic side might have been rendered equally manifest. But here, if not always, Newton abstained from attempting that for which he had no capacity, while the exuberance of Goethe's nature caused him to undertake a task for which he had neither ordination nor vocation, and in the attempted execution of which his deficiencies became revealed.

One task among many—one defeat amid a hundred triumphs. But any recognition on my part of Goethe's achievements in other realms of intellectual action would, I fear, be regarded as impertinent. You remember the story of the first Napoleon when the Austrian plenipotentiary, in arranging a treaty of peace, began by formally recognizing the French Republic. "Efface that," said the First Consul; "the French Republic is like the sun; he is blind who fails to recognize it." And were I to speak of recognizing Goethe's merits, my effacement would be equally well deserved. "Goethe's life," says Carlyle, "if we examine it, is well represented in that emblem of a solar day. Beautifully rose our summer sun, gorgeous in the red, fervid east, scattering the spectres and sickly damps, of both of which there were enough to scatter; strong, benignant, in his noonday clearness, walking triumphant through the upper realms—and now mark also how he sets! 'So stirbt ein Held;' so dies a hero!"

Two grander illustrations of the aphorism "To err is human" can hardly be pointed out in history than Newton and Goethe. For Newton went astray not only as regards the question of achromatism, but also as regards vastly larger questions touching the nature of light. But though as errors they fall into the same category, the mistake of Newton was qualitatively different from that of Goethe. Newton erred in adopting a wrong mechanical conception in his theory of light, but in doing so he never for a moment quitted the ground of strict scientific method. Goethe erred in seeking to engraft in his 'Farbenlehre' methods altogether foreign to physics on to the treatment of a purely physical theme.

We frequently hear protests made against the cold mechanical mode of dealing with æsthetic phenomena employed by scientific men. The dissection by Newton of the light to which the world owes all its visible beauty and splendour seemed to Goethe a desecration. We find, even in our own day, the endeavour of Helmholtz to arrive at the principles of harmony and discord in music resented as an intrusion of the scientific intellect into a region which ought to be sacred to the human heart. But all this opposition and antagonism has for its essential cause the incompleteness of those with whom it originates. The feelings and aims with which Newton and Goethe respectively approached Nature were radically different, but they had an equal warrant in the constitution of man. As regards our tastes and tendencies, our pleasures and pains, physical and mental, our action and passion, our sorrows, sympathies, and joys, we are the

heirs of all the ages that preceded us ; and of the human nature thus handed down poetry is an element just as much as science. The emotions of man are older than his understanding, and the poet who brightens, purifies, and exalts these emotions may claim a position in the world at least as high and as well assured as that of the man of science. They minister to different but to equally permanent needs of human nature; and the incompleteness of which I complain consists in the endeavour on the part of either to exclude the other. There is no fear that the man of science can ever destroy the glory of the lilies of the field; there is no hope that the poet can ever successfully contend against our right to examine, in accordance with scientific method, the agent to which the lily owes its glory. There is no necessary encroachment of the one field upon the other. Nature embraces them both, and man, when he is complete, will exhibit as large a toleration.

[J. T.]

GENERAL MONTHLY MEETING,

Monday, April 5, 1880.

George Busk, Esq. F.R.S. Treasurer and Vice-President, in the Chair.

Professor James Dewar, M.A. F.R.S.
Walter Hills, Esq.
Rev. William Thomas Houldsworth,
Mrs. W. T. Houldsworth,
Mrs. William Huggins,
George Kelly, Esq. F.R.M.S.
Cecil Paget, Esq.
Capt. Matthew Henry Purcell, R.E.
Stephen A. Ralli, Esq.
Peter Wyatt Squire, Esq. F.L.S.
Capt. Henry J. L. Turnbull, R.A.

were elected Members of the Royal Institution.

The Thanks of the Members were given to Mr. Warren De La Rue, the Secretary, for his present of Bertin's Decomposition Apparatus (for Electrolysis).

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

Governor General of India:—
Geological Survey of India:
Records, Vol. XIII. Part 1. 8vo. 1880.
New Zealand Government—Statistics for 1878. fol. 1879.
Accademia dei Lincei, Reale, Roma—Atti, Serie Terza: Transunti: Tome IV. Fasc. 3. 4to. 1879.
Actuaries, Institute of—Journal, No. 118. 8vo. 1879.
Astronomical Society, Royal—Monthly Notices, Vol. XL. No. 4. 8vo. 1880.
Memoirs, Vol. XLI. 4to. 1879.
Bourchier, The Lady (the Editor)—Selections from the Letters of Sir Henry Codrington. 16to. 1880.
British Architects, Royal Institute of—1879–80: Proceedings, No. 10.
Transactions, No. 6. 4to.
Chemical Society—Journal for March, 1880. 8vo.
Dax: Société de Borda—Bulletins: 2e Série: Quinzième Année: Trimestre 1. 8vo. Dax, 1878.

Editors—American Journal of Science for March, 1880. 8vo.
Analyst for March, 1880. 8vo.
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WEEKLY EVENING MEETING,

Friday, April 9, 1880.

George Busk, Esq. F.R.S. Treasurer and Vice-President, in the Chair.

Professor T. H. Huxley, LL.D. F.R.S. &c.

The Coming of Age of 'The Origin of Species.'

[Reprinted from 'Nature,' May 6, 1880.]

Many of you will be familiar with the aspect of this small green-covered book. It is a copy of the first edition of the 'Origin of Species,' and bears the date of its production—the 1st of October, 1859. Only a few months, therefore, are needed to complete the full tale of twenty-one years since its birthday.

Those whose memories carry them back to this time will remember that the infant was remarkably lively, and that a great number of excellent persons mistook its manifestations of a vigorous individuality for mere naughtiness; in fact there was a very pretty turmoil about its cradle. My recollections of the period are particularly vivid; for, having conceived a tender affection for a child of what appeared to me to be such remarkable promise, I acted for some time in the capacity of a sort of under-nurse, and thus came in for my share of the storms which threatened the very life of the young creature. For some years it was undoubtedly warm work; but considering how exceedingly unpleasant the apparition of the new-comer must have been to those who did not fall in love with him at first sight, I think it is to the credit of our age that the war was not fiercer, and that the more bitter and unscrupulous forms of opposition died away as soon as they did.

I speak of this period as of something past and gone, possessing merely an historical, I had almost said an antiquarian interest. For, during the second decade of the existence of the 'Origin of Species,' opposition, though by no means dead, assumed a different aspect. On the part of all those who had any reason to respect themselves, it assumed a thoroughly respectful character. By this time the dullest began to perceive that the child was not likely to perish of any congenital weakness or infantile disorder, but was growing into a

stalwart personage, upon whom mere goody scoldings and threatenings with the birch-rod were quite thrown away.

In fact, those who have watched the progress of science within the last ten years will bear me out to the full when I assert that there is no field of biological inquiry in which the influence of the 'Origin of Species' is not traceable; the foremost men of science in every country are either avowed champions of its leading doctrines, or at any rate abstain from opposing them; a host of young and ardent investigators seek for and find inspiration and guidance in Mr. Darwin's great work; and the general doctrine of Evolution, to one side of which it gives expression, finds in the phenomena of biology a firm base of operations whence it may conduct its conquest of the whole realm of nature.

History warns us, however, that it is the customary fate of new truths to begin as heresies and to end as superstitions; and, as matters now stand, it is hardly rash to anticipate that, in another twenty years, the new generation, educated under the influences of the present day, will be in danger of accepting the main doctrines of the 'Origin of Species' with as little reflection, and it may be with as little justification, as so many of our contemporaries, twenty years ago, rejected them.

Against any such a consummation let us all devoutly pray; for the scientific spirit is of more value than its products, and irrationally-held truths may be more harmful than reasoned errors. Now the essence of the scientific spirit is criticism. It tells us that to whatever doctrine claims our assent we should reply, Take it if you can compel it. The struggle for existence holds as much in the intellectual as in the physical world. A theory is a species of thinking, and its right to exist is coextensive with its power of resisting extinction by its rivals.

From this point of view it appears to me that it would be but a poor way of celebrating the Coming of Age of the 'Origin of Species' were I merely to dwell upon the facts, undoubted and remarkable as they are, of its far-reaching influence and of the great following of ardent disciples who are occupied in spreading and developing its doctrines. Mere insanities and inanities have before now swollen to portentous size in the course of twenty years. Let us rather ask this prodigious change in opinion to justify itself; let us inquire whether anything has happened since 1859 which will explain, on rational grounds, why so many are worshipping that which they burned, and burning that which they worshipped. It is only in this way that we shall acquire the means of judging whether the movement we have witnessed is a mere eddy of fashion, or truly one with the irreversible current of intellectual progress, and, like it, safe from retrogressive reaction.

Every belief is the product of two factors: the first is the state of the mind to which the evidence in favour of that belief is presented; and the second is the logical cogency of the evidence

itself. In both these respects the history of biological science during the last twenty years appears to me to afford an ample explanation of the change which has taken place; and a brief consideration of the salient events of that history will enable us to understand why, if the 'Origin of Species' appeared now, it would meet with a very different reception from that which greeted it in 1859.

One-and-twenty years ago, in spite of the work commenced by Hutton and continued with rare skill and patience by Lyell, the dominant view of the past history of the earth was catastrophic. Great and sudden physical revolutions, wholesale creations and extinctions of living beings, were the ordinary machinery of the geological epic brought into fashion by the misapplied genius of Cuvier. It was gravely maintained and taught that the end of every geological epoch was signalized by a cataclysm, by which every living being on the globe was swept away, to be replaced by a brand-new creation when the world returned to quiescence. A scheme of nature which appeared to be modelled on the likeness of a succession of rubbers of whist, at the end of each of which the players upset the table and called for a new pack, did not seem to shock anybody.

I may be wrong, but I doubt if at the present time there is a single responsible representative of these opinions left. The progress of scientific geology has elevated the fundamental principle of uniformitarianism, that the explanation of the past is to be sought in the study of the present, into the position of an axiom; and the wild speculations of the catastrophists, to which we all listened with respect a quarter of a century ago, would hardly find a single patient hearer at the present day. No physical geologist now dreams of seeking outside the range of known natural causes for the explanation of anything that happened millions of years ago, any more than he would be guilty of the like absurdity in regard to current events.

The effect of this change of opinion upon biological speculation is obvious. For, if there have been no periodical general physical catastrophes, what brought about the assumed general extinctions and re-creations of life which are the corresponding biological catastrophes? And if no such interruptions of the ordinary course of nature have taken place in the organic, any more than in the inorganic, world, what alternative is there to the admission of evolution?

The doctrine of evolution in biology is the necessary result of the logical application of the principles of uniformitarianism to the phenomena of life. Darwin is the natural successor of Hutton and Lyell, and the 'Origin of Species' the logical sequence of the 'Principles of Geology.'

The fundamental doctrine of the 'Origin of Species,' as of all forms of the theory of evolution applied to biology, is "that the

innumerable species, genera, and families of organic beings with which the world is peopled have all descended, each within its own class or group, from common parents, and have all been modified in the course of descent."*

And, in view of the facts of geology, it follows that all living animals and plants "are the lineal descendants of those which lived long before the Silurian epoch." †

It is an obvious consequence of this theory of descent with modification, as it is sometimes called, that all plants and animals, however different they may now be, must, at one time or other, have been connected by direct or indirect intermediate gradations, and that the appearance of isolation presented by various groups of organic beings must be unreal.

No part of Mr. Darwin's work ran more directly counter to the prepossessions of naturalists twenty years ago than this. And such prepossessions were very excusable, for there was undoubtedly a great deal to be said at that time in favour of the fixity of species and of the existence of great breaks, which there was no obvious or probable means of filling up, between various groups of organic beings.

For various reasons, scientific and unscientific, much had been made of the hiatus between man and the rest of the higher mammalia, and it is no wonder that issue was first joined on this part of the controversy. I have no wish to revive past and happily forgotten controversies; but I must state the simple fact that the distinctions in cerebral and other characters, which were so hotly affirmed to separate man from all other animals in 1860, have all been demonstrated to be non-existent, and that the contrary doctrine is now universally accepted and taught.

But there were other cases in which the wide structural gaps asserted to exist between one group of animals and another were by no means fictitious; and, when such structural breaks were real, Mr. Darwin could account for them only by supposing that the intermediate forms which once existed had become extinct. In a remarkable passage he says:—

"We may thus account even for the distinctness of whole classes from each other—for instance, of birds from all other vertebrate animals—by the belief that many animal forms of life have been utterly lost, through which the early progenitors of birds were formerly connected with the early progenitors of the other vertebrate classes." ‡

Adverse criticism made merry over such suggestions as these. Of course it was easy to get out of the difficulty by supposing extinction; but where was the slightest evidence that such intermediate

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* 'Origin of Species,' ed. 1, p. 457.
† Ibid. p. 458.
‡ Ibid. p. 431.

forms between birds and reptiles as the hypothesis required ever existed? And then probably followed a tirade upon this terrible forsaking of the paths of "Baconian induction."

But the progress of knowledge has justified Mr. Darwin to an extent which could hardly have been anticipated. In 1862, the specimen of *Archæopteryx*, which until the last two or three years has remained unique, was discovered; and it is an animal which, in its feathers and the greater part of its organization, is a veritable bird, while in other parts it is as distinctly reptilian.

In 1868, I had the honour of bringing under your notice in this theatre the results of investigations made, up to that time, into the anatomical characters of certain ancient reptiles, which showed the nature of the modifications in virtue of which the type of the quadrupedal reptile passed into that of a bipedal bird; and abundant confirmatory evidence of the justice of the conclusions which I then laid before you has since come to light.

In 1875, the discovery of the toothed birds of the cretaceous formation in North America by Professor Marsh completed the series of transitional forms between birds and reptiles, and removed Mr. Darwin's proposition that "many animal forms of life have been utterly lost, through which the early progenitors of birds were formerly connected with the early progenitors of the other vertebrate classes," from the region of hypothesis to that of demonstrable fact.

In 1859, there appeared to be a very sharp and clear hiatus between vertebrated and invertebrated animals, not only in their structure, but, what was more important, in their development. I do not think that we even yet know the precise links of connection between the two; but the investigations of Kowalewsky and others upon the development of *Amphioxus* and of the *Tunicata* prove beyond a doubt that the differences which were supposed to constitute a barrier between the two are non-existent. There is no longer any difficulty in understanding how the vertebrate type may have arisen from the invertebrate, though the full proof of the manner in which the transition was actually effected may still be lacking.

Again, in 1859, there appeared to be a no less sharp separation between the two great groups of flowering and flowerless plants. It is only subsequently that the series of remarkable investigations inaugurated by Hofmeister has brought to light the extraordinary and altogether unexpected modifications of the reproductive apparatus in the *Lycopodiaceæ*, the *Rhizocarpeæ*, and the *Gymnospermeæ*, by which the ferns and the mosses are gradually connected with the Phanerogamic division of the vegetable world.

So, again, it is only since 1859 that we have acquired that wealth of knowledge of the lowest forms of life which demonstrates the futility of any attempt to separate the lowest plants from the lowest animals, and shows that the two kingdoms of living nature have a common borderland which belongs to both or to neither.

Thus it will be observed that the whole tendency of biological

investigation since 1859 has been in the direction of removing the difficulties which the apparent breaks in the series created at that time; and the recognition of gradation is the first step towards the acceptance of evolution.

As another great factor in bringing about the change of opinion which has taken place among naturalists, I count the astonishing progress which has been made in the study of embryology. Twenty years ago, not only were we devoid of any accurate knowledge of the mode of development of many groups of animals and plants, but the methods of investigation were rude and imperfect. At the present time there is no important group of organic beings the development of which has not been carefully studied, and the modern methods of hardening and section-making enable the embryologist to determine the nature of the process in each case with a degree of minuteness and accuracy which is truly astonishing to those whose memories carry them back to the beginnings of modern histology. And the results of these embryological investigations are in complete harmony with the requirements of the doctrine of evolution. The first beginnings of all the higher forms of animal life are similar, and however diverse their adult conditions, they start from a common foundation. Moreover, the process of development of the animal or the plant from its primary egg or germ is a true process of evolution—a progress from almost formless to more or less highly organized matter, in virtue of the properties inherent in that matter.

To those who are familiar with the process of development, all *à priori* objections to the doctrine of biological evolution appear childish. Any one who has watched the gradual formation of a complicated animal from the protoplasmic mass which constitutes the essential element of a frog's or a hen's egg has had under his eyes sufficient evidence that a similar evolution of the whole animal world from the like foundation is, at any rate, possible.

Yet another product of investigation has largely contributed to the removal of the objections to the doctrine of evolution current in 1859. It is the proof afforded by successive discoveries that Mr. Darwin did not over-estimate the imperfection of the geological record. No more striking illustration of this is needed than a comparison of our knowledge of the mammalian fauna of the Tertiary epoch in 1859 with its present condition. M. Gaudry's researches on the fossils of Pikermi were published in 1868, those of Messrs. Leidy, Marsh, and Cope on the fossils of the Western Territories of America have appeared almost wholly since 1870, those of M. Filhol on the phosphorites of Quercy in 1878. The general effect of these investigations has been to introduce to us a multitude of extinct animals, the existence of which was previously hardly suspected; just as if zoologists were to become acquainted with a country, hitherto unknown, as rich in novel forms of life as Brazil or South Africa once were to Europeans. Indeed the fossil fauna of the Western Territories of America bids fair to exceed in interest and importance

all other known Tertiary deposits put together; and yet, with the exception of the case of the American tertiaries, these investigations have extended over very limited areas, and at Pikermi were confined to an extremely small space.

Such appear to me to be the chief events in the history of the progress of knowledge during the last twenty years, which account for the changed feeling with which the doctrine of evolution is at present regarded by those who have followed the advance of biological science, in respect of those problems which bear indirectly upon that doctrine.

But all this remains mere secondary evidence. It may remove dissent, but it does not compel assent. Primary and direct evidence in favour of evolution can be furnished only by palæontology. The geological record, so soon as it approaches completeness, must, when properly questioned, yield either an affirmative or a negative answer: if evolution has taken place, there will its mark be left; if it has not taken place, there will lie its refutation.

What was the state of matters in 1859? Let us hear Mr. Darwin, who may be trusted always to state the case against himself as strongly as possible.

"On this doctrine of the extermination of an infinitude of connecting links between the living and extinct inhabitants of the world, and at each successive period between the extinct and still older species, why is not every geological formation charged with such links? Why does not every collection of fossil remains afford plain evidence of the gradation and mutation of the forms of life? We meet with no such evidence, and this is the most obvious and plausible of the many objections which may be urged against my theory." *

Nothing could have been more useful to the opposition than this characteristically candid avowal, twisted as it immediately was into an admission that the writer's views were contradicted by the facts of palæontology. But, in fact, Mr. Darwin made no such admission. What he says in effect is, not that palæontological evidence is against him, but that it is not distinctly in his favour; and without attempting to attenuate the fact, he accounts for it by the scantiness and the imperfection of that evidence.

What is the state of the case now, when, as we have seen, the amount of our knowledge respecting the mammalia of the Tertiary epoch is increased fifty-fold, and in some directions even approaches completeness?

Simply this, that if the doctrine of evolution had not existed palæontologists must have invented it, so irresistibly is it forced upon the mind by the study of the remains of the Tertiary mammalia which have been brought to light since 1859.

Among the fossils of Pikermi, Gaudry found the successive stages

* 'Origin of Species,' ed. 1, p. 463.

by which the ancient civets passed into the more modern hyænas; through the Tertiary deposits of Western America, Marsh tracked the successive forms by which the ancient stock of the horse has passed into its present form; and innumerable less complete indications of the mode of evolution of other groups of the higher mammalia have been obtained.

In the remarkable memoir on the phosphorites of Quercy, to which I have referred, M. Filhol describes no fewer than seventeen varieties of the genus *Cynodictis*, which fill up all the interval between the viverine animals and the bear-like dog *Amphicyon;* nor do I know any solid ground of objection to the supposition that in this *Cynodictis-Amphicyon* group we have the stock whence all the Viveridæ, Felidæ, Hyænidæ, Canidæ, and perhaps the Procyonidæ and Ursidæ, of the present fauna have been evolved. On the contrary, there is a great deal to be said in favour.

In the course of summing up his results, M. Filhol observes *:—

"During the epoch of the phosphorites, great changes took place in animal forms, and almost the same types as those which now exist became defined from one another.

"Under the influence of natural conditions of which we have no exact knowledge, though traces of them are discoverable, species have been modified in a thousand ways: races have arisen which, becoming fixed, have thus produced a corresponding number of secondary species."

In 1859, language of which this is an unintentional paraphrase, occurring in the 'Origin of Species,' was scouted as wild speculation; at present, it is a sober statement of the conclusions to which an acute and critically-minded investigator is led by large and patient study of the facts of palæontology. I venture to repeat what I have said before, that, so far as the animal world is concerned, evolution is no longer a speculation, but a statement of historical fact. It takes place alongside of those accepted truths which must be taken into account by philosophers of all schools.

Thus when, on the first day of October next, the 'Origin of Species' comes of age, the promise of its youth will be amply fulfilled; and we shall be prepared to congratulate the venerated author of the book, not only that the greatness of his achievement and its enduring influence upon the progress of knowledge have won him a place beside our Harvey; but, still more, that, like Harvey, he has lived long enough to outlast detraction and opposition, and to see the stone that the builders rejected become the head-stone of the corner.

[T. H. H.]

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* This passage was omitted in the delivery of the lecture.

WEEKLY EVENING MEETING,

Friday, April 16, 1880.

Sir W. Frederick Pollock, Bart., M.A., Vice-President, in the Chair.

M. Ernest Renan,
Membre de l'Académie Française, &c.

Marc-Aurèle.

Mesdames et Messieurs,

J'ai accepté, avec grande joie, de venir échanger quelques idées avec vous, en cet Institut illustre, voué aux plus hautes recherches de la science et de la vraie philosophie. Cette île, où j'ai tant d'amis et que je viens de visiter si tardivement, j'y rêvais dès mon enfance. Je suis Breton de France ; je voyais dans nos vieux livres l'Angleterre toujours appelée l'île des saints ; et, en effet, tous nos saints de la Bretagne armoricaine, ces saints d'une orthodoxie douteuse et qui, s'ils ressuscitaient, s'entendraient mieux avec nous qu'avec les jésuites, venaient de l'île de Bretagne. On me montrait dans leur chapelle l'auge de pierre en laquelle ils avaient passé la mer. De toutes les races, la race bretonne est celle qui a toujours pris la religion le plus au sérieux. Même quand le progrès de la réflexion nous a montré que quelques articles sont à modifier dans la liste des choses que nous avions autrefois tenues pour certaines, nous ne rompons jamais avec le symbole sous lequel nous avons d'abord goûté l'idéal. Car la foi ne réside pas, pour nous, en d'obscures propositions métaphysiques, elle est dans les affirmations du cœur. J'ai donc choisi pour m'entretenir avec vous, non quelqu'une de ces subtilités qui divisent, mais un de ces sujets chers à l'âme, qui rapprochent et réunissent. Je vous parlerai de ce livre tout resplendissant de l'esprit divin, de ce manuel de la vie résignée que nous a laissé le plus pieux des hommes, le césar Marc-Aurèle-Antonin. C'est la gloire des souverains que le plus irréprochable modèle de vertu se soit trouvé dans leurs rangs, et que les plus belles leçons de patience et de détachement soient venues d'une condition qu'on suppose volontiers livrée à toutes les séductions du plaisir et de la vanité.

I.

L'hérédité de la sagesse sur le trône est chose toujours rare ; je n'en vois dans l'histoire que deux exemples éclatants : dans l'Inde, la succession de ces trois empereurs mongols, Baber, Humaïoun et Akbar ; à Rome, à la tête du plus vaste empire qui fût jamais, les deux

règnes admirables d'Antonin-le-Pieux et de Marc-Aurèle. De ces deux derniers, Antonin fut, selon moi, le plus grand. Sa bonté ne lui fit pas commettre de fautes ; il ne fut pas tourmenté du mal intérieur qui rongea sans relâche le cœur de son fils adoptif. Ce mal étrange, cette étude inquiète de soi-même, ce démon du scrupule, cette fièvre de perfection sont des signes d'une nature moins forte que distinguée. Comme les plus belles pensées sont celles qu'on n'écrit pas, Antonin eut encore, à cet égard, une supériorité sur Marc-Aurèle ; mais ajoutons que nous ignorerions Antonin, si Marc-Aurèle ne nous avait transmis de son père adoptif ce portrait exquis, où il semble s'être appliqué, par humilité, à peindre l'image d'un homme encore meilleur que lui-même.

C'est lui aussi qui nous a tracé, dans le premier livre de ses *Pensées*, cet arrière-plan admirable, où se meuvent dans une lumière toute céleste les nobles et pures figures de son père, de sa mère, de son aïeul, de ses maîtres. Grâce à Marc-Aurèle, nous pouvons comprendre ce que ces vieilles familles romaines, qui avaient vu le règne des mauvais empereurs, gardaient encore d'honnêteté, de dignité, de droiture, d'esprit civil, et, si j'ose le dire, républicain. On y vivait dans l'admiration de Caton, de Brutus, de Thraséas et des grands stoïciens dont l'âme n'avait pas plié sous la tyrannie. Le règne de Domitien y était abhorré. Les sages qui l'avaient traversé sans fléchir y étaient honorés comme des héros. L'avénement des Antonins ne fut que l'arrivée au pouvoir de la société des sages dont Tacite nous a transmis les justes colères, société de sages formée par la ligue de tous ceux qu'avait révoltés le despotisme des premiers Césars.

Le salutaire principe de l'adoption avait fait de la cour impériale, au deuxième siècle, une vraie pépinière de vertu. Le noble et habile Nerva, en posant ce principe, assura le bonheur du genre humain pendant près de cent ans, et donna au monde le plus beau siècle de progrès dont la mémoire ait été conservée. La souveraineté ainsi possédée en commun par un groupe d'hommes d'élite, lesquels se la léguaient ou se la partageaient selon les besoins du moment, perdit une partie de cet attrait qui la rend si dangereuse. On arriva au trône sans l'avoir brigué, mais aussi sans le devoir à sa naissance ni à une sorte de droit divin ; on y arriva désabusé, ennuyé des hommes, préparé de longue main. L'empire fut un fardeau civil, qu'on accepta à son heure, sans que nul songeât à avancer cette heure. Marc-Aurèle y fut désigné si jeune que l'idée de régner n'eût guère chez lui de commencement et n'exerça pas sur son esprit un moment de séduction. A huit ans, quand il était déjà *præsul* des prêtres Saliens, Adrien remarqua ce doux enfant triste, et l'aima pour son bon naturel, sa docilité, son incapacité de mentir. A dix-huit ans, l'empire lui était assuré. Il l'attendit patiemment durant vingt-deux années. Le soir où Antonin se sentant mourir, après avoir donné pour mot d'ordre au tribun de service, *Æquanimitas*, fit porter dans la chambre de son fils adoptif la statue d'or de la Fortune, qui devait toujours se trouver dans l'appartement de l'empereur, il n'y eut pour celui-ci ni

surprise ni joie. Il était, depuis longtemps, blasé sur toutes les joies sans les avoir goûtées; il en avait vu, par la profondeur de sa philosophie, l'absolue vanité.

Le grand inconvénient de la vie pratique et ce qui la rend insupportable à l'homme supérieur, c'est que, si l'on y transporte les principes de l'idéal, les qualités deviennent des défauts, si bien que, fort souvent, l'homme accompli y réussit moins bien que celui qui a pour mobiles l'égoïsme ou la routine vulgaire. Trois ou quatre fois, la vertu de Marc-Aurèle faillit le perdre. Elle lui fit faire une première faute en lui persuadant d'associer à l'empire Lucius-Vérus, envers qui il n'avait aucune obligation. Vérus était un homme frivole et sans valeur. Il fallut des prodiges de bonté et de délicatesse pour l'empêcher de faire des folies désastreuses. Le sage empereur, sérieux et appliqué, traînait avec lui dans sa litière le sot collègue qu'il s'était donné. Il le prit toujours obstinément au sérieux; il ne se révolta pas une fois contre cet assommant compagnonnage. Comme les gens qui ont été très bien élevés, Marc-Aurèle se gênait sans cesse; ses façons venaient d'un parti-pris général de tenue et de dignité. Les âmes de cette sorte, soit pour ne pas faire de peine aux autres, soit par respect pour la nature humaine, ne se résignent pas à avouer qu'elles voient le mal. Leur vie est une perpétuelle dissimulation.

Selon quelques-uns, il aurait été dissimulé envers lui-même, puisque dans son entretien intime avec les dieux, sur les bords du Gran, parlant d'une épouse indigne de lui, il les aurait remerciés de lui avoir donné "une femme si complaisante, si affectueuse, si simple." J'ai montré ailleurs qu'on s'est quelque peu exagéré sur ce point la patience, ou, si l'on veut, la faiblesse de Marc-Aurèle. Faustine eut des torts; le plus grand fut d'avoir pris en aversion les amis de son mari; comme ce furent ces amis qui écrivirent l'histoire, elle en porta la peine devant la postérité. Mais une critique attentive n'a pas de peine à montrer ici les exagérations de la légende. Tout porte à croire que Faustine trouva d'abord le bonheur et l'amour dans cette villa de Lorium ou dans cette belle retraite de Lanuvium, sur les dernières pentes du mont Albain, que Marc-Aurèle décrit à Fronton, son maître, comme un séjour plein des joies les plus pures. Puis elle se fatigua de tant de sagesse. Disons tout: les belles sentences de Marc-Aurèle, sa vertu austère, sa perpétuelle mélancolie, purent sembler ennuyeuses à une femme jeune, capricieuse, d'un tempérament ardent et d'une merveilleuse beauté. Il le comprit, en souffrit et se tut. Faustine resta toujours "sa très bonne et très fidèle épouse." On ne réussit jamais, même après qu'elle fut morte, à lui faire abandonner ce pieux mensonge. Dans un bas-relief qui se voit encore aujourd'hui à Rome au musée du Capitole, pendant que Faustine est enlevée au ciel par une Renommée, l'excellent empereur la suit de terre avec un regard plein d'amour. Il était arrivé, ce semble, dans les derniers temps, à se faire illusion à lui-même et à tout oublier. Mais quelle lutte il dut traverser pour en arriver là! Durant de longues années, une maladie de cœur le consuma lentement. L'effort désespéré qui fait l'essence

de sa philosophie, cette frénésie de renoncement, poussée parfois jusqu'au sophisme, dissimulent au fond une immense blessure. Qu'il faut avoir dit adieu au bonheur pour arriver à de tels excès ! On ne comprendra jamais tout ce que souffrit ce pauvre cœur flétri, ce qu'il y eut d'amertume dissimulée par ce front pâle, toujours calme et presque toujours souriant. Il est vrai que l'adieu au bonheur est le commencement de la sagesse, et le moyen le plus sûr pour trouver le bonheur. Il n'y a rien de doux comme le retour de joie qui suit le renoncement à la joie; rien de vif, de profond, de charmant comme l'enchantement du désenchanté.

Des historiens, plus ou moins imbus de cette politique qui se croit supérieure parce qu'elle n'est suspecte d'aucune philosophie, ont naturellement cherché à prouver qu'un homme, si accompli, fût un mauvais administrateur et un médiocre souverain. Il paraît, en effet, que Marc-Aurèle pécha plus d'une fois par trop d'indulgence. Mais jamais règne ne fut plus fécond en réformes et en progrès. L'assistance publique, fondée par Nerva et Trajan, reçut de lui d'admirables développements. Des collèges nouveaux pour l'éducation gratuite furent établis; les procurateurs alimentaires devinrent des fonctionnaires de premier ordre et furent choisis avec un soin extrême; on pourvut à l'éducation des femmes pauvres par l'institut des *Jeunes Faustiniennes*. Le principe que l'État a des devoirs en quelque sorte paternels envers ses membres (principe dont il faudra se souvenir avec gratitude, même quand on l'aura dépassé), ce principe, dis-je, a été proclamé pour la première fois dans le monde par les Antonins. Ni le faste puéril des royautés orientales, fondées sur la bassesse et la stupidité des hommes, ni l'orgueil pédantesque des royautés du moyen-âge, fondées sur un sentiment exagéré de l'hérédité et sur une foi naïve dans les droits du sang, ne peuvent nous donner une idée de cette souveraineté toute républicaine de Nerva, de Trajan, d'Adrien, d'Antonin, de Marc-Aurèle. Rien du prince héréditaire ou par droit divin; rien, non plus, du chef militaire; c'était une sorte de grande magistrature civile, sans rien qui ressemblât à une cour ni qui enlevât à l'empereur son caractère tout privé. Marc-Aurèle, en particulier, ne fut ni peu ni beaucoup un roi, dans le sens propre du mot; sa fortune était industrielle, elle consistait surtout en briqueteries; son aversion pour "les césars," qu'il envisage comme des espèces de Sardanapales, magnifiques, débauchés et cruels, éclate à chaque instant. La civilité de ses mœurs était extrême; il rendit au sénat toute son ancienne importance; quand il était à Rome, il ne manquait jamais une séance, et ne quittait sa place que quand le consul avait prononcé la formule: *Nihil vos moramur, patres conscripti.* Presque toutes les années de son règne il fit la guerre, et il la fit bien, quoiqu'il n'y trouvât que de l'ennui. Ses insipides campagnes contre les Quades et les Marcomans furent très bien conduites; le dégoût qu'il en éprouvait ne l'empêchait pas d'y mettre l'application la plus consciencieuse.

Ce fut dans le cours d'une de ces expéditions que, campé sur les

bords du Gran, au milieu des plaines monotones de la Hongrie, il écrivit les plus belles pages du livre exquis qui nous a révélé son âme tout entière. Il est probable que, de bonne heure, il tint un journal intime de ses pensées. Il y inscrivait les maximes auxquelles il recourait pour se fortifier, les réminiscences de ses auteurs favoris, les passages des moralistes qui lui parlaient le plus, les principes qui dans la journée l'avaient soutenu, parfois les reproches que sa conscience scrupuleuse croyait avoir à s'adresser. " On se cherche des retraites solitaires, chaumières rustiques, rivages des mers, montagnes ; comme les autres, tu aimes à rêver ces biens. A quoi bon, p
t'est permis à chaque heure de te retirer en ton âme ? Nulle part l'homme n'a de retraite plus tranquille, surtout s'il a en lui-même de ces choses dont la contemplation suffit pour rendre le calme. Sache donc jouir de cette retraite, et là renouvelle tes forces. Qu'il y ait là de ces maximes courtes, fondamentales, qui, tout d'abord, rendront la sérénité à ton âme et te remettront en état de supporter avec résignation le monde où tu dois revenir." Pendant les tristes hivers du Nord, cette consolation lui devint encore plus nécessaire. Il avait près de soixante ans ; la vieillesse était chez lui prématurée. Un soir, toutes les images de sa pieuse jeunesse remontèrent en son souvenir, et il passa quelques heures délicieuses à supputer ce qu'il devait à chacun des êtres bons qui l'avaient entouré.

" Exemples de mon aïeul Vérus : Douceur de mœurs, patience inaltérable."

" Qualités qu'on prisait dans mon père, souvenir qu'il m'a laissé : Modestie, caractère mâle."

" Imiter de ma mère sa piété, sa bienfaisance ; m'abstenir, comme elle, non seulement de faire le mal, mais même d'en concevoir la pensée ; mener sa vie frugale, et qui ressemblait si peu au luxe habituel des riches."

Puis lui apparaissent tour à tour Diogénète, qui lui inspira le goût de la philosophie et rendit agréables à ses yeux le grabat, la couverture composée d'une simple peau et tout l'appareil de la discipline hellénique ; Junius Rusticus, qui lui apprit à éviter toute affectation d'élégance dans le style et lui prêta les Entretiens d'Épictète ; Apollonius de Chalcis, qui réalisait l'idéal stoïcien de l'extrême fermeté et de la parfaite douceur ; Sextus de Chéronée, si grave et si bon ; Alexandre le grammairien, qui reprenait avec une politesse si raffinée ; Fronton, " qui lui apprit ce qu'il y a, dans un tyran, d'envie, de duplicité, d'hypocrisie, et ce qu'il peut y avoir de dureté dans le cœur d'un patricien ; " son frère Sévérus, " qui lui fit connaître Thraséas, Helvidius, Caton, Brutus, qui lui donna l'idée de ce qu'est un État libre, où la règle est l'égalité naturelle des citoyens et l'égalité de leurs droits ; d'une royauté qui place avant tout le respect de la liberté des citoyens," et, dominant tous les autres de sa grandeur immaculée, Antonin, son père par adoption, dont il nous trace l'image avec un redoublement de reconnaissance et d'amour. " Je remercie les dieux, dit-il en terminant, de m'avoir donné de bons aïeuls, de bons parents,

une bonne sœur, de bons maîtres, et, dans mon entourage, dans mes proches, dans mes amis, des gens presque tous remplis de bonté. Jamais je ne me suis laissé aller à aucun manque d'égards envers eux; par ma disposition naturelle, j'aurais pu, dans l'occasion, commettre quelque irrévérence; mais la bienfaisance des dieux n'a pas permis que la circonstance s'en soit présentée. Je dois encore aux dieux d'avoir conservé pure la fleur de ma jeunesse; d'avoir été élevé sous la loi d'un prince et d'un père qui devait dégager mon âme de toute fumée d'orgueil, me faire comprendre qu'il est possible, tout en vivant dans un palais, de se passer de gardes, d'habits resplendissants, de torches, de statues, m'apprendre enfin qu'un prince peut presque resserrer sa vie dans les limites de celles d'un simple citoyen, sans montrer pour cela moins de noblesse et moins de vigueur, quand il s'agit d'être empereur et de traiter les affaires de l'État. Ils m'ont donné de rencontrer un frère dont les mœurs étaient une continuelle exhortation à veiller sur moi-même, en même temps que sa déférence et son attachement devaient faire la joie de mon cœur. Grâce aux dieux encore, je me suis hâté d'élever ceux qui avaient soigné mon éducation aux honneurs qu'ils semblaient désirer. Ce sont eux qui m'ont fait connaître Apollonius, Rusticus, Maximus, et qui m'ont offert, entourée de tant de lumière, l'image d'une vie conforme à la nature. Je suis resté en deçà du but, il est vrai; mais c'est ma faute. Si mon corps a résisté longtemps à la rude vie que je mène; si, malgré mes fréquents dépits contre Rusticus, je n'ai jamais passé les bornes ni rien fait dont j'aie eu à me repentir; si ma mère, qui devait mourir jeune, a pu néanmoins passer près de moi ses dernières années; si, chaque fois que j'ai voulu venir au secours de quelque personne pauvre ou affligée, je ne me suis jamais entendu dire que l'argent me manquait; si moi-même je n'ai eu besoin de rien recevoir de personne; si j'ai une femme d'un tel caractère, si complaisante, si affectueuse, si simple; si j'ai trouvé tant de gens capables pour l'éducation de mes enfants; si, à l'origine de ma passion pour la philosophie, je ne suis pas devenu la proie de quelque sophiste, c'est aux dieux que je le dois. Oui, tant de bonheurs ne peuvent être l'effet que de l'assistance des dieux et d'une heureuse fortune."

Cette divine candeur respire à chaque page. Jamais on n'écrivit plus simplement pour soi, à seule fin de décharger son cœur, sans autre témoin que Dieu. Pas une ombre de système. Marc-Aurèle, à proprement parler, n'a pas de philosophie; quoiqu'il doive presque tout au stoïcisme transformé par l'esprit romain, il n'est d'aucune école. Selon notre goût, il a trop peu de curiosité; car il ne sait pas tout ce que devait savoir un contemporain de Ptolémée et de Galien; il a quelques opinions sur le système du monde qui n'étaient pas au niveau de la plus haute science de son temps. Mais sa pensée morale, ainsi dégagée de tout lien avec un système, y gagne une singulière hauteur. L'auteur du livre de *l'Imitation* lui-même, quoique fort détaché des querelles d'école, n'atteint pas jusque-là; car sa manière de sentir est essentiellement chrétienne; ôtez les dogmes chrétiens, son livre

ne garde plus qu'une partie de son charme. Le livre de Marc-Aurèle, n'ayant aucune base dogmatique, conservera éternellement sa fraîcheur. Tous, depuis l'athée ou celui qui se croit tel, jusqu'à l'homme le plus engagé dans les croyances particulières de chaque culte, peuvent y trouver des fruits d'édification. C'est le livre le plus purement humain qu'il y ait. Il ne tranche aucune question controversée. En théologie, Marc-Aurèle flotte entre le déisme pur, le polythéisme interprété dans un sens physique à la façon des stoïciens, et une sorte de panthéisme cosmique. Il ne tient pas beaucoup plus à l'une des hypothèses qu'à l'autre, et il se sert indifféremment des trois vocabulaires, déiste, polythéiste, panthéiste. Ses considérations sont toujours à deux faces, selon que Dieu et l'âme ont ou n'ont pas de réalité. C'est le raisonnement que nous faisons à chaque heure ; car, si c'est le matérialisme le plus complet qui a raison, nous qui aurons cru au vrai et au bien, nous ne serons pas plus dupés que les autres. Si l'idéalisme a raison, nous aurons été les vrais sages et nous l'aurons été de la seule façon qui nous convienne, c'est-à-dire sans nulle attente intéressée, sans avoir compté sur une rémunération.

II.

Nous touchons ici au grand secret de la philosophie morale et de la religion. Marc-Aurèle n'a pas de philosophie spéculative ; sa théologie est tout à fait contradictoire ; il n'a aucune idée arrêtée sur l'âme et l'immortalité. Comment fut-il profondément moral sans les croyances qu'on regarde aujourd'hui comme les fondements de la morale ? Comment fut-il éminemment religieux sans avoir professé aucun des dogmes de ce qu'on appelle la religion naturelle ? C'est ce qu'il importe de rechercher.

Les doutes qui, au point de vue de la raison spéculative, planent sur les vérités de la religion naturelle ne sont pas, comme Kant l'a admirablement montré, des doutes accidentels, susceptibles d'être levés, tenant, ainsi qu'on se l'imagine parfois, à certains états de l'esprit humain. Ces doutes sont inhérents à la nature même de ces vérités, et l'on peut dire sans paradoxe que, si ces doutes étaient levés, les vérités auxquelles ils s'attaquent disparaîtraient du même coup. Supposons, en effet, une preuve directe, positive, évidente pour tous, des peines et des récompenses futures ; où sera le mérite de faire le bien ? Il n'y aurait que des fous qui de gaîté de cœur courraient à leur damnation. Une foule d'âmes basses feraient leur salut cartes sur table ; elles forceraient, en quelque sorte, la main de la divinité. Qui ne voit que, dans un tel système, il n'y a plus ni morale ni religion ? Dans l'ordre moral et religieux, il est indispensable de croire sans démonstration ; il ne s'agit pas de certitude, mais de foi. Voilà ce qu'oublie le déisme, avec ses habitudes d'affirmation intempérante. Il oublie que des croyances trop précises sur la destinée humaine enlèveraient tout le mérite moral. Pour nous, on nous annoncerait un argument péremptoire en ce genre, que nous ferions

comme saint Louis, quand on lui parla de l'hostie miraculeuse. Nous refuserions d'aller voir. Qu'avons-nous besoin de ces preuves brutales, qui gêneraient notre liberté ? Nous craindrions d'être assimilés à ces spéculateurs de vertu ou à ces peureux vulgaires, qui portent dans les choses de l'âme le grossier égoïsme de la vie pratique. Dans les premiers jours qui suivirent la foi à la résurrection de Jésus, ce sentiment se fit jour de la façon la plus touchante. Les vrais amis de cœur, les délicats aimèrent mieux croire sans preuve que de voir. "Heureux ceux qui n'ont pas vu et qui ont cru !" devint le mot de la situation. Mot charmant ! Symbole éternel de l'idéalisme tendre et généreux, qui a horreur de toucher de ses mains ce qui ne doit être vu qu'avec le cœur !

Notre bon Marc-Aurèle, sur ce point comme sur tous les autres, devança les siècles. Jamais il ne se soucia de se mettre d'accord avec lui-même sur Dieu et sur l'âme. Comme s'il avait lu la *Critique de la Raison pratique*, il vit bien que, lorsqu'il s'agit de l'infini, aucune formule n'est absolue, et qu'en pareille matière on ne peut avoir quelque chance d'avoir vu, une fois en sa vie, la vérité que si l'on s'est beaucoup contredit. Il détacha hautement la beauté morale de toute théologie arrêtée ; il ne permit au devoir de dépendre, d'aucune opinion métaphysique, sur la cause première. Jamais l'union intime avec le dieu caché ne fut poussée à de plus inouïes délicatesses. "Offre au gouvernement du dieu qui est au dedans de toi un être viril, mûri par l'âge, ami du bien public, un Romain, un empereur ; un soldat à son poste, attendant le signal de la trompette ; un homme prêt à quitter sans regret la vie."—"Il y a bien des grains d'encens destinés au même autel ; l'un tombe plus tôt, l'autre plus tard dans le feu ; mais la différence n'est rien."—"L'homme doit vivre selon la nature pendant le peu de jours qui lui sont donnés sur la terre, et, quand le moment de la retraite est venu, se soumettre avec douceur, comme une olive qui, en tombant, bénit l'arbre qui l'a produite, et rend grâce au rameau qui l'a portée."—"Tout ce qui t'arrange m'arrange, ô cosmos. Rien ne m'est prématuré ou tardif de ce qui, pour toi, vient à l'heure. Je fais mon fruit de ce que portent tes saisons, ô nature. De toi vient tout ; en toi est tout ; vers toi va tout."—"O homme ! tu as été citoyen dans la grande cité ; que t'importe de l'avoir été pendant cinq ou pendant trois ans ? Ce qui est conforme aux lois n'est inique pour personne. Qu'y a-t-il donc de si fâcheux à être renvoyé de la cité, non par un tyran, non par un juge inique, mais par la nature même qui t'y avait fait entrer ? C'est comme quand un comédien est congédié du théâtre par le préteur qui l'y avait engagé. Mais, diras-tu, je n'ai pas joué les cinq actes ; je n'en ai joué que trois. Tu dis bien ; mais, dans la vie, trois actes suffisent pour faire la pièce entière. . . . Pars donc content, puisque celui qui te congédie est content."

Est-ce à dire qu'il ne se révoltât pas quelquefois contre le sort étrange qui s'est plu à laisser seuls, face à face, l'homme avec ses éternels besoins de dévouement, de sacrifice, d'héroïsme, et la nature,

avec son immoralité transcendante, son suprême dédain pour la vertu? Non. Une fois du moins l'absurdité, la colossale iniquité de la mort le frappe. Mais bientôt son tempérament complètement mortifié reprend le dessus, et il se calme. "Comment se fait-il que les dieux, qui ont ordonné si bien toutes choses, et avec tant d'amour pour les hommes, aient négligé un seul point, à savoir, que les hommes d'une vertu éprouvée, qui ont eu pendant leur vie une sorte de commerce avec la divinité, qui se sont fait aimer d'elle par leurs actions pieuses et leurs sacrifices, ne revivent pas après la mort, mais soient éteints pour jamais ? Puisque la chose est ainsi, sache bien que, si elle avait dû être autrement, ils n'y eussent pas manqué ; car, si cela eût été juste, cela était possible ; si cela eût été conforme à la nature, la nature l'eût comporté. Par conséquent, de cela qu'il n'en est pas ainsi, confirme-toi en cette considération qu'il ne fallait pas qu'il en fût ainsi. Tu vois bien toi-même que faire une telle recherche, c'est disputer avec Dieu sur son droit. Or, nous ne disputerions pas ainsi contre les dieux, s'ils n'étaient pas souverainement bons et souverainement justes ; s'ils le sont, ils n'ont rien laissé passer dans l'ordonnance du monde qui soit contraire à la justice et à la raison."

Ah ! c'est trop de résignation, Mesdames et Messieurs. S'il en est véritablement ainsi, nous avons droit de nous plaindre. Dire que si ce monde n'a pas sa contre-partie, l'homme qui s'est sacrifié pour le bien ou le vrai doit le quitter content et absoudre les dieux, cela est trop naïf. Non, il a le droit de les blasphémer ! Car enfin pourquoi avoir ainsi abusé de sa crédulité ? Pourquoi avoir mis en lui des instincts trompeurs, dont il a été la dupe honnête ? Pourquoi cette prime accordée à l'homme frivole ou méchant ? C'est donc celui-ci, qui ne se trompe pas, qui est l'homme avisé ?... Mais alors maudits soient les dieux qui placent si mal leurs préférences ! Je veux que l'avenir soit une énigme ; mais, s'il n'y a pas d'avenir, ce monde est un affreux guet-apens. Remarquez, en effet, que notre souhait n'est pas celui du vulgaire grossier. Ce que nous voulons, ce n'est pas de voir le châtiment du coupable, ni de toucher les intérêts de notre vertu. Ce que nous voulons n'a rien d'égoïste : c'est simplement d'être, de rester en rapport avec la lumière, de continuer notre pensée commencée, d'en savoir davantage, de jouir un jour de cette vérité que nous cherchons avec tant de travail, de voir le triomphe du bien que nous avons aimé. Rien de plus légitime. Le digne empereur, du reste, le sentait bien. "Quoi ! la lumière d'une lampe brille jusqu'au moment où elle s'éteint, et ne perd rien de son éclat ; et la vérité, la justice, la tempérance, qui sont en toi, s'éteindraient avec toi ! " Toute la vie se passa, pour lui, dans cette noble hésitation. S'il pécha, ce fut par trop de piété. Moins résigné, il eût été plus juste ; car sûrement demander qu'il y ait un spectateur intime et sympathique des luttes que nous livrons pour le bien et le vrai, ce n'est pas trop demander.

Il est possible aussi que, si sa philosophie eût été moins exclusivement morale, si elle eût impliqué une étude plus curieuse de l'histoire

et de l'univers, elle eût évité certains excès de rigueur. Comme les ascètes chrétiens, Marc-Aurèle pousse quelquefois le renoncement jusqu'à la sécheresse et la subtilité. Ce calme qui ne se dément jamais, on sent qu'il est obtenu par un immense effort. Certes, le mal n'eut jamais pour lui aucun attrait ; il n'eut à combattre aucune passion : "Quoi qu'on fasse ou quoi qu'on dise, écrit-il, il faut que je sois homme de bien, comme l'émeraude peut dire : 'Quoi qu'on dise ou qu'on fasse, il faut bien que je sois émeraude et que je garde ma couleur.' Mais, pour se tenir toujours sur le sommet glacé du stoïcisme, il lui fallut faire de cruelles violences à la nature et en retrancher plus d'une noble partie. Cette perpétuelle répétition des mêmes raisonnements, ces mille images sous lesquelles il cherche à se représenter la vanité de toutes choses, ces preuves souvent naïves, de l'universelle frivolité témoignent des combats qu'il eut à livrer pour éteindre en lui tout désir. Parfois il en résulte, pour nous, quelque chose d'âpre et de triste ; la lecture de Marc-Aurèle fortifie, mais ne console pas ; elle laisse dans l'âme un vide, à la fois délicieux et cruel, qu'on n'échangerait pas contre la pleine satisfaction. L'humilité, le renoncement, la sévérité pour soi-même n'ont jamais été poussés plus loin. La gloire, cette dernière illusion des grandes âmes, est réduite à néant. Il faut faire le bien sans s'inquiéter si personne le saura. Il voit bien que l'histoire parlera de lui ; il songe parfois aux hommes du passé auxquels l'avenir l'associera. "S'ils n'ont joué qu'un rôle d'acteurs tragiques, dit-il, personne ne m'a condamné à les imiter." L'absolue mortification où il était arrivé avait éteint en lui jusqu'à la dernière fibre de l'amour-propre.

La conséquence de cette philosophie austère aurait pu être la raideur et la dureté. C'est ici que la bonté rare de la nature de Marc-Aurèle éclate dans tout son jour. Sa sévérité n'est que pour lui. Le fruit de cette grande tension d'âme, c'est une bienveillance infinie. Toute sa vie fut une étude à rendre le bien pour le mal. Après quelque triste expérience de la perversité humaine, il ne trouve, le soir, à écrire que ce qui suit : "Si tu le peux, corrige-les ; dans le cas contraire, souviens-toi que c'est pour l'exercer envers eux que t'a été donnée la bienveillance. Les dieux eux-mêmes sont bienveillants pour ces êtres ; ils les aident, tant leur bonté est grande ! à acquérir santé, richesse, gloire. Il t'est permis de faire comme les dieux." Un autre jour, les hommes furent bien méchants, car voici ce qu'il écrivait sur les tablettes : "Tel est l'ordre de la nature : des gens de cette sorte doivent, de toute nécessité, agir ainsi. Vouloir qu'il en soit autrement, c'est vouloir que le figuier ne produise pas de figues. Souviens-toi, en un mot, de ceci : dans un temps bien court, toi et lui vous mourrez : bientôt après, vos noms même ne survivront plus." Ces réflexions d'universel pardon reviennent sans cesse. A peine se mêle-t-il parfois à cette ravissante bonté un imperceptible sourire. "La meilleure manière de se venger des méchants, c'est de ne pas se rendre semblable à eux ;" ou un léger accent de fierté : "C'est chose royale, quand on fait le bien, d'entendre dire du mal de soi." Un jour,

il a un reproche à se faire. "Tu as oublié, dit-il, quelle parenté sainte unit chaque homme avec le genre humain; parenté non de sang et de naissance, mais participation à la même intelligence. Tu as oublié que l'âme raisonnable de chacun est un dieu, un dérivé de l'Être suprême."

Dans le commerce de la vie, il devait être exquis, quoiqu'un peu naïf, comme le sont d'ordinaire les hommes très bons. Les neuf motifs d'indulgence qu'il se fait valoir à lui-même (livre xi, article 18) nous montrent sa charmante bonhomie en présence de difficultés de famille qui venaient peut-être de son indigne fils. "Si dans l'occasion, se dit-il à lui-même, tu l'exhortais paisiblement, et lui donnais sans colère, alors qu'il s'efforce de te faire du mal, des leçons comme celle-ci. 'Non, mon enfant! nous sommes nés pour autre chose. Ce n'est pas moi qui éprouverai le mal, c'est toi qui t'en fais à toi-même, mon enfant!' Montre-lui adroitement, par une considération générale, que telle est la règle, que ni les abeilles n'agissent comme lui, ni aucun des animaux qui vivent naturellement en troupes. N'y mets ni moquerie, ni insulte, mais l'air d'une affection véritable, d'un cœur que n'aigrit point la colère; non comme un pédant, non pour te faire admirer de ceux qui sont là; mais n'aie en vue que lui seul." Commode (si c'est de lui qu'il s'agit) fut sans doute peu sensible à cette bonne rhétorique paternelle; une des maximes de l'excellent empereur était que les méchants sont malheureux, qu'on n'est méchant que malgré soi et par ignorance; il plaignait ceux qui n'étaient pas comme lui; il ne se croyait pas le droit de s'imposer à eux.

Il voyait bien la bassesse des hommes; mais il ne se l'avouait pas. Cette façon de s'aveugler volontairement est le défaut des âmes d'élite. Le monde n'étant pas, du tout, tel qu'elles le voudraient, elles se mentent à elles-mêmes pour le voir autre qu'il n'est. De là un peu de convenu dans leurs jugements. Chez Marc-Aurèle, ce convenu nous cause parfois un certain agacement. Si nous voulions le croire, ses maîtres, dont plusieurs furent des hommes assez médiocres, auraient été sans exception des hommes supérieurs. On dirait que tout le monde autour de lui a été vertueux. Cela va à un tel point qu'on a pu se demander si ce frère, dont il fait un si grand éloge dans son action de grâces aux dieux, n'était pas son frère par adoption, Lucius-Vérus. Cela est peu probable. Mais il est sûr que le bon empereur était capable de fortes illusions, quand il s'agissait de prêter à autrui ses propres vertus.

Cette qualité, selon quelques critiques qui se sont produites dès l'antiquité, en particulier sous la plume de l'empereur Julien, lui fit commettre une faute énorme, ce fut de ne pas avoir déshérité Commode. Voilà des choses qu'il est facile de dire à distance quand les obstacles ne sont plus là, et qu'on raisonne loin des faits. On oublie d'abord que les empereurs, depuis Nerva, qui rendirent l'adoption un système politique si fécond, n'avaient pas de fils. L'adoption avec exhérédation du fils ou du petit-fils se voit au premier siècle de l'empire, mais n'a pas de bons résultats. Marc-Aurèle, par principes, était évidemment

pour l'hérédité directe, à laquelle il voyait l'avantage de prévenir les compétitions. Dès que Commode fut né, en 161, il le présenta seul aux légions, quoiqu'il eût un jumeau ; souvent il le prenait tout petit entre ses bras et renouvelait cet acte, qui était une sorte de proclamation. En 166, c'est Lucius-Vérus lui-même qui demande que les deux fils de Marc, Commode et Annius-Vérus, soient faits césars. En 172, Commode partage avec son père le titre de Germanique; en 173, après la répression de la révolte d'Avidius, le sénat, pour reconnaître en quelque sorte le désintéressement de famille qu'avait montré Marc-Aurèle, demande par acclamation l'empire et la puissance tributienne pour Commode. Déjà le mauvais naturel de ce dernier s'était trahi par plus d'un indice connu de ses pédagogues; mais comment préjuger par quelques mauvaises notes de l'avenir d'un enfant de douze ans? En 176, 177, son père le fait *Imperator*, consul, Auguste. Ce fut sûrement une imprudence ; mais on était lié par les actes antérieurs; Commode, d'ailleurs, se contenait encore. Dans les dernières années, le mal se décela tout à fait; à chaque page des derniers livres des *Pensées*, nous voyons la trace du martyre intérieur du père excellent, de l'empereur accompli, qui voit un monstre grandir à côté de lui, prêt à lui succéder et décidé à prendre en toute chose, par antipathie, le contre-pied de ce qu'il avait vu faire aux gens de bien. La pensée de déshériter Commode dut, sans doute, alors venir plus d'une fois à Marc-Aurèle. Mais il était trop tard. Après l'avoir associé à l'empire, après l'avoir proclamé tant de fois parfait et accompli devant les légions, venir à la face du monde le déclarer indigne eût été un scandale. Marc fut pris par ses propres phrases, par ce style d'une bienveillance convenue qui lui était trop habituel. Et après tout, Commode avait dix-sept ans; qui pouvait être sûr qu'il ne s'améliorerait pas? Même après la mort de Marc-Aurèle, on put l'espérer. Commode montra d'abord l'intention de suivre les conseils des personnes de mérite dont son père l'avait entouré.

Le reproche que l'on peut faire à Marc-Aurèle n'est donc pas de n'avoir point destitué son fils ; c'est d'avoir eu un fils. Ce ne fut pas sa faute si le siècle ne fut pas capable de porter tant de sagesse. En philosophie, le grand empereur avait placé si haut l'idéal de la vertu que personne ne devait se soucier de le suivre ; en politique, son optimisme bienveillant avait affaibli les services, surtout l'armée. En religion, pour avoir été trop attaché à une religion d'État dont il voyait bien la faiblesse, il prépara le triomphe violent du culte non officiel, et il laissa planer sur sa mémoire un reproche, injuste il est vrai, mais dont l'ombre même ne devrait pas se rencontrer dans une vie si pure.

Nous touchons ici à un des points les plus délicats de la biographie de Marc-Aurèle. Il est malheureusement certain que quelques condamnations à mort furent, sous son règne, prononcées et exécutées contre des chrétiens. La politique de ses prédécesseurs avait été constante à cet égard. Ils voyaient dans le christianisme une secte secrète,

anti-sociale, rêvant le renversement de l'empire ; comme tous les hommes attachés aux vieux principes romains, ils crurent à la nécessité de le réprimer. Il n'était pas besoin, pour cela, d'édits spéciaux : les lois contre les *cœtus illiciti*, les *collegia illicita* étaient nombreuses. Les chrétiens tombaient, de la manière la plus formelle, sous le coup de ces lois. Certes, il eût été digne du sage empereur, qui introduisit tant de réformes pleines d'humanité, de supprimer les édits qui entraînaient de cruelles et injustes conséquences. Mais il faut observer d'abord que le véritable esprit de liberté, comme nous l'entendons, n'était alors compris de personne, et que le christianisme, quand il fut maître, ne le pratiqua pas mieux que les empereurs païens ; en second lieu, que l'abrogation de la loi des sociétés illicites eût été la ruine de l'empire, fondé essentiellement sur ce principe que l'État ne doit admettre en son sein aucune société différente de lui. Le principe était mauvais, selon nos idées ; il est bien certain, du moins, que c'était la pierre angulaire de la constitution romaine. Marc-Aurèle, loin de l'exagérer, l'atténua de toutes ses forces, et une des gloires de son règne est l'extension qu'il donna au droit d'association. Cependant il n'alla pas jusqu'à la racine ; il n'abolit pas complètement les lois contre les *collegia illicita*, et il en résulta, dans les provinces, quelques applications infiniment regrettables. Le reproche qu'on peut lui faire est le même qu'on pourrait adresser aux souverains de nos jours qui ne suppriment pas, d'un trait de plume, toutes les lois restrictives des libertés de réunion, d'association, de la presse. A la distance où nous sommes, nous voyons bien que Marc-Aurèle, en étant plus complètement libéral, eût été plus sage. Peut-être le christianisme, laissé libre, eût-il développé, d'une façon moins désastreuse, le principe théocratique et absolu qui était en lui. Mais on ne saurait reprocher à un homme d'État de n'avoir pas provoqué une révolution radicale en prévision des événements qui doivent arriver plusieurs siècles après lui. Trajan, Adrien, Antonin, Marc-Aurèle ne pouvaient connaître des principes d'histoire générale et d'économie politique qui n'ont été aperçus que de notre temps, et que nos dernières révolutions pouvaient seules révéler. En tout cas, la mansuétude du bon empereur fut, en ceci, à l'abri de tout reproche. On n'a pas, à cet égard, le droit d'être plus difficile que Tertullien : "Consultez vos annales, dit-il aux magistrats romains, vous y verrez que les princes qui ont sévi contre nous sont de ceux qu'on tient à honneur d'avoir eus pour persécuteurs. Au contraire, de tous les princes qui ont respecté les lois divines et humaines, nommez-en un seul qui ait persécuté les chrétiens. Nous pouvons même en citer un qui s'est déclaré leur protecteur, le sage Marc-Aurèle. S'il ne révoqua pas ouvertement les édits contre nos frères, il en détruisit l'effet par les peines sévères qu'il établit contre leurs accusateurs." Il faut se rappeler que l'empire romain était dix ou douze fois grand comme la France, et que la responsabilité de l'empereur dans les jugements qui se rendaient en province était très faible. Il faut se rappeler surtout que le christianisme ne réclamait pas simplement la liberté des cultes ;

tous les cultes qui toléraient les autres étaient fort à l'aise dans l'empire ; ce qui fit au christianisme et au judaïsme une situation à part, c'était leur intolérance, leur esprit d'exclusion.

Nous avons donc vraiment raison de porter au cœur le deuil de Marc-Aurèle. Avec lui la philosophie a régné. Un moment, grâce à lui, le monde a été gouverné par l'homme le meilleur et le plus grand de son siècle. D'affreuses décadences suivirent ; mais la petite cassette qui renfermait les pensées des bords du Gran fut sauvée. Il en sortit ce livre incomparable où Épictète était surpassé, cet Évangile de ceux qui ne croient pas au surnaturel, qui n'a pu être bien compris que de nos jours. Véritable Évangile éternel, le livre des *Pensées* ne vieillira jamais, car il n'affirme aucun dogme. La vertu de Marc-Aurèle, comme la nôtre, repose sur la raison, sur la nature. Saint Louis fut un homme très vertueux, parce qu'il était chrétien ; Marc-Aurèle fut le plus pieux des hommes, non parce qu'il était païen, mais parce qu'il était un homme accompli. Il fut l'honneur de la nature humaine et non d'une religion déterminée. La science viendrait à détruire en apparence Dieu et l'âme immortelle, que le livre des *Pensées* resterait jeune encore de vie et de vérité. La religion de Marc-Aurèle est la religion absolue, celle qui résulte du simple fait d'une haute conscience morale placée en face de l'univers. Elle n'est d'aucune race, ni d'aucun pays. Aucune révolution, aucun changement, aucune découverte ne pourront la changer.

WEEKLY EVENING MEETING,

Friday, April 23, 1880.

George Busk, Esq. F.R.S. Treasurer and Vice-President, in the Chair.

Walter Herries Pollock, Esq. M.A.

Dumas Père.

Mr. Pollock began by stating that Alexandre Dumas, the elder and greater of that name, has been, perhaps, more persistently underrated, in England at least, than any modern writer of his calibre. His only English biographer devoted his feeble powers to the depreciation of his subject, and swallowed all the malevolent stories invented or exaggerated by a pamphleteer whose real name was Jacquot, and who assumed the better-sounding name of De Mirecourt. Thackeray, however, in the 'Roundabout Papers,' has constantly given praise, not more high than deserved, to a writer who, in the 1830 group, came second only, Mr. Pollock thought, to the genius who towered far above all his companions—Victor Hugo.

A number of interesting details were then given respecting the life and works of Dumas, selected from his 'Souvenirs Dramatiques' and 'Mémoires,' which have scarcely a dull page, except when they deal with politics.

Dumas came of a distinguished family, and had Creole blood. When very young he was a clerk in a public office, and was impelled by his innate genius to endeavour to enlarge his moderate income by writing dramas, having been much excited thereto through witnessing the performance of 'Hamlet' by English actors. Idolizing Shakespeare, he aimed at copying him. The rejection of his first piece, 'Christine,' through the opposition of the aged Mademoiselle Mars and the jealousy of the Classicists, has been humorously described by himself; but his 'Henri III. et sa Cour' was highly successful at the Théâtre Français. After giving an analysis of this striking play, produced when its author was only twenty-six years of age, Mr. Pollock commented on its effect in leading the way to the decisive victory which Victor Hugo gained over the Classicists by his 'Hernani.'

Dumas' generous appreciation of his contemporaries was then mentioned, as well as his quarrel with his collaborateur, Gaillardet, in

the production of the 'Tour de Nesle.' The authors fought a duel, but eventually Gaillardet rendered justice to his colleague. . . .

Dumas excelled in telling and embellishing romantic and humorous stories, and readers of 'The Three Musketeers' will remember many passages in which the heroes of that immortal work are concerned in many boyish escapades. It may be noted in passing that amongst the accusations brought against Dumas by his detractors is one to the effect that the whole of 'The Three Musketeers' was written by somebody else. It need hardly be said that the notion is on the face of it absurd and carries with it its own condemnation. But if Dumas excelled in light dialogues and in the description of wild adventure, there are, on the other hand, few writers who can touch him in scenes of dramatic passion. "There are to my mind few finer things in fiction than the scenes in the sequel to 'The Three Musketeers'—'Twenty Years Later' it is called—which deal with the trial and execution of Charles I. We know that they are not true to history; but while we read we are compelled to believe in them and to follow them with breathless interest, and that, after all, has something to say to the question of art, whether in a novelist, a painter, or an actor. I remember a conversation between M. Mounet-Sully and an English critic concerning the performance of Hamlet by Mr. Irving. The critic pointed out this and that defect, which he had discovered in the Englishman's rendering. The Frenchman heard him out, and replied, 'It may be all as you say, but what does that matter? I can only tell you that he moved me as no other actor has ever moved me, and that is all that I care about.' There is, it seems to me, in this speech a great truth, to be accepted of course, like most generalities, with certain reservations. If no fault were to be found with any performance which stirs our feelings, the occupation of criticism would be gone. The crudest means might be employed to harrow up the emotions and might pass for exquisite art. But when a high-toned and highly artistic effort is made to move us and succeeds in moving us, then surely, though we need not be blind to the shortcomings of the attempt, yet it is better to dwell more on its successful than on its insufficient results.

"Dumas Père was not of course a deliberately moral writer, but there is hardly one of his books which can be the cause of immorality to any reasonable grown-up person; while one, 'La Tulipe Noire,' specially mentioned by Thackeray, has not a line which, to quote Mr. Podsnap, can call a blush to the cheek of the young person. As to Dumas succeeding in moving his readers, that of course must be a matter of individual opinion and experience. We live in a free country, and no one is forced to admire or like Dumas' writing. But those who do not are, I think, deprived of a considerable pleasure.

"Dumas was born in 1802 at Villers Cotterets, a small country town between Paris and Rheims, and he died in 1870. He began writing when he was between twenty and thirty, and in the course

of his life he produced rather more than three hundred romances and eighty dramas, besides ephemeral articles. One of his detractors went through an elaborate calculation to prove that no one man could have written every word that appeared with Dumas' name attached to it. It would be absurd to argue that he did write every such word, and his admirers would perhaps be sorry to think, from a literary point of view, that he was guilty of all the stuff that was put forth under his name. The third volume of 'Les Quarante Cinq,' for instance, is most obviously from an alien hand. From a moral point of view it is not perhaps desirable to defend the practice of adopting other people's work as one's own. Only let it be observed that the work which Dumas did so adopt is never equal to his own, and can be recognized as not being his own, just as the pupil's work in what are called the studio-pictures of the old masters can be recognized.

"As to his being merely an arranger of other people's ideas, that is a charge which might as easily and as justly be brought against many writers of greater genius and fame. He never concealed the sources of his inspirations. He has recorded how his first successful drama was founded on a passage in an old French chronicler, and on a chapter in Walter Scott. Is there anything more disgraceful in thus putting two and two together than in Shakespeare's going for his plots to Holinshed? If taking suggestions from history and fiction is criminal, then almost every writer of mark is worthy of the hulks. But the fact is that the meanest reptile, if it has a sting, is capable of doing damage out of all proportion to its apparent power. The artfully concocted slanders of Jacquot, self-styled De Mirecourt, have left their mark. They have been eagerly seized on by all the tribe of writers to whose nature the key-note is envy; and they have spread so far that unhappily one cannot say of them, what Pierre Clément said of a libellous pamphlet on Colbert, published just after the great Minister's death, 'History takes no notice of these anonymous insults.' All one can do is to lift up one's voice against them.

"To sum up, Dumas was born, as has been said, in 1802, and died in 1870. When as a very young man he occupied a somewhat dreary position as a clerk in a public office, he was fired by a noble ambition which first assumed a definite shape under the influence of Shakespeare. He rose, and quickly, to the very height of success. It was his fault that he bore himself with less dignity after than before he had attained his success, for, amongst other things, it certainly was somewhat undignified to adopt the system of unacknowledged collaboration. But even if the greater part of the charges brought against him in this respect were admitted, it would still be seen that his industry was no less extraordinary than his imagination. He acquired and kept a position in the first rank as a play-writer, as a novelist, and as a writer of that kind of discursive essay of which Mr. Sala is in England the master. He had immense wit, not a little poetical feeling, a perfect command of dramatic resource, and unflagging gaiety. If he wrote much that one would not put into the hands of boys and

maidens, yet there is some of his writing which is stainless, and where is there an author of the same calibre who has written exclusively for boys and maidens? His method was at any rate, like that of the play-writer quoted by Hamlet, 'an honest method'; he did not palter, as the modern French school of play-writing does, with vice and virtue, keeping one foot in the dominion of earth, and casting a false glamour of splendour around corruption. He made immense sums, and unhappily spent them more easily than he got them. He was open-handed to a fault. He had a child-like vanity and a child-like simplicity mixed with a curious astuteness. His name will I think live, and his work be rated at its proper value long after the efforts of his detractors are forgotten."

[W. H. P.]

WEEKLY EVENING MEETING,

Friday, April 30th, 1880.

Thomas Boycott, M.D. F.L.S. Vice-President, in the Chair.

G. J. Romanes, Esq. F.R.S.

Mental Evolution.

(Abstract deferred.)

ANNUAL MEETING,

Saturday, May 1, 1880.

George Busk, Esq. F.R.S. Treasurer and Vice-President, in the Chair.

The Annual Report of the Committee of Visitors for the year 1879, testifying to the continued prosperity and efficient management of the Institution, was read and adopted. The Real and Funded Property now amounts to nearly 85,000*l.*, entirely derived from the Contributions and Donations of the Members.

Forty-nine new Members paid their Admission Fees in 1879.

Sixty-seven Lectures and Twenty Friday Evening Discourses were delivered in 1879.

The Books and Pamphlets presented in 1879 amounted to about 278 volumes, making, with 509 volumes (including Periodicals bound) purchased by the Managers, a total of 787 volumes added to the Library in the year.

Thanks were voted to the President, Treasurer, and Secretary, to the Committees of Managers and Visitors, and to the Professors, for their services to the Institution during the past year.

The following Gentlemen were unanimously elected as Officers for the ensuing year:

President—The Duke of Northumberland, D.C.L. LL.D.
Treasurer—George Busk, Esq. F.R.C.S. F.R.S.
Secretary—Warren De La Rue, Esq. M.A. D.C.L. F.R.S.

Managers.

The Earl Bathurst.
George Berkley, Esq. M.I.C.E.
William Bowman, Esq. F.R.S. F.R.C.S.
Thomas Boycott, M.D. F.L.S.
Frederick Joseph Bramwell, Esq. F.R.S.
Joseph Brown, Esq. Q.C.
The Earl of Derby, M.A. LL.D. F.R.S.
Capt. Douglas Galton, C.B. D.C.L. F.R.S.
Hon. Sir Wm. R. Grove, M.A. D.C.L. F.R.S.
Cæsar Henry Hawkins, Esq F.R.S. F.R.C.S.
William Watkiss Lloyd, Esq.
Henry Pollock, Esq.
John Rae, M.D. L.L.D.
Robert P. Roupell, Esq. M.A. Q.C.
James Spedding, Esq.

Visitors.

George B. Buckton, Esq. F.R.S. F.L.S.
Stephen Busk, Esq.
The Lord Sackville Cecil.
George Howard Darwin, Esq. M.A. F.R.S.
William Henry Domville, Esq.
James N. Douglass, Esq.
Right Hon. The Lord Claud Hamilton.
Alfred G. Henriques, Esq.
Robert Mann, M.D. F.R.C.S.
John Fletcher Moulton, Esq.
William Henry Preece, Esq. M.I.C.E.
Lachlan Mackintosh Rate, Esq.
James Romanes, Esq.
Hon. John Gage Prendergast Vereker.
Edward Woods, Esq. M.I.C.E.

GENERAL MONTHLY MEETING.

Monday, May 3, 1880.

George Busk, Esq. F.R.S. Treasurer and Vice-President, in the Chair.

The following Vice-Presidents for the ensuing year were announced:—

Earl Bathurst.
William Bowman, Esq. F.R.S.
Thomas Boycott, M.D. F.L.S.
Joseph Brown, Esq. Q.C.
George Busk, Esq. F.R.S. Treasurer.
Warren De La Rue, Esq. M.A. D.C.L. F.R.S. Secretary.

Colonel James McLeod Innes, R.E.
Sydney Ernest Kennedy, Esq.
Mrs. Bernarda Lees,
Edward Pollock, Esq.
Charles Van Raalte, Esq.

were elected Members of the Royal Institution.

John Tyndall, Esq. D.C.L. LL.D. F.R.S.
was re-elected Professor of Natural Philosophy.

The Managers reported that they had re-appointed Professor James Dewar, M.A. F.R.S. as Fullerian Professor of Chemistry.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

Accademia dei Lincei, Reale, Roma—Atti, Serie Terza: Transunti: Tome IV. Fasc. 4. 4to. 1879.
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WEEKLY EVENING MEETING,

Friday, May 7, 1880.

Thomas Boycott, M.D. F.L.S. Vice-President, in the Chair.

William Henry Flower, LL.D. F.R.S. P.Z.S. &c.

Hunterian Professor of Comparative Anatomy, Royal College of Surgeons of England.

Fashion in Deformity.

I have to ask your attention this evening to certain outward manifestations of a propensity common to human nature in every aspect in which we are acquainted with it—the most primitive and barbarous, and the most civilized and refined—but one which is, as far as I know, peculiar to human nature.

I shall speak of *deformity* in the sense of alteration of the natural form of any part of the body, and those cases of voluntary deformation will be considered which are performed, not by isolated individuals, or with special motives, but by considerable numbers of members of a community in imitation of one another—in fact, according to *fashion*, "that most inexorable tyrant to which the greater part of mankind are willing slaves."

Fashion is now often associated with change, but in more primitive communities fashions of all sorts are more permanent than with us; and in all communities such fashions as those I am now speaking of are, for obvious reasons, far less likely to be subject to the fluctuations of caprice than those affecting the dress only, which, even in Shakespeare's time, changed so often that "the fashion wears out more apparel than the man." Alterations once made in the form of the body cannot be discarded or modified in the lifetime of the individual, and therefore as fashion is intrinsically imitative, such alterations have the strongest possible tendency to be reproduced generation after generation.

The origins of these fashions are mostly lost in obscurity, all attempts to solve them being little more than guesses. Some of them have become associated with religious or superstitious observances, and so have been spread and perpetuated; some have been vaguely thought to be hygienic in motive; most have some relation to conventional standards of improved personal appearance; but whatever their origin, the desire to conform to common usage, and not to appear singular, is the prevailing motive which leads to their continuance.

The most convenient classification of these customs will be one

which is based upon the part of the body affected by them, and I will begin with the more superficial and comparatively trivial—the treatment of the hair and other appendages of the skin.

Here we are at once introduced to the domain of fashion in her most potent sway. The facility with which hair lends itself to various methods of treatment has been a temptation too great to resist in all known conditions of civilization. Innumerable variations of custom exist in different parts of the world, and marked changes in at least all more or less civilized communities have characterized successive epochs of history. Not only the length and method of arrangement, but even the colour of the hair, is changed in obedience to caprices of fashion. In many of the islands of the Western Pacific, the naturally jet-black hair of the natives is converted into a tawny brown by the application of lime, obtained by burning the coral found so abundantly on their shores; and not many years since similar means were employed for producing the same result among the ladies of Western Europe—a fact which considerably diminishes the value of an idea entertained by many ethnologists, that community of custom is evidence of community of origin or of race.

Notwithstanding the painful and laborious nature of the process, when conducted with no better implements than flint knives, or pieces of splintered bone or shell, the custom of keeping the head closely shaved prevails extensively among savage nations. This, doubtless, tends to cleanliness, and perhaps comfort, in hot countries; but the fact that it is in many tribes practised only by the women and children, shows that these considerations are not those primarily engaged in its perpetuation. In some cases, as among the Fijians, while the heads of the women are commonly cropped or closely shaved, the men cultivate, at great expense of time and attention, a luxuriant and elaborately arranged mass of hair, exactly reversing the conditions met with in the most highly civilized nations.

In some regions of Africa it is considered necessary to female beauty carefully to eradicate the eyebrows, special pincers for the purpose forming part of the appliances of the toilette; while the various methods of shaving and cutting the beard among men of all nations are too well known to require more than a passing notice. The treatment of finger nails, both as to colour and form, has also been subject to fashion; but the practical inconveniences attending the inordinate length to which these are permitted to grow in some parts of the east of Asia, appears to have restricted the custom to a few localities.

If time allowed, the exceedingly wide-spread custom of tattooing the skin might be here considered, as a result of the same propensity as that which produces the other more serious deformations, now to be spoken of; but it will be as well to pass at once to these.

The nose, the lips, and the ears have in almost all races offered great temptations to be used as foundations for the display of ornament, some process of boring, cutting, or alteration of form being

necessary to render them fit for the purpose. When Captain Cook, exactly one hundred years ago, was describing the naked savages of the east coast of Australia,* he says :—" Their principal ornament is the bone which they thrust through the cartilage which divides the nostrils from each other. What perversion of taste could make them think this a decoration, or what could prompt them, before they had worn it or seen it worn, to suffer the pain and inconvenience that must of necessity attend it, is perhaps beyond the power of human sagacity to determine. As this bone is as thick as a man's finger, and between

FIG. 1.

Australian Native, with bone nose-ornament.

five and six inches long, it reaches quite across the face, and so effectually stops up both the nostrils that they are forced to keep their mouths wide open for breath, and snuffle so when they attempt to speak that they are scarcely intelligible even to each other. Our seamen, with some humour, called it their spritsail-yard; and indeed it had so ludicrous an appearance, that till we were used to it we found it difficult to refrain from laughter."

Eight years later, on his visit to the north-west coast of America, Captain Cook found precisely the same custom prevailing among the natives of Prince William's Sound, whose mode of life was in most other respects quite dissimilar to that of the Australians, and who belong ethnologically to a totally different branch of the human race.

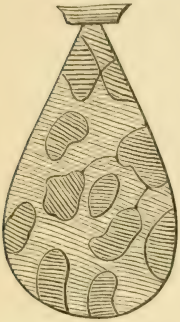
In 1681, Dampier † thus describes a custom which he found exist-

* 'First Voyage,' vol. ii. p. 633.

† 'Voyage Round the World,' ed. 1717, vol. i. p. 32.

ing among the natives of the Corn Islands, off the Mosquito Coast, in Central America :—" They have a Fashion to cut Holes in the Lips of the Boys when they are young, close to their Chin, which they keep open with little Pegs till they are fourteen or fifteen years old; then they wear Beards in them, made of Turtle or Tortoise-shell, in the Form you see in the Margin. The little Knotch at the upper end they put in through the Lip, where it remains between the Teeth and the Lip; the under Part hangs down over their chin. This they commonly wear all day, and when they sleep they take it out. They have likewise Holes bored in their Ears, both Men and Women, when young, and by continual stretching them with great Pegs, they grow to be as big as a mill'd Five-shilling Piece. Herein they wear Pieces of Wood, cut very round and smooth, so that their Ear seems to be all Wood, with a little Skin about it."

FIG. 2.



It is a remarkable thing that an almost exactly similar custom still prevails among a tribe of Indians inhabiting the southern part of

FIG. 3.

Botocudo Indian; from Bigg-Wither's 'Pioneering in South Brazil' (1878).

Brazil—the Botocudos, so called from a Portuguese word meaning a plug or stopper. Among these people the lip-ornament consists of a conical piece of hard and polished wood, frequently weighs a quarter of

a pound, and drags down, elongates, and everts the lower lip, so as to expose the gums and teeth, in a manner which to our taste is hideous, but with them is considered an essential adjunct to an attractive and correct appearance.

In the extreme north of America, the Eskimo "pierce the lower lip under one or both corners of the mouth, and insert in each aperture a double-headed sleeve-button or dumb-bell-shaped labret, of bone, ivory, shell, stone, glass, or wood. The incision when first made is about the size of a quill, but as the aspirant for improved beauty grows older, the size of the orifice is enlarged until it reaches the width of half to three-quarters of an inch." * These operations appear to be practised only on the men, and are supposed to possess some significance other than that of mere ornament. The first piercing of the lip, which is accompanied by some solemnity as a religious feast, is performed on approaching manhood.

But the people who have carried these strange customs to the greatest excess are the Thlinkeets, who inhabit the south-eastern shores of Alaska.† "Here it is the women who, in piercing the nose and ears, and filling the apertures with bones, shells, sticks, pieces of copper, nails, or attaching thereto heavy pendants, which drag down the organs and pull the features out of place, appear to have taxed their inventive powers to the utmost, and with a success unsurpassed by any nation in the world, to produce a model of hideous beauty. This success is achieved in their wooden lip-ornament, the crowning glory of the Thlinkeet nation, described by a multitude of eye-witnesses. In all female free-born Thlinkeet children, a slit is made in the under lip, parallel with the mouth, and about half an inch below it. A copper wire, or a piece of shell or wood, is introduced into this, by which the wound is kept open and the aperture extended. By gradually introducing larger objects the required dimensions of the opening are produced. On attaining the age of maturity, a block of wood is inserted, usually oval or elliptical in shape, concave on the sides, and grooved like the wheel of a pulley on the edge in order to keep it in place. The dimensions of the block are from two to six inches in length, from one to four inches in width, and about half an inch thick round the edge, and it is highly polished. Old age has little terror in the eyes of a Thlinkeet belle; for larger lip-blocks are introduced as years advance, and each enlargement adds to the lady's social status, if not to her facial charms. When the block is withdrawn, the lip drops down upon the chin like a piece of leather, displaying the teeth, and presenting altogether a ghastly spectacle. The privilege of wearing this ornament is not extended to female slaves."

In this method of adornment the native Americans are, however, rivalled, if not eclipsed, by the negroes of the heart of Africa.

* H. H. Bancroft, 'Native Races of the Pacific States of North America,' vol. i. 1875.

† See Bancroft, *op. cit.* vol. i. for numerous citations from original observers regarding these customs.

"The Bongo women (says Schweinfurth *) delight in distinguishing themselves by an adornment which to our notion is nothing less than a hideous mutilation. As soon as a woman is married, the operation commences of extending her lower lip. This, at first only slightly bored, is widened by inserting into the orifice plugs of wood, gradually increasing in size, until at length the entire feature is enlarged to five or six times its original proportions. The plugs are cylindrical in form, not less than an inch thick, and are exactly like the pegs of bone or wood worn by the women of Musgoo. By this means the lower lip is extended horizontally till it projects far beyond the upper, which is also bored and fitted with a copper plate or nail, and now and then by a little ring, and sometimes by a bit of straw, about as thick as a lucifer-match. Nor do they leave the nose intact; similar bits of straw are inserted into the edges of the nostrils, and I have seen as many as three of these on each side. A very favourite ornament for the cartilage between the nostrils is a copper ring, just like those that are placed in the noses of buffaloes and other beasts of burden for the purpose of rendering them more tractable. The greatest coquettes among the ladies wear a clasp, or cramp, at the corners of the mouth, as though they wanted to contract the orifice, and literally to put a curb upon its capabilities. These subsidiary ornaments are not, however, found at all universally among the women, and it is rare to see them all at once upon a single individual; the plug in the lower lip of the married women is alone a *sine quâ non*, serving as it does, for an artificial distinction of race."

The slightest fold or projection of the skin furnishes an excuse for boring a hole, and inserting a plug or a ring. There are women in the country whose bodies are pierced in some way or other in little short of a hundred different places, and the men are often not far behind in the profusion with which this kind of adornment is carried out.

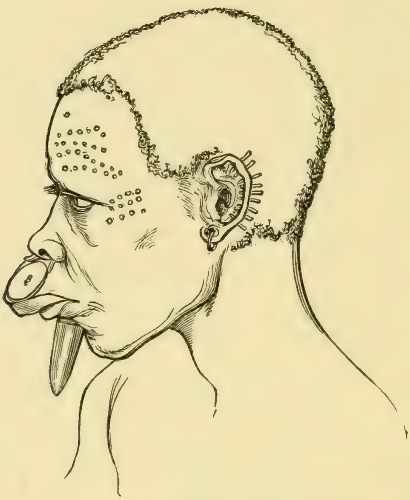
"The whole group of the Mittoo exhibits peculiarities by which it may be distinguished from its neighbours. The external adornment of the body, the costume, the ornaments, the mutilations which individuals undergo—in short, the general fashions—have all a distinctive character of their own. The most remarkable is the revolting, because unnatural, manner in which the women pierce and distort their lips; they seem to vie with each other in their mutilations, and their vanity in this respect, I believe, surpasses anything that may be found throughout Africa. Not satisfied with piercing the lower lip, they drag out the upper lip as well for the sake of symmetry.† . . . . Circular plates, nearly as large as a crown piece, made variously of quartz, of ivory, or of horn, are inserted into the lips that have been stretched by the growth of years, and then often bent in a position

* 'Heart of Africa,' vol. i. p. 297.

† The mutilation of both lips was also observed by Rohlfs among the women of Kadje, in Segseg, between Lake Tsad and the Benwe.

that is all but horizontal; and when the women want to drink they have to elevate the upper lip with their fingers, and to pour the draught into their mouth.

Fig. 4.



Loobah Woman; from Schweinfurth's 'Heart of Africa.'

"Similar in shape is the decoration which is worn by the women of Maganya; but though it is round, it is a ring and not a flat plate; it is called 'pelele,' and has no object but to expand the upper lip. Some of the Mittoo women, especially the Loobah, not content with the circle or the ring, force a cone of polished quartz through the lips as though they had borrowed the idea from the rhinoceros. This fashion of using quartz belemnites of more than two inches long, is in some instances adopted by the men."

The traveller who has been the eye-witness of such customs may well add, "Even amongst these uncultured children of nature, human pride crops up amongst the fetters of fashion, which, indeed, are fetters in the worst sense of the word; for fashion in the distant wilds of Africa tortures and harasses poor humanity as much as in the great prison of civilization."

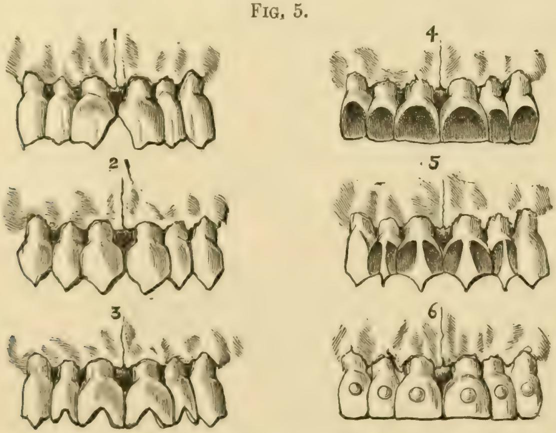
It seems, indeed, a strange phenomenon that in such different races, so far removed in locality, customs so singular—to our ideas so revolting and unnatural, and certainly so painful and inconvenient—should either have been perpetuated for an enormous lapse of time, if the supposition of a common origin be entertained, or else have developed themselves independently.

These are, however, only extreme or exaggerated cases of the almost universal custom of making a permanent aperture through

the lobe of the ear for the purpose of inserting some adventitious object by way of adornment, or even for utility, as in the man of the Island of Mangea, figured in Cook's Voyages, who carries a large knife through a hole in the lobe of the right ear. Among ourselves, the custom of wearing earrings still survives, even in the highest grades of society, although it has been almost entirely abandoned by one-half of the community, and in the other the perforation is reduced to the smallest size compatible with the purpose of carrying the ornament suspended from it.

The teeth, although allowed by the greater part of the world to retain their natural beauty and usefulness of form, still offer a field for artificial alterations according to fashion, which has been made use of principally in two distinct regions of the world and by two distinct races. It is, of course, only the front teeth, and mainly the upper incisors, that are available for this purpose. Among various tribes of negroes of Equatorial Africa, different fashions of modifying the natural form of these teeth prevail, specimens of which may be found in any large collection of crania of these people. One of the simplest consists of chipping and filing away a large triangular piece from the lower and inner edge of each of the central incisors, so that a gap is produced in the middle of the row in front (Fig. 5, 1). Another fashion is to shape all the incisors into sharp points, by chipping off the corners, giving a very formidable crocodilian appearance to the jaws (2); and another is to file out either a single or a double notch in the cutting edge of each tooth, producing a serrated border to the whole series (3).

FIG. 5.



Upper front teeth altered according to fashion. 1, 2, 3, African; 4, 5, 6, Malay.

The Malays, however, excel the Africans, both in the universality and in the fantastic variety of their supposed improvements upon nature. While the natural whiteness of the surface of these organs

is always admired by us, and by most people, the Malays take the greatest pains to stain their teeth black, which they consider greatly adds to their beauty. White teeth are looked upon with perfect disgust by the Dayaks of the neighbourhood of Sarawak. In addition to staining the teeth, filing the surface in some way or other is almost always resorted to. The nearly universal custom in Java is to remove the enamel from the front surface of the incisors, and often the canine teeth, hollowing out the surface, sometimes, but not often, so deeply as to penetrate the pulp cavity (4). The cutting edges are also worn down to a level line with pumice-stone. Another, and less common, though more elaborate fashion, is to point the teeth, and file out notches from the anterior surface of each side of the upper part of the crown, so as to leave a lozenge-shaped piece of enamel untouched; as this receives the black stain less strongly than the parts from which the surface is removed, an ornamental pattern is produced (5). In Borneo a still more elaborate process is adopted, the front surface of each of the teeth is drilled near its centre with a small round hole, and into this a plug of brass with a round or star-shaped knob is fixed (6). This is always kept bright and polished by the action of the lip over it, and is supposed to give a highly attractive appearance when the teeth are displayed.

Perhaps the strange custom, so frequently adopted by the natives of Australia, and of many islands of the Pacific, of knocking out one or more of the front teeth, might be mentioned here, but it is usually associated with some other idea than ornament or even mere fashion. In the former case it constitutes part of the rites by which the youth are initiated into manhood, and in the Sandwich Islands it is performed as a propitiatory sacrifice to the spirits of the dead.

The projection forwards of the front upper teeth, which we think unbecoming, is admired by some races, and among the negro women of Senegal it is increased by artificial means employed in childhood.*

All these modifications of form of comparatively external and flexible parts are, however, trivial in their effects upon the body to those which I shall speak of next, which induce permanent structural alterations both upon the bony framework and upon the important organs within.

Whatever might be the case with regard to the hair, the ears, the nose, and lips, or even the teeth, it might have been thought that the actual shape of the head, as determined by the solid skull, would not have been considered a subject to be modified according to the fashion of the time and place. Such, however, is far from being the case. The custom of artificially changing the form of the head is one of the most ancient and wide-spread with which we are acquainted. It is far from being confined, as many suppose, to an obscure tribe of Indians on the north-west coast of America, but is found, under various modifications, at widely different parts of the earth's surface, and

* Hamy, 'Revue d'Anthropologie,' Jan. 1879, p. 22.

among people who can have had no intercourse with one another. It appears, in fact, to have originated independently, in many quarters, from some natural impulse common to the human race. When it once became an established custom in any tribe, it was almost inevitable that it should continue, until put an end to by the destruction either of the tribe itself, or of its peculiar institutions, through the intervention of some superior force, for a standard of excellence in form, which could not be changed in those who possessed it, was naturally followed by all who did not wish their children to run the risk of the social degradation which would follow the neglect of such a custom. "Failure properly to mould the cranium of her offspring gives to the Chinook matron the reputation of a lazy and undutiful mother, and subjects the neglected children to the ridicule of their young companions, so despotic is fashion." * It is related in the narrative of Commodore Wilkes' United States Exploring Expedition,† that "at Niculuita Mr. Drayton obtained the drawing of a child's head, of the Wallawalla tribe (Fig. 6), that had just been released from its bandages, in order to secure its flattened shape. Both the parents showed great delight at the success they had met with in effecting this distortion."

FIG. 6.

Flat-headed Indian Child.

Many of the less severe alterations of the form to which the head is subjected are undesigned, resulting only from the mode in which the child is carried or dressed during infancy. Thus habitually carrying the child on one arm appears to produce an obliquity in the form of the skull which is retained to a greater or less degree all through life. The practice followed by nomadic people of carrying their infants fastened to stiff pillows or boards, commonly causes a flattening of the occiput; and the custom of dressing the child's head with tightly fitting bandages, still common in many parts of the Continent, and even used in England within the memory of many living people, produces an elongated and laterally constricted form.‡ In France this is well known, and so common is it in the neighbourhood of Toulouse, that a special form of head produced in this manner is known as the "*déformation Toulousaine.*"

Of the ancient notices of the custom of purposely altering the form

* Bancroft, *op. cit.* vol. i. p. 238.

† Vol. iv. p. 388.

‡ After the lecture a gentleman of advanced age showed me a circular depression round the upper part of his head, which he believed had been produced in this manner, as the custom was still prevailing at the time of his birth in the district of Norfolk, of which he was a native.

of the head, the most explicit is that of *Hippocrates*, who in his treatise, 'De Aëris, Aquis et Locis,' about 400 B.C., says,* speaking of the people near the boundary of Europe and Asia, near the *Palus Mœotis* (Sea of Azoff):—"I will pass over the smaller differences among the nations, but will now treat of such as are great either from nature or custom; and first, concerning the *Macrocephali*. There is no other race of men which have heads in the least resembling theirs. At first, usage was the principal cause of the length of their head, but now nature co-operates with usage. They think those the most noble who have the longest heads. It is thus with regard to the usage: immediately after the child is born, and while its head is still tender, they fashion it with their hands, and constrain it to assume a lengthened shape by applying bandages and other suitable contrivances, whereby the spherical form of the head is destroyed, and it is made to increase in length. Thus, at first, usage operated, so that this constitution was the result of force; but in the course of time it was formed naturally, so that usage had nothing to do with it."

Here, Hippocrates appears to have satisfied himself upon a point which is still discussed with great interest, and still not cleared up —the possibility of transmission by inheritance of artificially produced deformity. Some facts seem to show that such an occurrence may take place occasionally, but there is an immense body of evidence against its being habitual.

Herodotus also alludes to the same custom, as do, at later dates, Strabo, Pliny, Pomponius Mela, and others, though assigning different localities to the nations or tribes they refer to, and also indicating variations of form in their peculiar cranial characteristics.

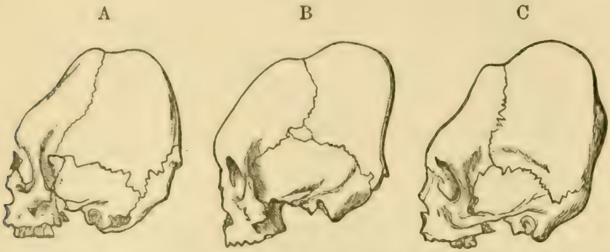
Recent archæological discoveries fully bear out these statements. Heads deformed in various fashions, but chiefly of the constricted, elongated shape, have been found in great numbers in ancient tombs, in the very region indicated by Herodotus. They have been found near Tiflis, where as many as 150 were discovered at one time, and at other places in the Caucasus, generally in rock tombs; also in the Crimea, and at different localities along the course of the Danube; in Hungary, Silesia, in the South of Germany, Switzerland, and even in France and Belgium. The people who have left such undoubted evidence of the practice of deforming their heads have been supposed by various authors to have been Avars, Huns, Tartars, or other Mongolian invaders of Europe; but later French authors who have discussed this subject are inclined to assign them to an Aryan race, who, under the name of Cimmerians, spread westward over the part of Europe in which their remains are now found, in the seventh or eighth century before our era. As the method of deformation in European specimens is not always identical, it is by no means certain that the custom may not have been in use among more than one nation. Whether the French habit, scarcely yet extinct, of tightly bandaging

* Sydenham Society's edition, by Dr. Adam, vol. i. p. 207.

the heads of infants, is derived from these people, or is of independent origin, it is impossible to say.

In Africa and Australia no analogous customs have been shown to exist, but in many parts of Asia and Polynesia, deformations, though

Fig. 7.



Skulls artificially deformed according to similar fashions. A, from an ancient tomb at Tiflis; B, from Titicaca, Peru. (From specimens in the Museum of the Royal College of Surgeons.) C, from the island of Mallicollo, New Hebrides.

usually only confined to flattening of the occiput, are common. Though often undesigned, it is done purposely, I am informed by Mr. H. B. Low, by the Dayaks, in the neighbourhood of Sarawak. Sometimes, in the islands of the Pacific, the head of the new-born infant is merely pressed by the hands into the desired form, in which case it generally soon recovers that which nature intended for it. In one island alone, Mallicollo, in the New Hebrides, the practice of permanently depressing the forehead is almost universal, and skulls are even found constricted and elongated exactly after the manner of the Aymaras of ancient Peru.

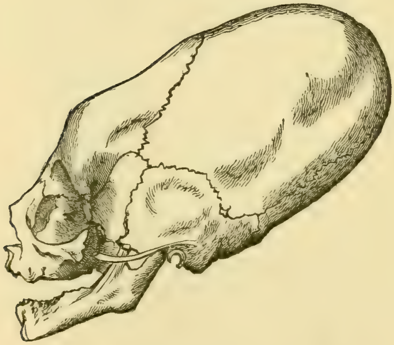
Though the Chinese usually allow the head to assume its natural form, confining their attentions to the feet, a certain class of mendicant devotees appear to have succeeded to a remarkable extent in getting their skulls elongated into a conical form, if the figure in Picart's 'Histoire des Religions,' vol. iv. plate 131, is to be trusted.

America is, however, or rather has been, the headquarters of all these fantastic practices, and especially along the western coast, and mainly in two regions, near the mouth of the Columbia River in the north, and in Peru in the south. The practice also existed among the Indians of the southern part of what are now the United States, and among the Caribs of the West India Islands. In ancient Peru, before the time of the Spanish conquest, it was almost universal. In an edict of the ecclesiastical authorities of Lima, issued in 1585, three distinct forms of deformation are mentioned. Notwithstanding the severe penalties imposed by this edict upon parents persisting in the practice, the custom was so difficult to eradicate, that another injunction against it was published by the Government as late as 1752.

In the West Indies, and the greater part of North America, the

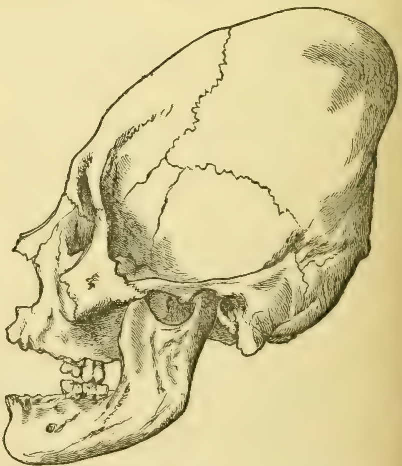
custom has become extinct with the people who used it; but the Chinook Indians, of the neighbourhood of the Columbia River, and the natives of Vancouver Island, continue it to the present day, and this is the last stronghold of this strange fashion, though under the influence of European example and discouragement it is rapidly dying out. Here the various methods of deforming the head, and their effects, have been studied and described by numerous travellers. The process commences immediately after the birth of the child, and is continued for a period of from eight to twelve months, by which time the head has permanently assumed the required form, although during subsequent growth it may partly regain its proper shape. "It might be supposed," observes Mr. Kane, who had large opportunities of watching the process, "that the operation would be attended with great suffering, but I never heard the infants crying or moaning, although I have seen their eyes seemingly starting out of the sockets from the great pressure; but, on the contrary, when the thongs were loosened and the pads removed, I have noticed them cry until they were replaced. From the apparent dulness of the children whilst under the pressure, I should

FIG. 8.



Deformed Skull of an Infant who had died during the process of flattening; from the Columbia River. (Mus. Roy. Coll. Surgeons.)

FIG. 9.



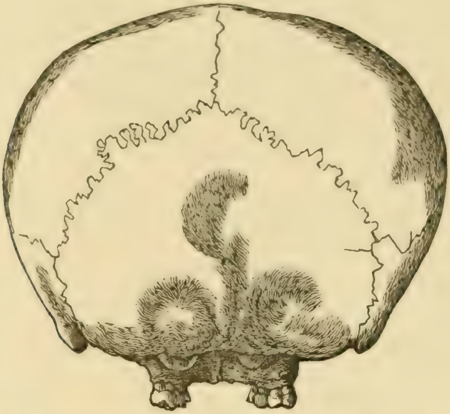
Artificially flattened Skull of ancient Peruvian. (Mus. Roy. Coll. Surgeons.)

imagine that a state of torpor or insensibility is induced, and that the return to consciousness occasioned by its removal must be naturally followed by a sense of pain."

Nearly, if not all, the different fashions in cranial deformity, observed in various parts of the world, are found associated within a very small compass in British Columbia and Washington Territory, each small tribe having often a particular method of its own. Many attempts have been made to classify these various deformities, but as

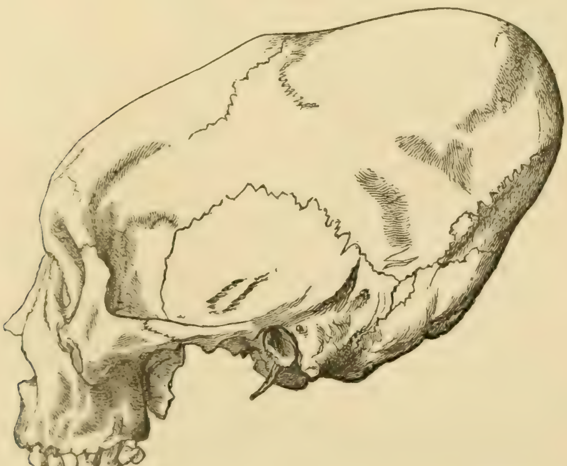
they mostly pass insensibly into one another, and vary according as the intention has been carried out with a greater or less degree of per-

Fig. 10.



Posterior view of Cranium, deformed according to the fashion of flattening, with compensatory lateral widening. (Mus. Roy. Coll. Surgeons.)

Fig. 11.



Cranium of Koskeemo Indian, Vancouver Island, deformed by circular constriction and elongation (Mus. Roy. Coll. Surgeons.)

severance and skill, it is not easy to do so. Besides the simple occipital and the simple frontal compressions, all the others may be grouped into two principal divisions. First (Figs 8 and 9), that in

which the skull is flattened between boards or other compressors, applied to the forehead and back of the head, and as there is no lateral pressure, it bulges out sideways to compensate for the shortening in the opposite direction. (Fig. 10.) This form is very often unsymmetrical, as the flattening boards, applied to a nearly spherical surface, naturally incline a little to one side or the other; and when this once commences, unless great care is used, it must increase until the very curious oblique flattening so common in these skulls is produced. This is the ordinary form of deformity among the Chinook Indians of the Columbia River, commonly called "Flat-heads." It is also most frequent among the Quichuas of Peru.

The second form of deformity (Figs. 7, 11, and 12) is produced by constricting bandages of deer's hide, or other similar material, encircling the head behind the ears, usually passing below the occiput behind, and across the forehead, and again across the vertex, behind the coronal suture, producing a circular depression. The result is an elongation of the head, but with no lateral bulging, and with no deviation from bilateral symmetry. This was the form adopted with trifling modifications by the *Macrocephali* of Herodotus, by the Aymara Indians of Peru, and by certain tribes, as the Koskeemos, of Vancouver Island. The "*déformation Toulousaine*" is a modification of the same form.

Fig. 12.

Posterior view of Cranium deformed according to the fashion of circular constriction and elongation. (Mus. Roy. Coll. Surgeons.)

The brain, of course, has had to accommodate itself to the altered shape of the osseous case which contained it; and the question naturally arises, whether the important functions belonging to this organ are in any way impaired or affected by its change of form. All observations upon the living Indians who have been subjected to it, concur in showing that if any modification in mental power is produced, it must be of a very inconsiderable kind, as no marked difference has been detected between them and the neighbouring tribes which have not adopted the fashion. Men whose heads have been deformed to an extraordinary extent, as Concomly, a Chinook chief, whose skull is preserved in the museum at Haslar Hospital, have often risen by their own abilities to considerable local eminence, and the fact that the relative social position of the chiefs, in whose families the heads are always deformed, and the slaves on whom it is never permitted, is constantly maintained, proves that the former evince no decided inferiority in intelligence or energy.

Although the American Indians, living a healthy life in their native wilds, and under physical conditions which cause all bodily lesions to occasion far less constitutional or local disturbance than is the case with people living under the artificial conditions, and the accumulated predisposition to disease which civilization entails, thus appear to suffer little, if at all, from this unnatural treatment, it seems to be otherwise with the French, on whom its effects have been watched by medical observers more closely than it can have been on the savages in America. "Dr. Foville proves, by positive and numerous facts, that the most constant and the most frequent effects of this deformation, though only carried to a small degree, are headaches, deafnesses, cerebral congestions, meningitis, cerebritis, and epilepsy ; that idiocy or madness often terminates this series of evils, and that the asylums for lunatics and imbeciles receive a large number of their inmates from among these unhappy people." * For this cause the French physicians have exerted all their influence, and with great success, to introduce a more rational system in the districts where the practice of compressing the heads of infants prevailed.

I will now pass from the head to the extremities, and shall have little to say about the hands, for the artificial deformities practised upon those members, are confined to chopping off one or more of the fingers, generally of the left hand, and usually not so much in obedience merely to fashion, as part of an initiatory ceremony, or an expiation or oblation to some superior, or to some departed person. Such practices are common among the American Indians, some tribes of Africans, the Australians, and Polynesians, especially those greatest of all slaves of ceremonial, the Fijians, where the amputation of fingers is demanded to appease an angry chieftain, or voluntarily performed on the occasion of the death of a relative as a token of affection.

On the other hand, the feet have suffered more, and altogether with more serious results to general health and comfort, from simple conformity to pernicious customs, than any other part of the body. But on this subject, instead of relating the unaccountable caprices of the savage, we have to speak only of people who have already advanced to a tolerably high grade of civilization, and to include all those who are at the present time foremost in the ranks of intellectual culture.

The most extreme instance of modification of the size and form of the foot in obedience to fashion, is the well-known case of the Chinese women, not entirely confined to the upper classes, but in some districts pervading all grades of society alike. The deformity is produced by applying tight bandages round the feet of the girls when about five years old. The process is an extremely painful one, and its results are not only an alteration in the relative position of the growing bones

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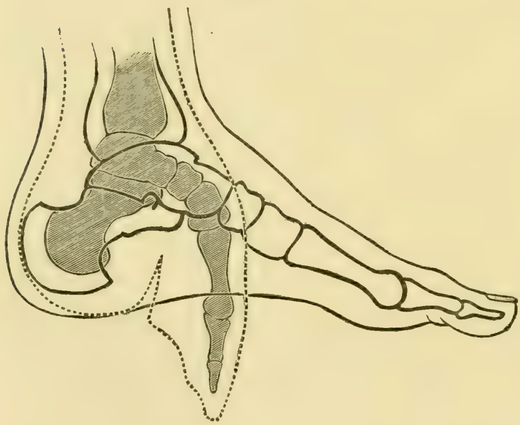
* Gosse, "Essai sur les Déformations artificielle du Crane," 'Annales d'Hygiène publique,' 2 ser. tom. iv. p. 8.

and other structures, but an arrest of their development, so that they remain permanently in a stunted or atrophied condition. The alterations of form consist in two distinct processes: 1, bending the four outer toes under the sole of the foot, so that the first or great toe alone retains its normal position, and a narrow point is produced in front; 2, compressing the roots of the toes and the heel downwards and towards one another so as greatly to shorten the foot, and produce a deep transverse fold in the middle of the sole (Fig. 14). The whole has now the appearance of the hoof of some animal rather than a human foot, and affords a very inefficient organ of support, as the peculiar tottering gait of those possessing it, clearly shows.

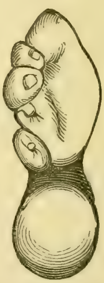
But strange as this custom seems to us, it is only a slight step in excess of what the majority of people in Europe subject themselves and their children to. From personal observation of a large number of feet of persons of all ages and of all classes of society in our own country, I do not hesitate to say that there are very few, if any, to be

Fig. 13.



Section of Natural Foot with the Bones, and a corresponding section of a Chinese Deformed Foot. The outline of the latter is dotted, and the bones shaded.

Fig. 14.



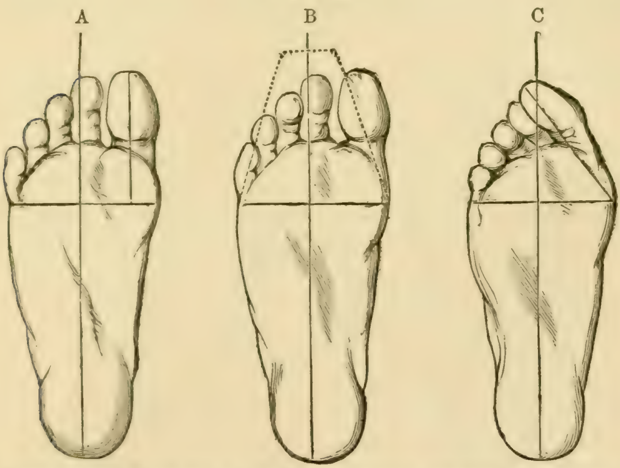
Sole of Chinese Woman's Foot.

met with that do not, in some degree, bear evidence of having been subjected to a compressing influence more or less injurious. Let anyone take the trouble to inquire into what a foot ought to be. For external form look at any of the antique models—the nude Hercules Farnese or the sandalled Apollo Belvidere; watch the beautiful freedom of motion in the wide-spreading toes of an infant; consider the wonderful mechanical contrivances for combining strength with mobility, firmness with flexibility; the numerous bones, articulations, ligaments; the great toe, with seven special muscles to give it that versatility of motion which was intended that it should possess—and

then see what a miserable, stiffened, distorted thing is this same foot, when it has been submitted for a number of years to the "improving" process to which our civilization condemns it. The toes all squeezed and flattened against each other; the great toe no longer in its normal position, but turned outwards, pressing so upon the others that one or more of them frequently has to find room for itself either above or under its fellows; the joints all rigid, the muscles atrophied and powerless; the finely formed arch broken down; everything which is beautiful and excellent in the human foot destroyed, to say nothing of the more serious evils which so generally follow—corns, bunions, in-growing nails, and all their attendant miseries.

Now, the cause of all this will be perfectly obvious to anyone who compares the form of the natural foot with the last upon which the shoemaker makes the covering for that foot. This, in the words of

Fig. 15.



A. Natural form of the sole of the Foot, the great toe parallel to the axis of the whole foot. B. The same, with outline of ordinary fashionable boot. C. The necessary modification of the form of the foot consequent upon wearing such a boot.

the late Mr. Dowie, "is shaped in front like a wedge, the thick part or instep rising in a ridge from the centre or middle toe, instead of the great toe, as in the foot, slanting off to both sides from the middle, terminating at each side and in front like a wedge; that for the inside or great toe being similar to that for the outside or little toe, as if the human foot had the great toe in the middle and a little toe at each side, like the foot of a goose!" The great error in all boots and shoes made upon the system now in vogue in all parts of the civilized world lies in this method of construction upon a principle of bilateral symmetry. A straight line drawn along the sole from the middle of

the toe to the heel will divide a fashionable boot into two equal and similar parts, a small allowance being made at the middle part, or "waist," for the difference between right and left foot. Whether the toe is made broad or narrow it is always equally inclined at the sides towards the middle line, whereas in the foot there is no such symmetry. The first or inner toe is much larger than either of the others, and its direction perfectly parallel with the long axis of the foot. The second toe may be a little larger than the first, as generally represented in Grecian art, but it is more frequently shorter; the other rapidly decrease in size (Fig. 15, A). The modification which must have taken place in the form of the foot and direction of the toes before such a boot can be worn with any approach to ease is shown at C. Often it will happen that the deformity has not advanced to so great an extent, but everyone who has had the opportunity of examining many feet, especially among the poorer class, must have met with many far worse. The two figured (Fig. 16), one (C) from a labouring working man, the other (A and B) from a working woman, both

Fig. 16.

A B C

English Feet deformed by wearing improperly-shaped shoes. From nature.

patients at a London hospital, are very ordinary examples of the European artificial deformity of the foot, and afford a good comparison with the Chinese. It not unfrequently happens that the dislocation of the great toe is carried so far that it becomes placed almost at a right angle to the long axis of the foot, lying across the roots of the other toes.

The changes that a foot has to undergo in order to adapt itself to the ordinary shape of a shoe could probably not be effected unless commenced at an early period, when it is young and capable of being gradually moulded into the required form. It seems perfectly marvellous that anyone who had ever looked at a healthy pair of human

feet could have thought of the possibility of wearing a stiff, unyielding shoe of identical form for both right and left, and yet the very trifling difference which is at present allowed is a comparatively modern innovation, and is even now too frequently disregarded, especially where most needed, as in the case of children.

The loss of elasticity and motion in the joints of the foot, as well as the wrong direction acquired by the great toe, are not mere theoretical evils, but are seriously detrimental to free and easy progression, and can only be compensated for in walking by a great expenditure of muscular power in other parts of the body, applied in a disadvantageous manner, and consequently productive of general weariness. The labouring men of this country, who from their childhood wear heavy, stiff, and badly-shaped boots, and in whom, consequently, the play of the ankle, feet, and toes is lost, have generally small and shapeless legs and wasted calves, and walk as if on stilts, with a swinging motion from the hips. Our infantry soldiers also suffer much in the same manner, the regulation boots in use in the service being exceedingly ill-adapted for the development of the feet. Much injury to the general health—the necessary consequence of any impediment to freedom of bodily exercise—must also be attributed to this cause. Since some of the leading shoemakers have ventured to deviate a little from the conventional shape, those persons who can afford to be specially fitted are better off as a rule than the majority of poorer people, who, although caring less for appearance, and being more dependent for their livelihood upon the physical welfare of their bodies, are obliged to wear ready-made shoes of the form that an inexorable custom has prescribed.

No sensible person can really suppose that there is anything in itself ugly, or even unsightly, in the form of a perfect human foot; and yet all attempts to construct shoes upon its model are constantly met with the objection that something extremely inelegant must be the result. It will perhaps be a form to which the eye is not quite accustomed; but we all know how extremely arbitrary is fashion in her dealings with our outward appearance, and how anything which has received her sanction is for the time considered elegant and tasteful, while a few years later it may come to be looked upon as positively ridiculous. That our eye would soon get used to admire a different shape, may be easily proved by anyone who will for a short time wear shoes constructed upon a more correct principle, when the prevailing pointed shoes, suggestive of cramped and atrophied toes, become positively painful to look upon.

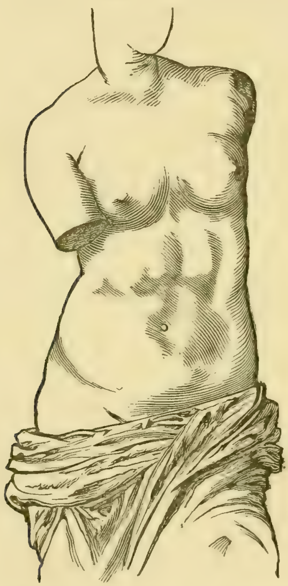
Only one thing is needed to aggravate the evil effect of a pointed toe, and that is the absurdly high and narrow heel so often seen now on ladies' boots, which throws the whole foot in an unnatural position in walking, produces diseases well known to all surgeons in large practice, and makes the nearest approach yet effected by any European nation to the Chinese custom which we generally speak of with surprise and reprobation. And yet this fashion appears just now on the increase

among people who boast of the highest civilization to which the world has yet attained.

But when, in spite of all the warnings of common sense and experience,* we continue to torture and deform our horses' mouths and necks, with tight bearing reins, as injurious, as useless, and as ugly, as any of these customs we practise on ourselves, and all for no better reason, we may well say with Dr. Johnson, "few enterprises are so hopeless as a contest with fashion."

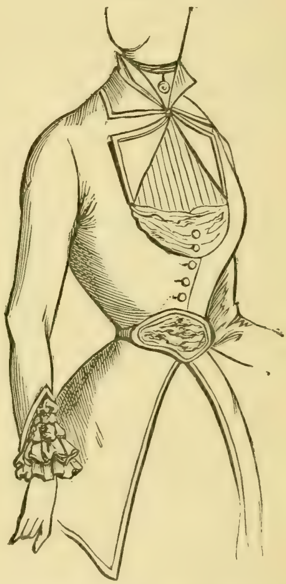
I must speak last upon one of the most remarkable of all the artificial deformities produced by adherence to a conventional standard, and one which comes very near home to many of us.

Fig. 17.



Torso of the Statue of Venus of Milo.

Fig. 18.



Paris Fashion, May, 1880.

It is no part of the object of the present discourse to give a medical disquisition upon the evils of tight-lacing, though much might be said of the extraordinary and permanent change of form and relative position produced by it, not only on the bony and cartilaginous framework of the chest, but also in the most important organs of life contained within it, changes far more serious in their effects than those of the Chinook's skull and brain, or the Chinese woman's foot. It is only necessary to compare these two figures (Figs. 17

* See 'Bits and Bearing Reins,' by Edward Fordham Flower. Cassell and Co., 1879.

and 18), one acknowledged by all the artistic and anatomical world to be a perfect example of the natural female form, to be convinced of the gravity of the structural changes that must have taken place in such a form, before it could be reduced so far as to occupy the space shown in the second figure, an exact copy of one of the models now held up for imitation in the fashionable world. The wonder is not that people suffer, but that they continue to live, under such conditions.

It is quite possible, or even probable, that some of us may think the latter the more beautiful of the two. If any should do so, let us pause to consider whether we are sure that our judgment is sound on the subject. Let us remember that to the Australian, the nose-peg is an admired ornament; that to the Thlinkeet, the Botocudo, and the Bongo negro, the lip dragged down by the heavy plug, and the ears distended by huge discs of wood, are things of beauty; that the Malay prefers teeth that are black to those of the most pearly whiteness, that the Western Indian despises the form of a head not flattened down like a pancake, or elongated like a sugar-loaf, and then let us carefully ask ourselves whether we are sure that in leaving nature as a standard of the beautiful, and adopting a purely conventional criterion, we are not falling into an error exactly similar to that of all these people whose tastes we are so ready to condemn.

The fact is, that in admiring such distorted forms as the constricted waist, and symmetrically pointed foot, we are simply putting ourselves on a level in point of taste with those Australians, Botocudos, and negroes. We are taking fashion and nothing better, higher, or truer for our guide; and after the various examples brought forward this evening, may I not well ask,

> "Seest thou not, what a deformed thief this fashion is?"
>
> "*Much Ado about Nothing,*" III. 3.

[W. H. F.]

WEEKLY EVENING MEETING,

Friday, May 14, 1880.

Warren De La Rue, Esq. M.A. D.C.L. F.R.S. Secretary and Vice-President, in the Chair.

Lord Reay.

Certain Aspects of Social Democracy in Germany.

The idea of socialism is to substitute collective capital and collective labour for individual capital and for individual labour. Individual labour and capital disappear. The results of this combination of capital and labour are put at the disposal of the individuals according to the measure of their labour. By these means it is contemplated to put a stop to the anarchy of competition. The division of labour is of course left to the settlement of public officers. Income from any other source than labour ceases to exist.

The object is said by its apostles to be no other than to correct the evils resulting from the present agglomeration of capital which has destroyed the small peasant and the small artificer, who were the real representatives of combined capital and labour. They could not stand against the power of a free and better organised association of capital and labour. They cannot be revived. But what *can* be done is to make the association which ruined them, not their antagonist, but their best friend by absorbing them into it. As the number of great capitalists grows smaller and smaller, and the number of those dependent on them larger, the hour draws near, when the great capitalist himself must become a unit in a larger combination of labourers. It will be easier to force a *few* great capitalists into the one great central productive and distributive co-operative association, than to deal with the existing number. It follows that the present tendency of concentrating capital in a few hands is most welcome to the socialists, because it will make their task lighter in having to deal in the future with a more limited number. Whether the actual owner of capital acquired it in his own person or in the person of a predecessor, through honest or dishonest means, is of no great consequence.

Socialists do not attack individuals, regarding their deeds as simply a result of the social organisation of which they form part; they attack that social organisation itself. In that existing organisation labour does not get its due, and capital gets more than its due. Wages are too low, profits too high. Labour always finds itself in

an inferior position to capital. New inventions, free trade, economic rent, enrich *capitalists*, but as a rule—so it is urged—do not improve the position of the working classes. On the other hand, if free trade, scientific discoveries, economic rent, cheap and abundant production, enrich everybody through the one great central institution, as started by social democracy, no injustice can possibly arise. The increase of wealth represents an increase of the comfort of all. In this wealth they have a share equivalent to their labour, but which can never make them independent of future labour.

The question in what way an indemnity should be given to the present owners of capital does not disturb the equanimity of socialists. These capitalists are for a certain number of years to get in goods an equivalent for their present property, but the process of increasing their capital by means of these goods is arrested. Means of enjoying life will, perhaps, be abundantly given to *several* generations of rich men, but the means of accumulating interest-bearing capital will be stopped. The descendants of the richest individual will sooner or later become workmen. Meanwhile the rich individual will have ample leisure to prepare his grandchildren and great grandchildren for the pleasant prospect of having to look to their own wits for their sustenance, and if they have no wits, to the wits of the State. Capital is no longer the property of any individual, it is the property of all.

Though nobody will be able to acquire any capital, it does not follow that nobody will be better off than his neighbour. For his labour he can get anything he wishes and which is to be found in the storehouses of the association, from which, of course, baneful articles will be excluded.* A difficulty arises in connection with certain professions—for instance, medical men—but this can be overcome. It would be unnecessary to oblige individuals to take another doctor or surgeon than the one they liked. His pay would not allow him to accumulate capital, because he would simply get his share of labour-checks as an equivalent for his visits, consultations, or operations.

Socialism, of course, has no room for credit in any form; the stock exchange, the mint, mortgages, shares, bonds, consols, all disappear. A person who does not want to settle his account against the State *at once*, can leave it as a claim against the community to be settled in future. This claim will not yield any interest. When the claimant requires payment he will only get it in goods. What inconvenience may arise in the case of perishable goods, I need not point out.

Labour may be unequal, and the retribution of labour also unequal, but the inequality is only an inequality of *temporary* ease and comfort. Trade and markets disappear as well as money. Labour and checks for labour are all-sufficient! Put the required pro-

* Herbert Spencer's books will certainly be nowhere, and what the fate of the great philosopher in a social democratic community would be, I do not attempt to describe.

duction, say, at three million hours of *bonâ-fide* labour, though the labour may really be performed in four million hours, and you have checks equivalent to one three-millionth part, and goods exchangeable for such a check.

The question naturally arises, what is to be done if—putting it in its extreme form—the demand on the part of the owners of these checks concentrates itself on, say, four or five kinds of produce, leaving other kinds untouched. One of two things must follow; either the value of that produce for which there is a greater demand must be increased, or the supply must be regulated, not according to the demand, but according to an estimate made by the officials who are in charge of the department of supply of goods. With the latter form, liberty of demand disappears, with the former, socialism comes down to the vulgarity of our present practice, and descends from the higher regulation latitudes into the lower latitudes of present economic anarchy of supply and demand.

The great difficulty, however, is in the distribution of labour. Socialism cannot afford to remunerate a skilled labourer for an hour's good work on the same scale as an unskilled labourer, who is practically wasting his time though he toils the whole day. It must, therefore, either marshal its labouring population and assign to each his work, or establish varying rates of payment on the basis of results. If the latter is done, the labourer retains his liberty, and will judge for himself whether he will pass from the field of labour in which he is engaged, to one in which wages rule higher and where the check of one three-millionth is more easily obtained than in another. The question is simply this: does socialism make it imperative to establish a code of labour enforcing production of certain goods in a certain manner? In that case a certain number of men would be told off to work a certain number of hours in the fields, another set of men would be ordered to work a given number of hours in a factory, another set of men would be obliged to carry goods from one place to another, and strict supervision would become necessary to distribute the checks in proper relation to the real work which had been turned out. Or does socialism adopt a scale of remuneration by labour-checks, leaving the labourer to be drawn by his spontaneous action and independent judgment into those channels to which the wants of the community seem to him to point? In the former case we should have universal vassalage, in the latter a situation not entirely at variance with that which exists.

The former, however, seems more in accordance with the object aimed at than the latter, and the more practical, if the object of the socialists is to make the quantity of labour the only test of value, and the only element in civilisation worthy of encouragement or even of notice, and its remuneration equal. Even then how it would be possible to tax the labour value of two pictures, or of two comic songs, or of two lectures, seems to baffle human ingenuity.

Karl Marx, by far the most eminent social democrat of the present

day, himself admits that there is a difference between the work of man and that of a bee. "Man," he says, "does not only transform matter, but he transforms his design, which he comprehends, which determines the mode and method of his action, as a law, to which his will must be subordinate." Putting this in a less abstract or Hegelian form, what Marx admits, is that skill is all important in human labour. Hence the difficulty of equal remuneration for that reason, also on account of another test: the usefulness of labour. The same article may be useful on the 1st of March, and perfectly useless on the 1st of April.

On the other hand, the extremely complicated question of taxation in our present condition, and certainly in the present condition of Germany, vanishes. As the Government of the world has in its own warehouses every possible product, from torpedoes to oatmeal, whatever the Government needs has simply to be taken out of them. It becomes only a matter of book-keeping. Whether the citizen should bear his proportion in this taxation relatively to his earned labour-checks, or whether it should be an absolute deduction, is an open but also a test question. If it is the duty of every citizen to the State to supply an equal amount of labour, there is no better way of punishing those who do not come up to the maximum than by deducting from their labour-checks the same amount that you deduct from those who have earned the maximum, leaving the latter with a larger share of what remained. To this question, therefore, we should have a clear answer, because it would spread a flood of light on the character of socialism. If the same amount of taxation is to be paid by every citizen, we have virtually the forced labour system of the old Indian community, with or without the lash.

The inheritance of the citizens is limited to labour-checks and means of enjoyment, but with these independent family life is perfectly conceivable. The question what kinds of production the State must undertake will be of the utmost importance on account of the impossibility, through the absence of private capital, of providing for individual needs, and any provision made by the public distributive department for needs, which are not general, must inevitably raise discontent. Supposing the State starts a Royal Institution, a hippodrome, an aquarium, or an organ, it is obvious that all the citizens who live far off and who do not want in exchange for labour-checks to be informed about Marcus Aurelius, to hear the State organ, to see the State gorilla, or to enjoy the feats of the State clown, will feel aggrieved. If it is left to a combination of private individuals to start hippodromes and music halls as private ventures, then these individuals are *pro tanto* withdrawn from the collective production of useful things to the production of useless things, and their support by the transfer of the labour-checks of their fellow citizens lessens in proportion the means of useful enjoyment of the community and the quantity of useful things produced by the State.

Another difficulty presents itself. If *private* individuals are

forbidden to be proprietors of the means of production, then what is to determine whether a thing is a means of production or an object of enjoyment? Take this instance. A citizen presents labour-checks enough to let him have a pony out of the State breeding-stud, and another presents a sufficient number of labour-checks to get a hansom. If they combine and ply for hire in their leisure hours, in exchange, of course, of labour-checks, the pony and the hansom cease to be a means of enjoyment of private individuals, and become a means of production, though not owned by the State. If inspectors are to be appointed to determine the limit between objects of enjoyment and means of production, and to keep people within the limits of enjoyment and out of the limits of production, I do not envy their task.

Socialism, it is clear from these remarks, is not Communism. Individual labour is remunerated, inheritance is not impossible, family life does not come to an end, education need not be moulded in one particular form, a division of goods does not take place, saving—though it has not the incentive, which the bearing of interest gives to it—is not hopeless. That improvidence would arise in such a community is evident, and—unless forced labour were adopted—it is not clear how it would be dealt with.

The great fundamental error of social democracy is that it constitutes society on the basis of acquisition *for society*, whereas human nature compels the individual to acquire *for himself*. Social democracy is therefore *Naturwidrig*, not consonant with human instincts.

According to the advanced evolutionists, carbon, hydrogen, oxygen, and nitrogen constitute a plastidule, the most minute independent living mass of protoplasm. The soul with which it is endowed is called "*die Plastidul Seele*," or protoplastic soul. Virchow very wittily speaks of Carbon and Co. as the "*Gründer*" or promoters of the protoplastic soul. "Gründer" being a term of reproach for the founders of unsound financial concerns, Virchow embraces in one common hatred Carbon and Co., and the socialists. With due regard for the vehemence of this learned sarcasm, I venture to doubt whether a *rapprochement* between social democracy and evolution can be established, as Virchow does. I do not venture, in these precincts sacred to science, to attack or to defend the theory of evolution, but whatever it is, it is essentially opposed to socialism. Out of original simplicity and unity evolution develops a multitude of phenomena, a complex organism; socialism wants to reduce this complex organism to abnormal simplicity. The contrast is even more striking, when we take the theory of natural selection. It starts from the inexorable competition of all living creatures, and then allows only the fittest to survive. The aristocracy—taking the word in its proper sense—instead of disappearing, are the only class who have a chance. Socialism starts from exactly opposite premises, and takes from the "fittest" their vitality, clips their wings. Whatever

evolutions the socialists may perform, they will not be of a scientific character. Their motto will probably be Stahl's: Science must again retrace its steps. Complete liberty would crush the weak, complete equality the strong; unscientific equality is therefore adopted instead of scientific liberty.

It is only because in Berlin the popular lines apply—

> "Wer die Wahrheit kennet und saget sie frei,
> Der kommt in Berlin auf die Stadt Vogtei"*—

that the *Neue Kreuz Zeitung* was able to make the theory of evolution responsible for the crimes of Hödel and Nobiling. Forsooth the danger is not that socialism may love, but rather that it will hate, science. Of all anti-socialist forces science is perhaps one of the strongest.

It is easier to dispose of the relations of socialism with evolution than of the harmony on some important points of socialists and of such men as the late Bishop von Ketteler, Moufang, and Joerg, of the German centre party; Rudolf Meyer, of the conservative party; and of the school represented by *Pastor* Todt, who has a weekly paper, called "*Der Staats Socialist* (*von einem Pietisten, einem Schutzzöllner und einem Agrarier gestiftet*)," and in which he writes: "The present struggle of competition is nothing but a system of expropriation veiled by illusions with regard to property."

The practical side of social democracy is represented by the Gotha programme of 1875, which adopts the following principles as those of the various sections of the party:—

I. Labour is the origin of all wealth and of all culture. The produce of the labour of society belongs to society; labour is universal; none are exempted; compensation is given to all concerned according to their merits. The emancipation of labour must be the task of the working classes, compared with whom all other classes are only a reactionary multitude.

II. Starting from these premises, the socialistic labour party of Germany works towards a free State and a socialistic community.

The socialistic labour party of Germany, though they at present work in a national spirit, are aware of the *international* character of the working class movement, and are determined to fulfil all the duties which it imposes on the working men to make the fraternity of all men a reality.†

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* "Who knows and proclaims the truth at Berlin is sure to get into prison."

† Lately the *Leipziger Volks Zeitung* published the following declaration addressed to it by some of the socialist leaders: "You publish: 'the Paris paper *Patrie* has received from one of its friends at Berlin who is in a position to be well informed, a communication, according to which the socialists in Berlin, Breslau, Leipzig, Hamburg, Munich, and Stuttgardt held meetings, to come to an exchange of ideas regarding the present situation in France. The socialists in Leipzig, Breslau, and Munich found the progress of socialism in France so remarkable, that an address was decided upon to the French brethren, of which

The party demands the institution of socialistic productive associations aided by the State under the democratic control of the working people. The associations are to be started so extensively for manufacturing and farming, that out of them will grow the socialistic organisation of collective labour.

The socialistic labour party of Germany claims as the essential conditions of the State:—

1. Universal direct compulsory suffrage.
2. Legislation directly by the people, decisions respecting peace and war by the people.
3. Duty to share in the general defence of the country. Army of the people, instead of a standing army.
4. Abolition of all exceptional laws, especially respecting the press, public meetings and associations.
5. Jurisdiction by the people. Unpaid.
6. General compulsory education by the State. Free in all its grades. Religion declared to be a private interest.

The immediate demands of the socialistic labour party of Germany, within the limits of society as at present constituted, are as follows:—

1. Extension of political rights and liberties in the direction of the preceding claims.
2. Only a progressive income-tax for the State and the parish, instead of all existing indirect taxes, especially of those which press on the people.
3. Unlimited right of coalition.
4. A normal day's labour in reference to the wants of society. Prohibition of Sunday labour.
5. Prohibition of children's labour and of all labour of women hurtful to their health and morality.
6. Laws protecting the life and health of labourers, sanitary inspection of labourers' dwellings. Control of mines, of factories, of workshops, of home industry by officials elected by the working men.
7. Regulation of prison labour.

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the fundamental idea was as follows: The German democrats can, alas! only utter good wishes for the final triumph of the social republic in France, but hope, that the French democracy after its victory will give active help to its foreign brethren who still sigh under the yoke, especially to the German proletariat; as soon as the social republic is constituted in France, it can only last if the whole of Europe is speedily rejoicing over the same blessings as France.' As Leipzig is also mentioned in this passage, we are compelled to declare in the name of the Leipzig socialists, that the whole report, as far as we know, is completely without foundation. We feel ourselves powerful enough to settle matters with our opponents, and do not want any extraneous assistance. And we do not believe that German social democracy counts a single member who is of a different opinion.—Leipzig, October the 22nd, 1879. A. Bebel, W. Liebknecht, F. W. Fritzsche, Wilh. Hasenclever."

8. Complete self-government for all savings banks, relief associations, friendly societies, and clubs.

These were the resolutions of the Congress of Gotha of May the 27th, 1875, where 25,000 working men were represented. Liebknecht and Geib represented one section, Hasenclever and Hasselmann another, in the previous meeting which had settled the terms of the compromise.

This manifesto contains the principles advocated by Marx, and evidently assumes that the history of the world is simply the history of a struggle between various classes, and that you have to upset the economic structure of society to get a better society and a different body politic. It may be well to note that Marx expressly declares that "the arm of criticism does not stand in the place of the criticism by arms. Material power must be upset by material power, but *theories* also become material power, when they sway the multitude. Theories are capable of getting hold of the multitude when they are demonstrated *ad hominem*, and they are demonstrated *ad hominem* when they become radical. To be radical is to get at the root of a matter. The root of man is man himself. The clear evidence in favour of the radicalism of the German theory, and therefore of its practical vitality, is that it starts from a distinct positive superior view of religion. The criticism of religion ends with the doctrine, that man is the highest being for man, and therefore with the categorical order to overthrow all relations in which man is a humbled, servile, abandoned, despicable being: relations which cannot be better described than by the exclamation of a Frenchman on hearing of a proposed tax on dogs: 'Poor dogs, you are going to be treated like men.'"

The best proof of the influence of socialism is in the fact that in Germany there are about twelve socialist members in Parliament, whereas in the American Congress, in the Danish, Dutch, and Belgian Parliaments, there is not a single professed socialist.

On the 6th of March, 1880, Dr. *Freiherr* von Hertling said in the *Reichstag:* "We were certainly surprised by the great results obtained at the late elections by social democrats, but I believe that we should go too far, if we took it for granted that all those who voted for social democrats were conscious adherents of all their doctrines." This does not mend matters. The return at the late election in Hamburg of an insignificant shoemaker, Hartmann, without any organisation of the party, without committee, without funds, by an overwhelming majority, can only be attributed to the fact, that at Hamburg, as at Frankfort-am-Main, social democracy has wealthy friends generously disposed towards it.

Wherever socialism lifts its head, Germans are its apostles. Eccarius, Becker, and Gögg, in Geneva (1873); Meyer and Höflicher accompany the bloody riots in New York; at St. Louis, Fischer and Kuhriem, who sends a telegram to Leipzig: "St. Louis, a town of 300,000 souls, is in our power!" In Switzerland the German cantons

contain the greater number of socialists; in Italy, of the Italians who understand German some are socialists. Why is this? The chief reason is, perhaps, the extraordinary development of learning in Germany and the onesidedness of German learning. Germany owes a great deal to its Universities, but its gratitude is assuming rather alarming proportions. Some Germans seem to think that all social evils can be cured by systems. When, therefore, notwithstanding *intellectual* superiority, Germany seemed to remain in a comparative condition of *material* inferiority, it was natural that the mass of Germans should begin to say: It is all very well that every provision is made for our brains, but what is the provision for our stomachs? What is the provision made for the weak against the oppression of the mighty capitalist—whether he be an individual promoter or a shareholder in a limited liability company? Here, of course, was a great opening for young men with generous dispositions. The question certainly was not a very easy one, but that made it all the more attractive. Instead of writing profound treatises for a very limited circle of learned readers, the temptation to write for a very large public was great.

The apotheosis of the State became the favourite theme. The English principle, that the individual should reap the full benefit of his own individual actions, work on his own responsibility, and not be hindered, but also not assisted in any way except by cheap education, is the exact counterpart of this apotheosis of the State, which may be called *militarism.* Whether the State absorbs the whole strength of the nation in the army, as in Germany, or absorbs it in water, as in Maine, by the liquor law, the principle is the same. It is a military mode of propagating opinions, and therefore an outrage on a fundamental principle of liberty, which is the toleration of error, or rather of what the majority for the time being entitles error. Socialism is the most logical application of these principles, which are not as uncongenial to the German mind as they are to ours.

Socialism would crush individual volition. It assumes, that individuals being left to their own devices, must come to grief, but acting collectively will escape from the miseries of this present life. This theory is tempting both to a privileged class afraid of losing its privileges, and to the "residuum," which sees in it the shortest cut to power. It ought to be resisted by the enlightened liberty-loving members of all classes. Unfortunately the German middle class is not enlightened politically. It has not yet realised the advantages of independence. It cannot grasp the fact, that it is the merit of representative institutions to keep the various interests in balance, allowing them to protect themselves from State interference instead of courting it. The parliamentary system is quite as hateful to the socialists as it is to oligarchs, because it is a check on supremacy and an inducement to controversy.

Oligarchy and socialism both enslave the human mind. Demo-

cracy introduced, where the Government is more or less confined to police duties, will not find a large scope for its powers of doing mischief, but, where it succeeds to a paternal Government, it is apt to assume parental authority on a dangerous scale, forgetting that it is the parent of children, a good number of whom are wiser than itself. Communities tending to democracy should therefore be extremely careful with what duties they charge the State. The smaller the inheritance, in this case, the less "damnosa" it will prove in the future. The great difficulty in making laws is to prevent these laws from going beyond their intended object.

The questions of Siéyès—What is the middle class? Nothing; What should it be? Everything;—are exaggerations, but certainly in a less dangerous direction than Lassalle's advice given in these words: "Take, friends, this pledge: if ever it comes to a struggle between the monarchy by divine right on the one side, and this miserable middle class on the other, then take your oath, that you will stand on the side of monarchy against the middle class. . . . from my youth I have been a republican, and notwithstanding, or perhaps exactly on that account, I have come to the conclusion, that nothing can have a greater future and a more blissful influence than royalty, if it can only decide to become 'Sociales Königthum.'" Bismarck said of Lassalle, of whom he had a very high opinion: "Whether the German empire would exactly culminate in the dynasty of Hohenzollern or in that of Lassalle was probably doubtful, but Lassalle's tendencies were certainly monarchical."

Napoleon expressed the idea very forcibly: "Given a triangle," he said; "one side represents the Church, the second side the army, the third side the people, and in the centre you have the middle class well fenced in."

In Germany the whole political system has been so framed as to check political independence and vigour, either in the higher or in the middle class. The ruling power in Germany is an exceedingly well-trained, highly organised bureaucracy (*bureaucratismus*). Self-government is hardly in its infancy; everything is done for the people. Bureaucratic initiative supersedes all parliamentary initiative. The socialists naturally, therefore, wish to lay hold of this bureaucracy; they must take possession for their own purposes of the existing machinery. Instead of increasing the power of Parliament, the extreme parties on either side in Germany weaken its control: the executive by its demands of arbitrary power, the social democrats by their dislike to joint action with the liberal party. Meanwhile the increase of activity of the Government, its assumption of more responsibility—as in the management of railways—constitutes a concession to the principles of social democracy and stimulates the revolutionary appetites. Of this "socialisme d'état" there are symptoms in France, where bills have been introduced by private members securing to the working classes either a pension at the close of life or a small capital to start with. Instead of abandoning protection and reducing all

taxation which interferes with the first necessities of the working classes, the circuitous process is maintained of taking with one hand and giving back with the other, the net result of which can only be the salary of a certain number of superfluous officials, besides the obvious damage done to trade.

These principles are found in the programme of May the 30th, 1873, of *Das Verein für Social Politik*, composed of the most learned political economists of Germany. They wish to avoid the stern individualism of the Manchester school, as well as the social revolution which would result from the monopoly of capital by the State. "We are of opinion," they say, "that the unlimited freedom of action of individual interests which are partly opposed to each other and are not of equal strength, does not guarantee the well-being of the *whole* community; that the exigencies of a common feeling of humanity must also influence economic conditions much more, and that the well-considered intervention of the State must be admitted in time to protect the just interests of all concerned. . . . In bringing this intervention to a serious issue, the egotism of the individual and the selfish wants of the various classes of the community will be made subject to the permanent and higher calling of the whole community." The first sentence is a protest against the Manchester school, the second against socialism.

Wagner's theory goes beyond this; he wants a compromise with socialism, the increase of collective at the expense of individual property. He would vest all property in land and houses of a town in some public authority. It is said that he went to Varzin, Prince Bismarck's seat, to expound a scheme by which the whole insurance business of the country would be undertaken by the State. Even von Sybel, who is a decided opponent of socialism, writes about wealth: "As such it has no value, it obtains it only by satisfying human wants; acquisition of property should only be an object in so far as it is the means of attaining higher ends, such as health and capacity for labour, enjoyment and power, intelligence and benevolence. Where the pursuit of wealth is in antagonism with these objects, there economic laws remain true, but they have to bow to higher laws, and human society, the State, is not only justified, but obliged to require this submission from each of its citizens, and in case of need, to use compulsion." Here we have von Sybel throwing on the State the duty of adjusting human society, and of drawing the line between thrift, a virtue, and niggardly shabbiness, a vice.

In what an absurdity this would land us. It is not the State which can or must enjoin the submission of human society to higher laws. This can only be done by the dictates of conscience and by the voluntary effort of each individual. That socialism considers the acquisition of wealth as the highest law is one of its most grievous errors, and must have a demoralising influence on its votaries. How is social democracy to be combated? By exceptional laws? By

establishing what the Germans call a state of dry siege? By compromise? Certainly not. Socialism is a fallacy.

The attitude of a Government towards any movement arising in the nation against its wishes, should always be guided by the thought that it is not brought face to face with an extraneous and a hostile force, but with its own fellow-citizens. Whatever measures are taken, evidently are taken by one part of the nation against another part. From the nature of things that part, however small a minority, is strengthened by any measures which savour of persecution. In the long run the family instinct which exists in nations will assert itself. A statesman who does not think of his own ends, but of the continuity and the ultimate good of the nation, will therefore do all he can to remain neutral in all social and religious struggles, unless they constitute a real peril to the very existence of society. The worst thing that can happen to an author or an artist is not violent criticism, but that his work should be ignored. This applies to parties and the action of Government towards them as well. Whatever it notices it magnifies. The licensed victualler unnoticed is nowhere, legislated upon or against he becomes an important factor; the clergy in the Rhenish provinces left alone were not very influential, but under the control of the authorities, they become popular heroes.

The socialist leaders are quite aware of this. They know that every term of imprisonment adds to the numbers of their followers, whatever disagreeable results this may have for themselves. They want a struggle, not victory. Victorious, they would quarrel over the spoils and immediately meet with resistance too strong for them. One of the best speeches *in favour* of the Repression Act, introduced in 1878 in the German Parliament, was delivered by the socialist, M. N. Hasselmann, though of course he voted *against* the bill. The socialists afford a great opportunity to any statesman who wishes to keep the nation in a feverish condition. All he has to do is to point to their nefarious doctrines. This is the result of what the socialists are pleased to call their "Idealismus" which, however, does not shrink from calling the execution of Hödel "murder," and the murder of General Mesensow an "irregular execution." The Act against the socialists which has just been renewed is to quell "die Untergrabung der bestehenden Staats- und Gesellschafts-Ordnung," and gives exceptional power to the police.

The Austrian Government is not placed in the same difficulty as the German Government, because Austrians do not care for theories unless they see pecuniary results. If in Saxony, where social democracy is strongest, those results were to follow, it is probable that social democracy would get a following in Austria. At present no special measures are enacted by the Austrian Government. It relies on the efficacy of a strict law for the expulsion of foreigners by the police, and the control of the right of public meeting. The police, who are always represented at these meetings, would at once close them in case anything revolutionary were taught. No

association can be formed which does not previously submit its rules for approval to the Government. The Austrian Government does not consider that there is any need for strengthening their hands by new laws.

In Germany intense alarm was created by the spread of a new creed which certainly did not scorn to adopt abstract tenets, which took good care to start from entirely new premises, which proclaimed everybody to have been deceived by everybody else, and which was hailed with a cordial welcome. The question it really opens is this: Can the German people be made prosperous at home without resorting to emigration? Can the resources of the country be developed? Can the fear be removed that, unless revolutionary means are used, Germany must remain a poor and unhappy country? The solution we should propose, I think, would be the same we gave to a similar crisis after Waterloo. Let trade, manufactures, and agriculture bestir themselves;—remove everything, in whatever shape or form, which hampers their development;—see how your taxation can be reduced. If this cannot be done without a European understanding or disarmament, then why not come to this understanding, why not depart from what Mirabeau called "l'industrie nationale de la Prusse la guerre."

The same burden which presses on Germany, presses on other States. Education is going on in these other States, and will sooner or later produce the same craving for comfort which it has produced in Germany. This result is inevitable. Education generates greater requirements. State demands made a century ago, and then appearing plausible, will now be severely criticised by those on whom they are made, and by whom they must be approved. If they are not approved, and yet continued all the same, they will prepare a soil on which any weeds may grow. The social democrats know this, and rejoice over all the mistakes which are made by their opponents, and, quite naturally, fear most of all real reformers. Enemies of individual capital, they cannot endure those who would make it possible for everybody to become more or less of a capitalist. Their propaganda thrives on the antagonism between wealth and the proletariat. A society composed of thrifty men, contented because they could enjoy quietly their savings; a society in which all dishonesty was rigorously ostracised, whenever it could not be treated as a criminal offence; a society where a certain amount of well-being was within the reach of all, would not yield them a considerable return of converts.

As long as the average wages of a German mechanic are not much more than half those of an English one, with a heavy income-tax affecting even the smallest incomes, house rent in the great cities enormously high, and the bread winner called away for three years' service at the age of twenty, you may annihilate the liberty of speech of social democracy, but you cannot prevent it from spreading. Processions will attend funerals of leaders: 12,000 persons at Breslau of Reinders; 20,000 to 40,000 persons at Hamburg of Geib; though it is

remarkable that no illegal outrage has been committed since the passing of the anti-socialistic Act. Five years ago half of the inhabitants of Germany were too poor to pay any direct tax; of those that remained, more than 400,000 had to be prosecuted, and of these 160,000 were simply found to be completely destitute of means.

It is not wonderful, then, that even young Government clerks fresh from the University are unconsciously using in State documents the jargon of State socialism, and that they inveigh against the spoils of private enterprise and individual egotism as if they were writing for the socialist press, and not inditing documents intended to suppress socialism. The German Government is placed in a delicate position. Their actions are scrutinised by minds filled with chimeras. From professorial chairs, from low platforms, the restoration of society is the never-ceasing theme. Practical ideas fall flat, and hard work, which must inevitably be the first duty of the greater part of mankind, becomes unattractive. A German workman forfeits his wages, convinced that eventually the "Volksstaat" will give him unprecedented comforts. Meanwhile he is dismissed by his employer and becomes cynically covetous. He does not want reform, which is the more just application of an existing principle, but revolution, which is the substitution of a new principle for an old one. He is not satisfied with *the same* privileges which the other citizens enjoy.

Germans have been accustomed to centuries of patriarchal government. That patriarch cannot suddenly abdicate. He should, however, surrender gradually his functions. The question of State interference is the great political question of the day. Where the State undertakes everything, as in the socialist State, society ceases to live, and becomes like a corpse. The rout of the Manchester school ("das Manchesterthum"), on which the *Nord Deutsche Zeitung* throws the blame of socialism, would be, on the contrary, the defeat of its bitterest enemy. In Germany, faith in organisation has superseded faith in liberty. Bismarck himself gave the explanation in his speech of May the 8th, 1880, when he said: "Thirty years I have fought for German unity"—mark: not liberty—though he also made the following remarkable statement: "I deem it necessary to state, that the enthusiasm for the principle of German unity is slightly weakened. Yes, gentlemen, weakened. Particularism has increased, the antagonism between parties is fiercer, the struggle of passions more violent. I am fully entitled to appear on this subject as a competent witness." Nobody here I suppose will dispute the gravity of these words.

We owe too much to German learning, to German science, to German literature, not to wish that united Germany should prosper. A regular action of the heart of Europe is all-important. But the unity of Germany must be accompanied by the free development of all German internal forces, to be safe from disruption. Let us hope that, after having travelled through the wilderness of protection and repression, Germany may enter on a smooth course of real liberty.

Rightly interpreted, this only means a higher efficiency of *all* the constituents of a people in their various functions, not of some only. Less activity in the Government means an increased activity of *all* citizens coöperating to further prosperity and culture. Lassalle said the great question of the day was simply "eine Magenfrage," a question of food supply, which the English free-traders—according to others—reduced to "eine Beutelfrage," a purse question. Germany is not suffering merely from either the one or the other; the whole condition of Germany is one of moral debility and intellectual nervousness. Common sense is the tonic required above all others; without it moral and intellectual remedies are of no use.

Thiers left the inheritance of social democracy to Germany. He also prophesied that "wicked" Frenchmen would be more beloved by the nations of the future world, than well-educated Englishmen. Perhaps, because as Novalis said: "Every Englishman is an island," and our institutions are made to fit the British Archipelago. The prospect for the nations of the future world—half of which is likely to speak English and to be imbued with English ideas—is probably that of a more luxuriant political vegetation than Thiers seemed willing to admit, if they adopt or hold to the present faith of Englishmen, that socialism is only another form of what Englishmen have always abhorred and will always repudiate: despotism.

[R.]

WEEKLY EVENING MEETING,

Friday, May 21, 1880.

The Duke of Northumberland, D.C.L. LL.D. President, in the Chair.

William Spottiswoode, Esq. D.C.L. M.A. LL.D. Pres.R.S. *M.R.I.* &c.

Electricity in Transitu.

The subject which I have proposed for this evening's discourse does not offer the wide perspective of modern investigation opened out by that of Professor Huxley, nor can it claim the manifold and varied sympathies evoked by the lectures which have followed on the succeeding Fridays. It belongs rather to the region of minute philosophy, and will on that account perhaps require more than usual patience and attention. Following the lines of a research on which Mr. Moulton and myself have been for some time engaged, I hope to extend by one or two steps our knowledge of the internal mechanism of that complicated, and still somewhat mysterious subject, the Electric Discharge. And in so doing I must leave aside, or at least only incidentally touch upon, many collateral points of interest which have presented themselves in our inquiry; because my main object will be ultimately to bring ourselves face to face with those important elements which we have called the small time-quantities of the phenomenon; that is, the times during which the different parts of the discharge are effected. These quantities are, however, so transient in duration, so evanescent in magnitude, that they elude all direct observation even with our most delicate instruments; and therefore, abandoning all attempts at absolute measurement, we have endeavoured, as it were, to lay wait for them as they pass, and, catching up any waif or stray indication that they may leave behind *in transitu*, to form such relative estimate of the quantities in question as may prove possible.

It is well known that when an electrical discharge is effected in air or other gas at atmospheric pressure, it passes in an irregular bright line or spark. If the discharge be made in a closed tube, and the tube be gradually exhausted, the discharge becomes thicker as the exhaustion proceeds, until it completely fills the tube with light. During the process of exhaustion, the discharge, when effected in a suitable manner, exhibits the phenomena of stratification in its various phases; while at the same time a very marked dissymmetry between

the positive and the negative ends of the discharge displays itself. This dissymmetry increases with the progress of the exhaustion.

But passing over, for the present, these features of our subject, I wish to draw your attention to a peculiar condition of the discharge which, having studied with much care, we have turned to account as our special method of research. It is as follows: If a continuous source of electricity, such as a Holtz machine, be used, and the terminals of the tube be connected with the main conductors, or poles, in the usual way, the discharge will pass through the tube in a condition which to all direct observation appears to be continuous in respect of time; although the researches of Mr. De La Rue, and of others, alike point to the conclusion that the discharge is in every case discontinuous and disruptive. In this condition the discharge is indifferent to the presence of a conductor, or even to that of a charged body, such as an electrophorous, or a Leyden jar. If the latter be brought near enough to discharge itself on to the tube, the luminous column will, it is true, exhibit a momentary flutter, but will show no other sign of susceptibility. This momentary flutter is, nevertheless, worthy of being noticed, as it will reappear at a later stage of our investigation.

If, however, one of the connections between the machine and the tube be broken by a small interval of air, over which the discharge must always leap in the form of a spark, the luminous column immediately becomes sensitive to the approach of a conductor. This break in connection, or air-spark, may be made either in the wire leading from the positive, or in that leading from the negative pole of the machine—in other words, in that leading to the positive or to the negative terminal of the tube; and it will be convenient to speak of these two dispositions as the positive air-spark and the negative air-spark arrangement respectively. When an air-spark is used the discharge will be described as intermittent; when it is not used it will be called continuous, although, having reference to a remark made above, the latter term can strictly be used only in a qualified sense.

This condition of sensitiveness is that which was mentioned above as having been the subject of our special study. The general fact of sensitiveness in an electrical discharge had been noticed by previous observers; but its connection with intermittence, and the laws which regulate it, do not appear hitherto to have attracted the attention which they deserve.

In order to examine this condition of the discharge, let us begin with a tube of moderate exhaustion, which presents a column of light from the positive terminal through the greater part of its length; then a blank space; and lastly a halo of light enveloping the negative terminal. If an air-spark be now introduced into the positive part of the circuit, the column will lengthen and approach the negative terminal, and it will at the same time contract laterally, and become narrower. If a conductor, such as the finger, be now made to approach the tube the column is repelled; and if the conductor be brought still

nearer, the column is severed into two, while from the point under that where the finger rests there issues a halo similar to that which surrounded the negative terminal before the air-spark was introduced. The explanation of this phenomenon is to be sought in the fact that the positive electricity coming from the machine accumulates at the air-spark interval until it has acquired sufficient tension to make the leap. It then passes *per saltum* into the tube, giving to the latter an instantaneous charge of the same name as that appertaining to the air-spark terminal. The conductor outside, through a redistribution of the electricity on its surface, is able to supply to the tube by induction the electricity which it needs, and forms a quasi terminal immediately within the point of contact. In the case in question, the quasi terminal is a negative one; the repulsion is the equivalent of the blank space, and the blue discharge that of the negative halo. On account of the fact that this inductive supply of electricity from the outside relieves the charge upon the tube due to the impulse from the air-spark terminal, we have called the effects in question the *relief effects.* They are by their very nature intermittent and co-periodic with the discharge whose needs are thus supplied.

The same is the case, *mutatis mutandis*, with a negative air-spark. The relief consists in a series of positive discharges, which are characterized by an attraction of the luminous column within the tube, and by the commencement of similar luminosity immediately within the point of contact. The effects with a negative air-spark are not so marked as those with a positive, for reasons to be mentioned hereafter.

It is further to be noticed that the completeness of these effects depends upon the capacity of the conductor. In the case of an earth connection, the capacity is infinite and the relief complete. If instead of the earth we take simply a reel of wire insulated bodily, then, when the wire is coiled up close to the tube, the relief which it affords is very small, viz. we find only moderate repulsion; but as the wire is uncoiled and led away at right angles to the tube, the capacity of the system increases, and the relief becomes more and more complete, viz. we have stronger repulsion, and when the relief is sufficient we have the blue discharge also.

If contact with the outside be made with a ring of tinfoil wrapped round the tube the effect will be more striking. In the case of negative relief, the positive impulses will start in the form of a hollow cone in the direction of the negative terminal, while the negative electricity left free beneath the tinfoil goes to meet and to satisfy the positive impulses arriving from the positive terminal; and in so doing, it truncates the positive column.

It will doubtless have occurred to some of my audience that, if the explanation of these effects be correct, it ought to be possible to imitate them by connecting a point on the outside of the tube with the opposite or non air-spark terminal, because we should then be supplying exactly what was wanted, viz. impulses of electricity of the

opposite name and coperiodic with those projected into the interior from the air-spark terminal. And this proves to be the case.

It may then be fairly asked, what will be the effect of leading to the outside of the tube impulses from the air-spark terminal itself; i. e. impulses coperiodic with those inside, but of the same name. These effects, which to distinguish them from the relief effects we have called *special*, are really what might have been anticipated; viz. special effects with a positive air-spark, or more briefly positive special effects are like relief effects with a negative air-spark, or negative relief effects, and *vice versâ*. This then completes the four possible combinations of air-spark with coperiodic inductive impulses, *ab extra*.

In all the cases of special effects it will be observed that the impulses conveyed by the wire outside have always arrived at the point of contact in time to produce their effect on the electricity advancing within the tube; in other words, that electricity, whether positive or negative, is conveyed along a conductor at least as quickly as along the gas. We shall in the sequel show reasons for thinking that it travels more quickly along a conductor.

Having exhausted the effects due to a single air-spark, we are naturally entitled to inquire what will be the effect if two air-sparks be used, the one positive the other negative. The experiment can be made with the same apparatus as that hitherto used, viz. a Holtz machine and an air-spark interval in each wire leading to the tube; but it is more easily effected with a small induction coil giving small but rapid impulses, which are equivalent to an air-spark at each terminal. In this case it will be found, on testing the tube by means of its relief or its special effects, that one half of the tube is charged positively, the other half negatively; and that between the two there is a neutral zone, showing no signs of charge whatever. By attaching a little condenser, e. g. a thunderplate, to one terminal or to the other, the impulses at that terminal become so toned down that the neutral zone is brought nearer to the attached terminal, in proportion to the capacity of the condenser. If either of the terminals be connected to earth, the neutral zone is brought close to the connected terminal; i. e. the tube is charged throughout with electricity of the opposite name.

It will have been noticed that through these processes, whether relief or special, whether positive or negative, when sufficiently energetic, the discharge is severed into two; although the exact configuration near the point of severance differs in the various cases. In other words, the tube has been divided into two parts, each of which presents the features of a complete discharge. Now, if this process be repeated at several points on the tube, the discharge will be subdivided into as many smaller but complete discharges as there are points of contact or of relief, the negative terminal itself being counted as one. These being sufficiently numerous, or at all events sufficiently near together, we have a complete artificial production of

the phenomena of striation. By this, as well as by many other experiments, we have been led to the conclusion that a stria with its attendant blank space is the physical unit of a striated discharge, and that a striated column is an aggregate of such unities formed by a step-by-step process, the general character of which is indicated in our intermittent discharges. To complete this view of the case, it would not be difficult to show, for we have made many experiments on the subject, that the so-called negative glow is merely a stria localized in position and modified in form by the solid terminal to which it is appended. We have on this account called it an anchored stria.

One of the most important consequences following from these experiments is that the discharges at the two terminals are in general independent of one another, excepting as regards the source from whence they come; and that each is primarily determined by the conditions at its own terminal, and only in a secondary degree, if at all, by the conditions which subsist at the opposite terminal. And since the discharges are not, at all stages of their entire duration (brief though it be), necessarily identical at both terminals, the tube will contain charges of free electricity at different times. A tube, therefore, during the passage of a discharge, is in no respect like a conductor, but is an independent electrical system, having an action very similar to that of an air vessel in a forcing pump.

This independence of action at each terminal may be illustrated by connecting only one of the conductors of the machine with one of the terminals of the tube, in which case a unipolar discharge will be seen to enter the tube; and unless it be strong enough of itself to reach the opposite terminal, or at all events within a range of it equal to a blank space, it will return and find exit by the way by which it came. By connecting the two terminals of the tube with one conductor of the machine, a double unipolar discharge will be produced, the two extremities of which will be found to be mutually repulsive. We have not now time to enter into the differences between positive and negative unipolar discharges; but it will be sufficient at present to remark that they each have, and maintain throughout their existence, the characteristics which belong to them respectively when they form portions of the complete discharge.

Thus far our attention has been mainly directed to the phenomena displayed by the column of luminosity connected with the positive terminal. The phenomena appertaining to the negative terminal are, however, not less important, as the beautiful experiments of Mr. Crookes have abundantly shown. But in order to study these negative phenomena with advantage we must carry our exhaustion, as he has done, to a much higher degree than in the tubes hitherto used. As the exhaustion proceeds, the positive column gradually shortens, and ultimately shrinks into insignificance, while the discharge from the negative, itself non-luminous, causes a continual projection of gaseous particles from the surface of the terminal, which impinge upon the glass with sufficient violence to cause phosphorescence. These,

although apparently no part of the discharge proper, as Mr. Crookes' experiments with a magnet, and others, seem clearly to show, are invariable accompaniments of it; and by means of them we may hope to learn something of the circumstances of the discharge in higher vacua. Now, although these molecular streams become more prominent as the exhaustion proceeds, and as the positive column sinks into insignificance, it is important to show that, in one form or another, they may be present in discharges at all pressures; and for this purpose it is necessary only to increase the violence of the discharge, by increasing the length of the air-spark employed. The effect is at once shown by the appearance of phosphorescence in the neighbourhood of the negative terminal, and by the relief discharge from the finger with a positive air-spark. But not only so, the phenomena of material streams issuing from the negative terminal are not confined to the molecules of gas, but are also exhibited by particles of finely-divided solid matter, such as lamp-black, when heaped over that terminal. With these, relief and special effects, analogous to those found in gaseous streams, may be shown.

Having thus launched ourselves into the region of high vacua, it is necessary to show that, notwithstanding the absence of the positive column, positive and negative air-sparks give rise to positive and negative charges on the tube, exactly as in lower vacua.

For this purpose, it will be best to make use of a second tube carrying a current, as a test, or, as we have called it, a standard tube. By connecting the outside of the tube to be tried with that of the standard tube, and by observing the effect of the one upon the other, we can immediately determine the nature of the discharge passing through the tube under examination. It is then found that, with a positive air-spark a positive discharge, and with a negative air-spark a negative discharge, passes from one end of the tube to the other, in high exactly as in low vacua.

In order to be quite clear as to the source of the molecular streams which cause the phosphorescence in the relief effects with a positive air-spark, the following should be mentioned:—If any solid object, such as a piece of wire, should be present in the tube below the point of contact, it will cast a shadow on the phosphorescence, precisely as in Crookes' experiments with the streams from the negative terminal. If there be two points of relief contact, the same object will throw two shadows, in directions conformable with radiations from each. To these, other experiments might be added.

A determination of the precise directions in which these molecular streams issue from a relieving surface is not a very simple problem; and we must here content ourselves with showing that, in the case of intermittent discharges at least, the streams do not issue normally. If a strip of tinfoil placed along the tube be used as a relieving surface, the phosphorescence takes the form of a sheet wrapped round the tube; if the strip be wrapped round the tube, the phosphorescence takes the form of a sheet laid along the tube. If contact be made

with the finger over a finite surface, or by a ring of wire laid close upon the tube, the phosphorescence takes the form, approximately, of the evolute of an ellipse. In all these cases the illumination is somewhat irregular; but the geometrical elements of which the phosphorescent figure is composed, and the stripes or striations of more intense light, are always formed at right angles to the longer dimension of the contact piece. This being so, suppose that we place on the tube a strip in such a curve that the normal planes to the curve will pass through the tangent at the corresponding point of the *image* of the curve, i. e. the curve on the opposite side of the tube, each point of which is exactly opposite to a point on the tinfoil. In such a case, all the striations will lie along the curve formed by the locus of the central patches of phosphorescence, and the result will be a single bright curved line of phosphorescence without any spreading out or striated margin. The curve fulfilling these conditions will be a helix, whose pitch is half a right angle. Experiment confirms the anticipation.

One more step in the study of these molecular streams is necessary for our present purpose, namely, an application to them of the same method which we have used with the electrical discharges themselves; viz. we must examine the effect of an inductive stream produced *ab extra* upon a direct stream due to the discharge inside the tube. These effects may be described generally as the *interference of molecular streams.*

If the finger be placed upon a highly exhausted tube through which a discharge with a positive air-spark is passing, the phosphorescence due to the molecular streams from the negative terminal is seen to fade away from the place where the finger rests, and from a region lying thence in the direction of the positive terminal. The effect is that of a shadow over that part of the tube; and as this is produced not by any real intervening object, but by an action from outside, we have termed it a *virtual shadow.* The phenomenon is due to a beating down of the streams of molecules coming from the negative terminal, by the transverse streams from the side of the tube immediately within the part touched.

The interference of two molecular streams may be further illustrated by a variety of experiments; and in particular by arranging within the tube a conductor of some recognizable form—say skeleton tetrahedron. If the tube be touched at a place opposite to this object, a shadow of the latter will be formed in the relief phosphorescence; but if the tube be touched also at a point on which the conductor rests, the shadow will be splayed out in a striking manner. This splaying or bulging of the shadow is due to the interference of the molecular streams issuing from the surface of the conductor, which then acts as a quasi negative terminal, with the original relief streams issuing from the first point of contact.

A still more striking instance of the interference of the molecular streams will occur if the tube be furnished with an intermediate

terminal in the form of a cone set transversely to the axis. If the tube be then touched at a point opposite to the cone, a patch of relief phosphorescence will be formed round the root of the terminal in question; but the patch will have a dark circular centre, due to the action of the cone as a shield against the molecular streams. If, however, the terminal itself be at the same time touched by a conductor, it also will shed molecular relief streams, which will interfere with those first mentioned, and will greatly increase the size of the circular dark patch.

We now come to the ultimate question that we have proposed for this evening, viz. the small time-quantities involved in the discharge. And, in the first place, it must be understood that the whole duration of the visible discharge is comprised within a period of which the most rapidly revolving mirror has been incompetent to give any account. It may be in the recollection of some of my audience that when the discharge from my great Induction coil was exhibited in this theatre with tubes on a revolving disk, the discharge showed a durational character as long as the coil alone was used; but as soon as a Leyden jar was introduced, which was in the main equivalent to an air-spark in a continuous current, the durational character disappeared, and nothing was visible but a bright line, the width of which depended, not upon the duration of the discharge, for no velocity of rotation in any way affected it, but only on the width of the slit through which the discharge in the tube was seen. But, notwithstanding the extreme rapidity with which the discharge is effected, our experiments have already shown that the spark or discharge is a complicated phenomenon, the various parts of which take place in a certain order or sequence of time; and that in virtue of this sequence we have succeeded, at the various pressures comprised within our range, in affecting and modifying it *in transitu*. This suggested the idea that, although the subject is surrounded with difficulties, it might still be possible to form some relative estimate, at all events, of the time occupied by the various parts of which the whole phenomenon is composed. And in fulfilment of this, the following are some of the conclusions to which we have been led.

The time occupied in the passage of electricity of either name along the tube is greater than that occupied in its passage along an equal length of wire.

This may be shown by connecting metallically a piece of tinfoil near the air-spark terminal with another near the distant terminal; for it is then seen that the former derives as much relief as if the latter were not on the tube. This shows (1) that at the time when the electric disturbance reached the nearer piece of tinfoil, the more distant piece was unaffected, and (2) that the disturbance propagated along the wire reached the second piece before the arrival of the same disturbance propagated within the tube.

The negative discharge occupies a period greater than that required by the particles composing the molecular streams to traverse the length of the tube, but comparable with it.

Proofs of this proposition are to be found in the phenomena of virtual shadows, and in other instances of the interference of molecular streams; but, omitting detailed experiments, the general argument on which the above conclusion is based, is as follows: If two molecular streams, one issuing with positive relief from the side of the tube, the other coming from the negative terminal, show signs of interference, it is clear that the former of these, which certainly started first, must have continued to flow, at all events, until the arrival of the latter.

The time occupied by the passage of electricity of either name along the tube is incomparably shorter than that occupied by the emission of the molecular streams, or (what is the same thing) the time occupied by the negative discharge.

In support of this conclusion, we have time only for a single experiment; and, although it is hardly adapted for lecture purposes, it is so curious and important that I will venture upon it, in the hope that it may be visible to at least some of the audience. If two pieces of tinfoil connected by a wire be placed, one near the negative, the other near the positive end of a tube through which a negative discharge with a rather long air-spark is passing, the former will show relief (positive) effects, the latter special (negative) effects; but no phosphorescence will be caused at the latter, however long the air-spark used. When the second patch is lifted off the tube and placed upon another through which no current is passing, phosphorescence is immediately produced. The explanation of this appears to be as follows: The negative electricity, bursting into the tube, summons all the positive which it can draw from the tinfoil. This is answered so promptly, that the second patch gives up to the first through the medium of the wire all the positive that it can yield, or, which is the same thing, draws off from the first all the negative that it can obtain; and this is done before the advancing negative reaches the distant patch. But so rapidly does the negative advance, that it reaches the distant patch before the molecular streams have had time to flow from the latter in a sufficient stream to produce phosphorescence; and it reaches it in time to *revoke* the supply of positive to the nearer, and to draw back the supply of negative which would have come to, and with it the molecular streams which would otherwise have flowed from the further patch. When the second patch is placed on an independent tube, where no such revocation is possible, phosphorescence actually appears, showing that the revocation is no mere supposition, but a real phenomenon.

From the last two laws, it follows as a consequence that *Negative electricity, and therefore also electricity of either name, in the tube outruns the molecular streams.*

These remarks will give, at all events, some idea of the conclusions to which the present method has led, and of the reasoning upon which those conclusions are based. But the issues of these time-quantities do not begin or end with the mere estimation of their relative magnitudes; they suggest questions about the time of formation of a positive

luminosity, or stria, and of a blank space—not necessarily identical, although correlative quantities; they suggest that the brilliancy of light, with so little attendant heat, may be due not only to the slight density of the medium, but also to its brevity of duration; they suggest that, for action of such rapidity as that of individual discharges, the mobility of the medium may count as nothing, and that for these infinitesimal periods of time gas may itself be as rigid and as brittle as glass. Time is an element in all mechanical action; and the converse of such brittleness is not unknown in experiments where substances, for all practical purposes hard and unconformable, have under the long-continued action of gravity, or of even moderate pressure, proved viscous and self-adapting in form.

"Quid magis est durum saxo, quid mollius undâ?
Dura tamen molli saxa cavantur aquâ."

[W. S.]

WEEKLY EVENING MEETING,

Friday, May 28, 1880.

WARREN DE LA RUE, Esq. M.A. D.C.L. F.R.S. Secretary and Vice-President, in the Chair.

FRANCIS HUEFFER, Esq.

Musical Criticism.

THE lecturer presumed his audience were in a certain sense musical critics, but he would not say they were good critics, for if they were, there would be no need for those persons who made musical criticism a profession, and undertook to tell the public what they should and what they should not like. There were many functions of criticism which they were infinitely better able to fulfil than any writer. Indeed, if the public only had courage to show what they thought of a singer, player, or composer, in spite of the reputation he might have established in foreign parts, a great many things would be impossible which now might be witnessed every day.

However much weight a criticism might have, judicious applause, or hissing, or significant silence was much more felt by performers. But in our moderate clime both censure and enthusiasm seldom exceeded certain limits. Foreign singers coming to this country never failed to praise our kindness, and from a sentimental point of view there was nothing more satisfactory. But audiences ought to remember that every time they applauded incompetency or mediocrity they insulted true merit. When foreign singers praised our kindness they perhaps often meant our ignorance.

Culpable leniency had led to the establishment of fixed customs, one of which was the *Encore nuisance.* At a ballad concert this mattered little, apart from the fact that most entertainments of that class were much too long without such repetitions. It was much less excusable to repeat a single movement of a sonata or a symphony, for that implied a want of reverence towards the composer. A sonata or symphony was an organism the component parts of which were carefully balanced by the writer to produce a harmonious impression. If one of the movements was repeated, this was naturally disturbed. The encore nuisance was even more insufferable in an opera. Mr. Hueffer mentioned striking instances of the impropriety of scenes being repeated and artists being recalled to the stage. Repetitions, he said, were unfair towards the performers, and if they knew their interest they would never comply with the request. It was well known that

singers had to study beforehand every gesture and every movement. Yet these had to appear as the spontaneous action of the moment called up by the inspiration of the moment, and without this illusion the dramatic effect was destroyed. All this ceased, however, with repetition, and we were let into the secret. The actors appeared no longer as free agents, but as marionettes pulled by strings.

The lecturer then proceeded to speak of the *Professional Critic*, saying that his task was that of an Interpreter as well as a Censor. In the former capacity he was the connecting link between the aspiration of the artist and the receptivity of the public. It might be supposed that the original inherent force of art would strike any one of itself. No doubt in its simplest form art would do so, but it was also a growth of ages and the result of many minds. Musical compositions, as well as literary, belonged to different periods, and contemporaries frequently failed to recognize genius. In all ages great composers met with exactly the same objections—one touch of Philistinism made the whole world kin. Here the sphere of the critic came in to herald genius and pave its way.

After referring to the musical critic's difficulty in making his ideas known, music not being reducible to words, Mr. Hueffer said a critic must not be too technical or too poetical. Schumann was instanced as one who hit a happy medium in his criticisms, and it was mentioned that he was one of the first to recognize the merits of Chopin, Berlioz, and William Sterndale Bennett. Writers of music were not, however, the best critics, and when Schumann became a great composer, and the head of a school, he lost much of his catholicity of judgment.

Many musicians spoke of their predecessors with scorn. A great original creator was necessarily a man of very marked stamp, and strongly impressed with his own idea, and therefore he had little sympathy with others of equally strong individuality.

The other office of the musical critic was that of Censor and General Monitor. That was a very disagreeable one, because the irritable race of musicians did not like to be censured. Critics, in fact, were held responsible not only for their own sins, but for the sins of their predecessors for five generations before them. Abuse levelled at Beethoven seventy years ago by some obscure scribe at Vienna or Leipsic was continually cited by dissatisfied young composers to show what musical criticism in general was worth. There were, of course, good and bad musical critics; those guilty of the abuse alluded to were no doubt bad ones, either intentionally perverse or hopelessly stupid. So at least one would think but for the curious fact that one of the most violent critics was Weber, the composer of 'Der Freischütz,' who bitterly attacked Beethoven. Weber, however, was a very young man at the time, and subsequently was ashamed of his own folly. Those who had judgment to discern and courage to declare new genius were almost as rare as that genius itself. But that there had been such men at all times was proved by the fact that the great composers became famous frequently during life, or at least

shortly afterwards, and not only in their own land, but far away, where only the press could carry their fame.

English critics were not ill-natured, but, on the contrary, like the non-professional, were much too lenient. One writer longed for synonyms for the word "charming," so often did he use it, and another prided himself on being able to write an entire column without committing himself to any opinion whatsoever. But critics should not speak like the connoisseur in Goldsmith, who said that a picture was good, but would have been better had the painter taken more pains. The critic, at all hazards, should speak decidedly. If artists thought themselves ill-used they could appeal to the supreme tribunal, the public. The public could, and should, applaud in spite of what they read in the newspapers if they thought there was unjust treatment.

Mr. Hueffer concluded by saying that there had been a great rise in musical taste of late in this country, caused, perhaps, by the efforts of conscientious writers who treated musical matters in the press. To improve matters further, and eradicate evils which still existed, lay with the public. They must study earnestly, and insist that those who spoke to them in print should speak competently and conscientiously. In that case English musical criticism would soon be what political criticism in English journals now was—the first in the world.

[F. H.]

WEEKLY EVENING MEETING,

Friday, June 4, 1880.

William Bowman, Esq. F.R.S. Vice-President, in the Chair.

H. Heathcote Statham, Esq.

Ornament.

Ornament may be defined as including all artistic design which is not of sufficient interest or expressive power to have independent value in itself, but which is added to some object to give to that object an interest or beauty which it would not otherwise possess. Thus the conventional foliage design which covers the Greek vase (Fig. 14 in illustrations) is ornament; but the delineations of human figures arranged in a continuous composition, which are often found on the body of such a Greek vase, cannot be classed as ornament; they are figure drawings upon a vase, but they possess sufficiently high artistic power and expressiveness to be of independent interest, upon whatever surface they might be drawn. Japanese trays and other objects often display beautiful drawings of birds and fishes, or grotesque attempts at landscape; but neither the birds, however beautiful, nor the landscapes, however preposterous, are ornament; they are pictures, for which the tray forms the groundwork and the frame.

Ornament, therefore, is not an independent, but a *relative* art; it is always an appendage to something else, something which could exist and could be of equal practical value without it, but to which it imparts an added grace and value. It is most important to bear in mind this relative condition of ornament in forming a true criticism of the art, since it is evident that, in accordance with this definition, ornament cannot be rightly judged of except in relation to the circumstances in which it is used, and its suitability to its position and to the uses of the object in connection with which it is found. And if we consider the nature of this relation of ornament to its circumstances, we shall find that we may broadly divide all ornament, in this respect, into two classes, which we may call respectively *surface ornament*, the object of which is to give interest to and diversify surfaces that would otherwise appear blank, and *functional ornament*, the object of which is to emphasize special features and assist in expressing their function or their relation to the whole. Thus the Arabic fret (Fig. 9) is a specimen of mere surface ornament, which might be carried over any extent of surface, merely to break it up and relieve it; while the vertical and horizontal flutings of the Ionic column

and its base (Fig. 13) are functional ornament, intended to emphasize respectively the verticality of the column and the horizontality of the base or bed-plate on which it rests, and to give a greater appearance of strength in each direction; and the flutings have no meaning, and hardly any beauty, except in connection with this functional expressiveness. The true office and value of functional ornament of this class may be illustrated by comparing this figure with the sketch of a fragment of a column (Fig. 18) preserved in Rome, where the column has been ornamented with carved foliage irregularly disposed over the surface, and not only adding nothing to the expression of strength in the column, but positively injuring this expression by producing an irregular and ragged outline in place of the strong clean line of the pure classic column. The man who did this probably thought he was doing a very picturesque and piquant thing, but in reality he was destroying all the sinew and muscle of the architecture, by placing ornament on it in such a way as to be only a falsity and an impertinence.

In regard to surface ornament, there is not so severe a logic to be observed; what is required is, that it should not in any way contradict or falsify the real nature of the surface to which it is applied, that it should be suitable to the material in which it is executed, and in most cases that it should have an obvious relation to the shape and extent of the surface which it occupies, and appear as if designed on purpose to fill that space. This latter demand may indeed be ignored in the case of simple repetition or diaper ornaments, which have little or no expressiveness in themselves, and merely serve to prevent the surface looking quite blank: such an ornament as Fig. 11, for instance, if used on a small scale, might reasonably be treated as a mere diversification of surface, and cut off by the bounding lines of the space without any special reference to its own configuration. But with surface ornament of a higher and more elaborate nature it is necessary to its satisfactory effect that it should appear to be designed for the place it occupies. In most Greek ornament this is the case: in the vase, Fig. 14, the repeated ornaments round the rim and the upper and lower part of the bowl may be regarded as to some extent functional, emphasizing the important parts of the construction of the object, but the foliage ornament is purely superficial, and while freely handled, it is at the same time carefully arranged so as to fill the space evenly, and the central line of the ornament is made coincident with the position of the handle, the space enclosed beneath the handle being specially filled by a leaf arranged to suit it. A Japanese artist would have drawn the foliage without any regard to the handle, and carried some of the leaves irregularly over it, as if by accident; and this sort of rule-of-thumb ornament is very much admired at present, its novelty and apparent piquancy having made it a fashion; but it is certainly inferior in logical and lasting interest to the Greek principle of ornamenting with direct and obvious reference to the space to be filled, or to the construction of the object

ornamented. The treatment of a Japanese plate indicated in Fig. 23, where the surface is irregularly divided into blue and white, and some sprigs are thrown on at one side, may rather be described as splashing a thing than ornamenting it; and still worse is the framework for a screen (Fig. 24), where foliage ornament is carried irregularly along the bars and over their angles from one face to another, straggling about quite independently of the form and construction of the object. And even our Greek friend seems to have missed a point in his vase, for the strongest as well as the most important point on the body of the vase is that where the handle springs from the surface, and this he has ignored in his ornament. If he had applied the same style of ornament somewhat as in Fig. 15, emphasizing the base of the handle by two or three strong lines, and causing the rest of the ornament to arise and develop from that point (leaving bare the part of the handle that is to be grasped, for there ornament would be misplaced), he would then have produced the same decorative effect in a manner that would at the same time have emphasized the most important feature on the surface of the vase, and would have caused the ornament to appear as manifestly intended for that special place and for no other. As it is, the base of the handle is the weak point in the design, whereas it ought to be the strong one.

When we pass from the question of the application of ornament to the consideration of the actual forms of ornament and their various characteristics, we shall find that all the immense variety of forms which have been used as ornament may be classified under two heads: what we may call *abstract ornament*, which is not an imitation of any object in art or nature, but which deals only with proportions and relations of lines and spaces, and *natural ornament*, which includes the use of forms more or less imitated from Nature. All ornament which is good may be classed under one or other of these heads; there is a third class, to be mentioned just now, but which is radically bad and may be left out of the question for the moment. Abstract ornament appeals mainly to what may be called our geometrical sense; to the pleasure which the eye derives from equal spacing and repetition, just as the ear derives pleasure from that equal spacing in time which we call "rhythm," and to the pleasure which both eye and mind derive from the play of line and the opposition of forms or spaces in compliance with geometrical proportion. A typical specimen of this class of ornament is the Greek fret, or, as it is sometimes called, "key-pattern" (Fig. 7), of which there are many varieties, from simple to exceedingly complicated forms. This is an example of the way in which interest may be given to surface ornament by a treatment which breaks up and evades the really simple basis of the ornament. This fret pattern is merely based upon squares drawn one within another, but the lines are broken off and reunited in such a way as to mask the real basis of the design, and cheat the eye by a kind of labyrinthine puzzle. The same kind of interest, that of presenting a certain puzzle to the eye and giving it a problem to trace out, belongs

to the more elaborate Arabic fret (Fig. 9), the basis of which is three hexagons drawn one within another, with an arrangement of squares one within another, connecting the faces of the hexagons. But the lines of the hexagons are so broken up that the inner line on one face runs into the second line on the next face, and the outer line on the next, and finally runs out of the hexagon and becomes part of the square design, thus producing an appearance of complication out of what is really a very simple decorative idea. This is the characteristic of all this class of Moorish decoration, which has perhaps been a little over-praised; it all consists in breaking up an essentially simple combination of lines in such a way as to present a puzzle to the eye; but when the trick of it is once mastered it rather loses its effect. Another type of ornament of which the interest is similar is the Celtic school of interlacing band ornament, of which Fig. 10 is a specimen; some of these are carried to an almost bewildering degree of elaboration. The Greek fret pattern has pervaded a great part of the world in one form or another: something like it is seen in the Egyptian specimen, Fig. 8; and in the British Museum is an old piece of Peruvian cloth in which the principle of the Greek fret is very well and rather elaborately carried out in a slightly different form.

These forms of ornament have obviously no relation whatever to nature in her outward aspect. Among the large class of ornamental forms which consist in the repetition of an object at equal distances, or the alternate repetition of two forms, we find a great many specimens which are equally artificial, and also a good many which, without imitating nature, seem to be taken from hints furnished by nature. We may perhaps trace in imagination the process by which such forms may possibly have been eliminated from a semi-natural origin. We might imagine, for example, that in a primitive stage of civilization the hut or wigwam might have been ornamented by some such natural objects as fir-cones, easily procurable, strung round the outside (A, Fig. 1). This would become a recognized and indispensable feature of a respectable wigwam, and would have so much impressed itself on the popular taste, that in a period of higher culture a conventional imitation of it (B) would be carved or painted round the dwelling, still preserving the general form of the natural object. The conventionalism of precise repetition and equal spacing might to some extent arise merely out of the fact that this mechanical repetition was easier of execution than the imitation of the variety of nature,* though the inherent love of rhythmical repetition would no doubt contribute to it. It would be an easy step to observe that greater

* There is probably a great deal of ancient art-work which we now call "conventionalized," and which we imitate, the so-called conventionalism of which arose from the imperfect attempt at realism. The figure drawing of mediæval stained glass is an example. It was probably the attempt on the part of the original artists to be as life-like as they possibly could, but in the modern mediæval revival its stiffness and imperfection have been regarded as positive beauties to be reproduced.

effect was gained by introducing a subordinate feature alternating with the principal one (C). A greater variety might next be aimed at by forming alternating groups instead of single forms, and the grouping of these would almost inevitably lead to a system of branching off on either side of a centre (D). This brings us to something not very far from the well-known Greek ornament shown in Fig. 2, which is sometimes called the honeysuckle ornament, but which in reality is probably no imitation of nature at all, but merely the natural principle of growth from a central stem systematically carried out in ornament. If we compare this painted ornament with the carved *antefixa* ornament of the cornice of a Greek temple (Fig. 3), it will be evident that both are designed on the same motive, but no one would think of calling the latter an imitation of nature. The principle of alternation of a principal and subordinate member, or of a long and round form, is met with everywhere in ornaments of repetition; Fig. 5 is an Egyptian specimen, Fig. 6 shows two forms of Greek ornament which have been employed perhaps more than any other ornamental detail, over the whole face of the civilized world, and of which the origin of the lower one at least is almost certainly artificial, and taken from personal ornament. Below it is a sketch of a bit of necklace from the Pelew Islands, which shows almost the exact form in little of the Greek " bead and reel " ornament.*

The Greeks, however, so completely conventionalized this and other ornaments, drawn originally, perhaps, from very prosaic sources, as to raise them to the rank of intellectually designed and studied ornament. There has been, however, a frequent use of artificial objects, merely copied and strung together to produce what is called ornament, and this is the third class of ornament referred to above, which is neither natural nor abstract, and which is always felt by a truly cultured taste to be bad and vulgar. For all true ornament is the application of thought and invention in the adaptation of natural form or natural law to the purposes of the decorator. But the imitation of mere artificial objects of use is the confession that the decorator who so uses them has no thought and no invention, and that natural law and natural form have less charm for him than the vulgar surroundings of his daily practical life. Accordingly, among the Greeks, who in their art were nothing if not critical, we hardly ever find the gross imitation of artificial objects; it only occurs in some subordinate work not of the best period. The Romans imposed upon the world, more than any other people, the vulgarity of what may be called furniture ornament. Their temples being places for the performance of sacrificial ritual, they thought it appropriate to ornament them externally with carvings of the head or skull of the

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* Some of the coincidences, it may be observed, between Greek ornament of the best school, and the productions of nearly barbarous people in far remote islands, are most curious, and would furnish in themselves a significant chapter in the history of ornament.

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animal that was sacrificed, and of the garlands with which he was decorated (Fig. 26), or even with the representation of the sacrificial implements themselves (Fig. 25). Such ornament represented the same thing to the Roman mind of the day which would be represented to our mind if the Law Courts were decorated with carvings of barristers' wigs spaced at equal distances and gowns festooned from one to another, or if the Board schools were decorated with a frieze of pens, inkstands, and spelling-books.* Such a decoration would at all events have a practical meaning to us, just as the representation of the garlands and the sacrificial implements had a practical meaning to the Roman public; so that if we adopted such suggestive ornaments on our buildings, we should at least be on the same ground as the Romans. But we have in fact fallen below them, for we imitate their bulls' heads and garlands without their having even any practical meaning for us: we reproduce the garland in stone, plaster, and terra cotta (Fig. 27: the modern builder calls it a "swag"), and place it all over our buildings without sense or meaning, because it had a meaning to the Romans. Vulgarity and absurdity could hardly be carried further.

It may be useful to note some other instances of misapplication of ornament in its relation to material and position. In surface ornament no design can be suitable which makes the surface look like what it is not. A flagrant instance of this is Fig. 12, from a Pompeiian mosaic floor, where the Greek fret is applied with a perspective treatment which causes it to appear as if in relief, and gives the impression that the visitor has to walk over a kind of gridiron. This sort of deception is bad in any position, but worst of all in a floor surface. Fig. 11 shows, by contrast, an Arabic design for brick pavement, not only in perfectly good taste for its position, but exactly suited to the material, and arising merely out of the studied arrangement of bricks of two or three different shapes and sizes. Figs. 19 and 20 are vases from the collection found by General Cisnola in Cyprus, of which Fig. 19 is suitably ornamented by circular rings following the natural movement of the vessel on its axis in the process of turning, while Fig. 20 shows an ornamentation by circles placed the other way merely for the sake of change, and in a manner which, instead of growing out of the process of manufacture, contradicts it. Fig. 21 is an example of the artistic effect that may be produced by merely fashioning an article in the most convenient method for its use and for the treatment of the material. It is one of the Hissarlik cups, intended to be held by both handles when used

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* Tennyson contributes a definition of this kind of ornament in his suggestion for decorating the tombstone of the "head-waiter at the Cock"—

"No carved crossbones, the types of death,
Shall show thee passed to Heaven,
But carved crosspipes, and underneath
A pint-pot, neatly graven."

for drinking from, and when not in use to stand inverted on its spreading rim, and its whole form precisely suggests this. Let some manufacturer, ambitious of novelty, place the handles as shown in Fig. 22, and the beauty of the thing is gone because its fitness is gone; the handles are in the way when using it, and it will not stand either way up. Take, again, the Japanese method of decorating a door, now so much in fashion, by painting on the panels a tree-form, which disappears under the framing and re-appears in the next panel (Fig. 30). This is an absolute contradiction of the facts of the construction of the door, in which each panel is a separate piece enclosed and held by the framing; whereas this way of decorating makes the framing appear as something laid over the whole and hiding part of the drawing. If it is considered piquant to treat the panels irregularly, at least each one should appear as a separate design (Fig. 29), and then they may be kept in their place (decoratively) by a simple treatment of the framing in lines following and emphasizing the lines of construction. In this case the line ornament is arranged so as exactly to denote the method of framing and the length of each piece, the side rails going right through to the top and the cross one being fixed between them; but this may be carrying "truth" a little farther than necessary. In any case, the panel decoration probably looks best when symmetrically arranged in relation to the centre of the panel (Fig. 31), rather than when treated irregularly; but fashion decides otherwise at present. Another example of the conflict of fashion and true taste is in the Chippendale chair (Fig. 33). This "ribbon-backed" chair was Chippendale's special pride; he said that he believed a better chair had never been made, and as far as the construction went he was probably right; and as far as design is concerned we may add that a worse one was never made. The festoons of ribbons in the back are utterly weak and unmeaning, and unsuited to the material and the position, and the scrolls which touch each other in the top rail and the legs present points of manifest weakness (in appearance) just where there ought to be strength. The chair by Sheraton (Fig. 32) is, though simple, a thoroughly well-designed one; the ornament is all applied so as to emphasize the lines of construction and give strength where it is required, the broad portion of the top rail is placed where it is needed for the back, the bend outwards of the foot is not only graceful in effect but operates in giving the chair a broader and firmer base. But both these chairs, though utterly different in principle and taste, are now offered and accepted indiscriminately as good furniture, merely because they both belong to a period the productions of which are in fashion at present. Lastly, it may be observed that no ornament ought to appear to contradict or ignore the laws of nature. We have a common style of ceiling ornament in the Queen Anne period, which is being much copied now, in which festoons seem to hang all round from a central ornament (Fig. 28). Now, as the festoon form is produced by the action of gravitation, which, as far as we are concerned, operates

vertically only, it is difficult to understand how flowers can hang horizontally in festoons every way, unless we suppose either that the centre ornament of the ceiling exercises a centrifugal force, or that the cornice of the room has powers of attraction. Such are the absurdities which follow the neglect of natural laws in the designing of ornament.

When we come to consider natural ornament, we are met by the further question, in addition to those which have been previously glanced at, what should be the relation of ornament founded on natural forms to nature herself; what degree of closeness of imitation of nature is possible or desirable in such ornament. If we look at the practice of former times, we find the Greeks usually treated natural forms in a highly conventionalized manner; if we compare the acanthus leaf of nature with that of the Corinthian capital (which is Greek in origin, though all the existing specimens of its complete form are probably Roman), we find the treatment of the leaf in marble so symmetrical and so sculpturesque that it almost becomes an invention of art rather than an imitation of nature. And in the use of natural leaves in other forms of ornament, as in the scroll (Fig. 16), the Greeks seem to have aimed not so much at imitating nature as at bringing natural forms into harmony with a very refined system of curves, such as are never found in natural growths: they thus to some extent combined the beauty of nature and that of geometric and mathematical proportion. Their curves are also constructed so as to proceed from one another in a strictly logical and harmonious manner, with which no vagary or variety of the natural foliage is ever allowed to interfere. This was far too refined a procedure for the Romans. The character of their scroll foliage work is indicated in such a fragment as Fig. 17: they adopted the Greek acanthus-leaf with great elaboration of surface and detail, and arranged great branches of it in irregular and broken curves, which somewhat resemble the form a real branch might take if we bent it into a scroll. At the same time the foliage is completely artificial, so that we have a confusion of principle, an artificial bough of foliage which is treated in a naturalistic manner, and which seems to demand in its nature a much severer treatment. The result is something which, in comparison with the purity of line and severity of style of the Greek foliage ornament, is heavy and cabbage-like in appearance. When we come to Gothic foliage ornament, we find a great deal, in early Gothic, that has strong affinity with Greek ornament: something approaching to, though not equalling, the Greek purity of line in scroll patterns, and something entirely equal to Greek work in the method of conventionalizing natural foliage and adapting it to ornamental design; the very difference between the two, the comparative roundness and massiveness of character in the Gothic ornamental foliage, being partly an illustration of its excellent adaptation to circumstances and material, since it is executed in coarse stone and in a dull climate, while the more refined Greek ornament was executed in marble and in a bright

climate. And this brings us to the considerations which seem really to govern and determine the relationship of natural ornament to the natural models from which it is derived, and which arise either from the position in which the ornament is applied or from the materials in which it is executed.

A few illustrations will elucidate this better than many words. Take, on the second page of illustrations, Fig. 34, a sketch of a sprig of a flower exactly as it grew (in the corner is represented the top view of the blossom the size of the original), and let us see how we shall have to shape that if we apply it in various different materials and methods. If we wish to paint it by hand in a panel (Fig. 35), there is then nothing to prevent us from making as good an imitation of the details and variety of nature as we can, only taking care to arrange the blossoms and leaves so as to be well distributed over the space, and to appear naturally to fill it. It is true that in some cases a better decorative effect might be produced by more conventionalism, but this depends upon other circumstances, and at all events the method of execution by hand leaves us perfectly free and unfettered in our treatment of our model, if we elect so to be: we are not restricted by any mechanical difficulties, nor by any inadequacy of material to produce precise imitation. But if we have to treat the same flower in a wall-paper, which is mechanically repeated in small sections, the attempt to give to the design the appearance of natural variety, as in Fig. 36, though it may look effective at a first glance, is liable to lose its effectiveness in our eyes when we find on closer examination that the same leaf is turned down, the same group of blossoms recurs at every 20 or 30 inches distance; it seems better in such a case to treat the flower more conventionally (Fig. 37), and to disavow any pretext of a naturalism which cannot be really sustained. If, on the other hand, we have to apply the flower as a border in needlework (Fig. 38), though we are obliged in this case to ignore some of the more delicate detail and gradation of tone, which this method cannot reproduce, we have again the freedom of handwork, and we may be at liberty to arrange the flowers and leaves along the border with all the irregularity of nature: since they must all be separately worked, and repetition is no economy in any way. But even in this case it is best to give some continuity to the ornament by continuous lines, and even by the symmetrical spacing of a smaller detail, in this case derived from the top view, or, as we may say, the "plan," of the blossom. If we have to treat it in inlay (Fig. 39), we are again free to employ any degree of variety, as far as convenience and economy of work are concerned, as each cutting is separately made; but here the effect of the process is so far removed from that of nature, the materials are so hard and unyielding in appearance, that it is best on other grounds to avoid any appearance of imitating nature, and to give to the work the symmetry and regularity of a completely artificial production. In doing this, however, we should keep in mind any slight peculiarity in the original model, and preserve a hint of it, in


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the adaptation; thus the characteristic little break in the stalk, at A, Fig. 34, is preserved in the inlay at A, and so is the fact that the two sides of the leaf do not spring from the base of its rib quite opposite each other, but one a little higher than the other. It is the preservation of such little incidents, even in highly conventionalized work, which gives character to an ornament derived from nature. But the attempt, sometimes made in very costly work, to imitate in inlaid stones and other such material the colours and natural irregularity and fragile appearance of flowers and other vegetation, is a mere *tour de force*, never really successful, only causing surprise that it can be accomplished in any degree. In such a material as needlework, however, even if symmetrical repetition be adopted, it is best to avoid bi-lateral repetition, because in such a material this can never be accomplished with entire success, and it leads to an impression that something has been attempted which is only imperfectly done. Such a pattern as that in Fig. 40 may be repeated with good effect, with alternate leaves and flowers of the same grouping; but there is no bi-lateral symmetry in it, and even the repetition of the groups will inevitably have a certain variety from the mere variations of the hand in working them.

The relation of conventionalism to nature may be further illustrated by another example. Take the nasturtium (Fig. 41), a flower which for some reason has been little used in ornament, though it is a very suggestive one. Part of the character of the blossom consists in the manner in which, when seen in front, each petal overlaps its neighbour on one side and is in turn overlapped on the other side, thus producing a partially spiral effect; and part of the character of the leaf consists in the radiation of the ribs from a point within the surface of the leaf, but not in its centre. In Fig. 42, which we may suppose a design for tiles, these characteristics are ignored, the flower is shown on one tile without the spiral growth, the leaf on the other with the ribs radiating from the centre; and the feature B (Fig. 41), which gives so much of the character to the side view of the flower, is in this design separated from the flower and introduced as an independent feature in the interstices of the circles.* All this is bad conventionalism, because it ignores the character and construction of the flower. In Fig. 43 these spiral and eccentric characteristics of blossom and leaf are preserved, and the appearance of the leaves collectively in nature, as discs overlapping each other, is suggested. The dark band round the blossom would be necessary to throw out its colour and give it the requisite force for tile design. The side view of the blossom balanced on its stalk is too light and fragile in appearance for tiles, but is introduced in the needlework border (Fig. 44); and in this and the last-named sketch the highly characteristic aspect of the

* This is no exaggeration of the kind of system pursued by some conventional designers, who seem to think the way to use flowers in ornament is to pull them to pieces and re-arrange them on a sort of Chinese puzzle principle.

leaf when viewed edgeways (C, Fig. 41) is introduced. In lace alone, perhaps, of all forms of decorative design, we may, as in Fig. 45, ignore symmetry and arrangement altogether, and admit all the irregularity of nature, the fragile material hardly bearing anything like precision or formality of design, which would seem to weight it too much.

Fig. 46 is a sketch of a spray of a foreign fern, *Adiantum trapeziforme* (the real character and beauty of which, however, cannot be shown on so small a scale and in mere outline). In its application as a wood inlay (Fig. 47), the trapeziform character of the leaves is an essential point, and also the zigzag of the stalk between the springing of each leaf, arising from the special form of "dichotomy" of growth in the plant; this is emphasized in the inlay by the additional lines on either side of it, which serve to fill in and give solidity to the ornament. This looks very stiff in comparison with the elegance of the natural spray, yet when applied as a border ornament, say to a table top, it would be much more effective as a whole than any realistic imitation of the spray. Nature sometimes, however, supplies us with a geometrical ornament ready made, as in the blossom of *kalmia* (Fig. 48), which is almost as precise in its symmetry as if set out with a pair of compasses. The eccentricities of nature furnish us with material for character also, as in the leaf of *begonia*, which is set so oddly sideways on the end of the stalk, and on which the leaf is based in Fig. 50, an ornament which is made from contiguous circles from each of which one segment is cut out and the tangent of the circle produced to meet the next circle; the same kind of way of evading the simple basis of the ornament which is found in the Greek fret before mentioned. Fig. 51 represents a bit of humorous design in nature, in which each leaf starts from the opposite side of the stem from that which it ultimately tends to, and each is torn off irregularly at the end; but if this were adapted for inlay (Fig. 52), it would not do to imitate the irregular termination of the leaf, we could only give a reminiscence of it in a regularly serrated border. The leaf Fig. 53 belongs to the same class as the last named, and is peculiar in its shape and character; it is shown as applied in Fig. 54 to a diaper pattern for stained glass, formed also on contiguous circles intercepted in various ways so as to give an appearance of intricacy, though following a fixed plan. This is an example of the same way of producing interest which is found in Arabic ornament—combining very simple elements of design so as to produce an appearance of elaboration and present a kind of problem to the eye.

These simple specimens may be taken as affording, of course, only some slight typical illustration of the philosophy of ornament and the relation in which it stands to natural forms, a subject which would offer almost endless variations for illustration if gone into in detail. One aspect of the subject may be touched upon in conclusion, which seems to connect it with the great modern all-pervading idea of evolution. For though we cannot historically trace back all the

forms of ornament to their origin, we can see enough to leave no doubt that if we had all the connecting links before us, we should find that many of the most admirable, widely used, and characteristic forms of ornament originated not so much in any sense of beauty, as in mere superstition and grossness; and that ornaments are habitually used in our churches, and public buildings, and habitations, the actual though remote origin of which, were it hinted at, would very much astonish those who execute and those who admire them; and it may perhaps be accepted as one more illustration of the upward tendency of human development, that even the very knowledge of this uncomely side of the subject has fallen away from all except those who have had special reason to study its history, and that from these clods of earthiness and superstition there has sprung this bright and innocent flower of ornament.

[H. H. S.]

GENERAL MONTHLY MEETING,

Monday, June 7, 1880.

George Busk, Esq. F.R.S. Treasurer and Vice-President, in the Chair.

Thomas Forster, Esq.
Charles Alexander Gordon, M.D.
Alexander Charles Macrae, M.D.
John Steel, Esq.
William Strang Steel, Esq.
Alfred Taylor, Esq.
Dr. Charles Meymott Tidy, F.C.S. F.I.C.

were elected Members of the Royal Institution.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

Governor General of India:—
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Palæontologia Indica; Series XIV. Vol. I. 1. fol. 1880.
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Memorie: Classe di Scienze Fisiche Matematiche e Naturali. Vols. III. IV. 4to. 1879.
Classe di Scienze Morale, Storiche e Filologiche. Vol. III. 4to. 1879.
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Transactions, No. 8, 9. 4to.
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Vol. I. Modern Scepticism.
Vol. II. Faith and Free Thought.
Vol. III. Credentials of Christianity.
Vol. IV. Popular Objections to Revealed Faith.
Vol. V. Striving for the Faith.
Vol. VI. La Vérité Chrétienne et le Doute Moderne.

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Manchester Geological Society—Transactions, Vol. XV. Parts 12, 13. 8vo. 1880.
Painter, R. Budd, M.D. F.R.C.S. M.R.I. (the Author)—Science, a Stronghold of Belief. 8vo. 1880.
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Mathematisch-Physische Classe:
Abhandlungen. Band XII. No. 4. 4to. 1876–8.
Berichte. 1879. 8vo.
Symons, G. J.—Monthly Meteorological Magazine, May, 1880. 8vo.
Tasmania, Royal Society—Papers and Proceedings for 1878. 8vo. 1879.
Telegraph Engineers, Society of—Journal, Part 32. 8vo. 1880.
Sir F. Ronald's Catalogue of Books relating to Electricity, Magnetism, &c. Ed. A. J. Frost. 8vo. 1880.
Tyndall, John, Esq. D.C.L. F.R.S. &c. (the Author)—Heat a Mode of Motion. Sixth Edition. 12mo. 1880.
United Service Institution, Royal—Journal, No. 104. 8vo. 1880.
Upsal University—Bulletin Mensuel de l'Observatoire Météorologique, Vol. XI. Nos. 7–12. 4to. 1879.
Verein zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, 1880: Heft, 4, 5.
Victoria Institute—Journal, Nos. 52, 53. 8vo. 1880.

Perigal, Henry, Esq.—Rotameter, a Kinematic Paradox. (*Apparatus.*)

GENERAL MONTHLY MEETING,

Monday, July 5, 1880.

George Busk, Esq. F.R.S. Treasurer and Vice-President, in the Chair.

Wilfrid H. Hudleston, Esq. M.A. F.G.S. F.C.S.
Richard Johnson, Esq. F.C.S.
Hamilton Owen Lindsay-Bucknall, Esq. Assoc. Inst. C.E.
Charles Hemsworth Linklater, Esq.
Claude Montefiore, Esq.
Stephen Winkworth, Esq.

were elected Members of the Royal Institution.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

Governor General of India:—Geological Survey of India. Records. Vol. XIII. Part 2.
Accademia dei Lincei, Reale, Roma—Atti, Serie Terza: Transunti: Tome IV. Fasc. 6. 4to. 1879.
Actuaries, Institute of—Journal, No. 119. 8vo. 1879.
Asiatic Society of Bengal—Proceedings, 1880. No. 1. 8vo.
Astronomical Society, Royal—Monthly Notices, Vol. XL. No. 7. 8vo. 1880.
Atkinson, Edmund, Esq. Ph.D.—'Jack Fuller, a Departed Friend to Science.' (Lithograph Portrait.) 1834.
Bankers, Institute of—Journal, Part 10. 8vo. 1880.
British Architects, Royal Institute of—1870–80: Proceedings, No. 16. Transactions, No. 10. 4to.
Chemical Society—Journal for June, 1880. 8vo.
Cornwall Polytechnic Society, Royal—Forty-seventh Annual Report, 1879. 8vo. 1880.
Crisp, Frank, Esq. LL.B. F.L.S. &c. M.R.I. (the Editor)—Journal of the Royal Microscopical Society, June, 1880. 8vo.
Editors—American Journal of Science for June, 1880. 8vo.
Analyst for June, 1880. 8vo.
Athenæum for June, 1880. 4to.
Chemical News for June, 1880. 4to.
Engineer for June, 1880. fol.
Horological Journal for June, 1880. 8vo.
Iron for June, 1880. 4to.
Journal of Applied Science for June, 1880. fol.
Nature for June, 1880. 4to.
Telegraphic Journal for June, 1880. 8vo.
Franklin Institute—Journal, No. 654. 8vo. 1880.

Frost, A. J. Esq. (the Author)—Memoir of Sir F. Ronalds, by A. J. Frost [with Ronalds' Catalogue]. 8vo. 1880.
Geographical Society, Royal—Proceedings, New Series. Vol. II. No. 6. 8vo. 1880.
Greig, J. K. Esq. (the Author)—Bank Note and Banking Reform. (K 103) 8vo. 1880.
Harlem, Société Hollandaise des Sciences—Archives Néerlandaises. Tome XV. Liv. 1, 2. 8vo. 1880.
Natuurkundige Verhandelingen. 3de Verz. Deel IV. Stuk 1. 4to. 1880.
Hayden, Dr. F. (the Author)—Eleventh Annual Report of the United States Geological and Geographical Survey of the Territories: Colorado, &c. 8vo. 1879.
Kershaw, S. W. Esq. F.S.A. (the Author)—Famous Kentish Houses. (K 103) 8vo. 1880.
Meteorological Office, The—Meteorological Observations at Stations of the Second Order for 1878. 4to. 1880.
Contributions to the Knowledge of the Meteorology of the Arctic Regions. Part II. 4to. 1880.
Perry, Rev. S. J. (the Author)—Stonyhurst Observatory: Results of Meteorological and Magnetical Observations: 1879. 16to. 1880.
Photographic Society—Journal, New Series, Vol. IV. No. 8. 8vo. 1880.
Preussische Akademie der Wissenschaften—Monatsberichte: Feb. 1880. 8vo.
Rowlatt, Rev. J. H. M.A. (the Author)—The Scripture Doctrine of Future Punishments. (K 103) 8vo. 1877.
Royal Society of London—Proceedings, No. 204. 8vo. 1880.
Catalogue of Scientific Papers, 1864–73, Vol. VIII. 4to. 1879.
Siemens, C. Wm. Esq. D.C.L. F.R.S. M.R.I. (the Author)—The Dynamo-Electric Current in its Application to Metallurgy, to Horticulture, and to Locomotion. 8vo. (Journal Soc. Tel. Engineers, 1880.)
Symons, G. J.—Monthly Meteorological Magazine, June, 1880. 8vo.
Teyler Museum—Archives, Vol. V. 2e Partie. 8vo. Haarlem, 1880.
United Service Institution, Royal—Journal, No. 105. 8vo. 1880.
Verein zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, 1880: Heft 6.
Victoria Institute—Journal, Nos. 52, 53. 8vo. 1880.
Vincent, B. Librarian R.I.—A. J. Warden: the Linen Trade, Ancient and Modern. 8vo. 1864.
Zoological Society—Proceedings, 1880. Part 1. 8vo. 1880.
Catalogue of the Library. 8vo. 1880.

GENERAL MONTHLY MEETING,

Monday, November 1, 1880.

George Busk, Esq. F.R.S. Treasurer and Vice-President, in the Chair.

Louis Eric Ames, Esq.

was elected a Member of the Royal Institution.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Lords of the Admiralty—Greenwich Spectroscopic and Photographic Results: 1878 and 1879. 4to. 1878–9.

The Governor General of India—Geological Survey of India:
Records. Vol. XIII. Part 3.
Memoirs: Vol. XV. Part 2. Vol. XVII. Parts 1, 2. 8vo. 1879–80.
Palæontologia Indica: Series X. Vol. I. Parts 4, 5. Series XIII. Part 2. fol. 1880.

The Secretary of State for India—Account of the Great Trigonometrical Survey of India. Vol. V. 4to. 1879.
Proceedings: 1879. No. 9, 1880. No. 1–6. 8vo.
The Cave Temples of India. By James Fergusson and James Burgess. 8vo. 1880.

The Meteorological Office—W. C. Ley, Aids to Study and Forecast of Weather. 8vo. 1880.

The French Government—Documents Inédits sur l'Histoire de France:
Lettres de Jean Chapelain. Ed. Ph. Tamizey de Larroque. Tome I. 1632–40. 4to. Paris, 1880.

Accademia dei Lincei, Reale, Roma—Atti, Serie Terza: Transunti: Tome IV. Fasc. 7. 4to. 1880.

Actuaries, Institute of—Journal, No. 120. 8vo. 1880.

American Academy of Arts and Sciences—Proceedings. Vol. XV. Part 1. 8vo. 1879.

American Philosophical Society—Catalogue of Library, Parts 1, 2. 8vo. 1878.
Proceedings, No. 105. 8vo. 1880.

Antiquaries, Society of—Proceedings, Second Series, Vol. VIII. No. 3. 8vo. 1880.

Asiatic Society of Bengal—Journal, Vol. XXXVIII. Part I. Extra No. 8vo. 1880. Vol. XLIX. Part I. No. 1. Part II. No. 1. 8vo. 1880.

Asiatic Society, Royal—Journal, New Series, Vol. XII. Parts 1–4. 8vo. 1880.

Astronomical Society, Royal—Monthly Notices, Vol. XL. No. 8. 8vo. 1880.

Bankers, Institute of—Journals, Parts 11, 12. 8vo. 1880.

Barlow, Charles, Esq. (the Author)—How to Make Money by Patents. (K 104) 8vo. 1880.

Batavia Observatory—Rainfall in the East Indian Archipelago, 1879. By Dr. P. A. Bergsma, the Director. 8vo. Batavia, 1880.

Bavarian Academy of Sciences, Royal—Sitzungsberichte, 1880. Hefte 2. 8vo.
Boston Society of Natural History—Memoirs, Vol. III. Part I. No. 3. 4to. 1879.
Proceedings, Vol. XX. Parts 2, 3. 8vo. 1878–80.
Occasional Papers: III. W. P. Crosby: Contributions to the Geology of Massachusetts. 8vo. 1880.
British Architects, Royal Institute of—1870–80: Proceedings, Nos. 17, 18. 1880–1. Nos. 1, 2. 4to.
Transactions, Nos. 11–13. 4to.
Cambridge University Press, the Syndics.—Professor G. G. Stokes. Mathematical and Physical Papers. Vol. I. 8vo. 1880.
Chemical Society—Journal for July–October, 1880. 8vo.
Civil Engineers' Institution—Minutes of Proceedings, Vols. LX. LXI. 8vo. 1880.
Corbet, John Dryden, Esq. (the Author)—Collected Poems. 2 vols. 12mo. 1877.
Crisp, Frank, Esq. LL.B. F.L.S. &c. M.R.I. (the Editor)—Journal of the Royal Microscopical Society, Vol. III. Nos. 4, 5. 8vo. 1880.
Dax: Société de Borda—Bulletins, 2e Série, Cinquième Année: Trimestre 3. 8vo. Dax, 1879.
Dublin Society, Royal—Transactions. Vol. I. Parts 1–12. Vol. II. Parts 1, 2. 4to. 1877–80.
Journal, Vol. VII. No. 45. 8vo. 1878.
Scientific Proceedings, Vol. I. Vol II. Parts 1–6. 8vo. 1877–80.
Editors—American Journal of Science for July–Oct. 1880. 8vo.
Analyst for July–Oct. 1880. 8vo.
Athenæum for July–Oct. 1880. 4to.
Chemical News for July–Oct. 1880. 4to.
Engineer for July–Oct. 1880. fol.
Horological Journal for July–Oct. 1880. 8vo.
Iron for July–Oct. 1880. 4to.
Journal of Applied Science for July–Oct. 1880. fol.
Nature for July–Oct. 1880. 4to.
Revue Scientifique and Revue Politique et Litteraire, Juli–Oct. 4to. 1880.
Telegraphic Journal for July–Oct. 1880. 8vo.
Franklin Institute—Journal, Nos. 655–9. 8vo. 1880.
Galton, Douglas, Esq. C.B. D.C.L. F.R.S. &c. (the Author)—Observations on the Construction of Healthy Dwellings. 8vo. 1880.
Geographical Society, Royal—Proceedings, New Series. Vols. II. Nos. 7–10. 8vo. 1880.
Geological Institute, Imperial, Vienna—Verhandlungen, 1880, Nos. 6–11. 8vo.
Jahrbuch: Band XXX. Nos. 2, 3. 8vo. 1880.
Geological Society—Quarterly Journal, No. 143. 8vo. 1880.
Harrison, W. H. Esq. (the Editor)—Psychic Facts from Various Authors. 16to. 1880.
Henry, Dr. James (Trustees of)—Æneidea, or Critical, Exegetical, and Æsthetical Remarks on the Æneis, by James Henry. Vol. II. (Book IV.) 8vo. 1879.
Leeds Philosophical and Literary Society—Annual Report, 1879. 8vo. 1880.
Linnean Society—Transactions, Second Series: Botany, Vol. I. Parts 8, 9. 4to. 1880.
Liverpool Polytechnic Society—Journal, various Nos. 8vo. 1880.
Lunacy Commissioners—Thirty-fourth Report. 8vo. 1879.
Madras Literary Society—Madras Journal of Literature and Science for 1879. 8vo. 1880.
Manchester Geological Society—Transactions, Vol. XV. Parts 14, 15. 8vo. 1880.
Mechanical Engineers, Institution of—Proceedings, April, 1880. 8vo.
Medical and Chirurgical Society, Royal—Proceedings, No. 51. 8vo. 1880.
Additions to Library, 1879–80. 8vo. 1880.
Meteorological Society—Quarterly Journal, Nos. 34, 35. 8vo. 1880.
Middle Temple, Hon. Society of—Catalogue of the Printed Books in the Library. 8vo. 1880.
Midland Institute of Engineers—Transactions, Vol. VII. Part 50. 8vo. 1880.

Morris, H. S. M.D. M.R.I.—H. S. Edwards: The Russians at Home and the Russians Abroad. 2 vols. 12mo. 1879.
Musical Association—Proceedings, Sixth Session, 1879–80. 8vo. 1880.
National Association for the Promotion of Social Science—Transactions: Manchester Meeting, 1879. 8vo. 1880.
Norfolk and Norwich Naturalists' Society—Transactions, Vol. III. Part 1. 8vo. 1879–80.
Pharmaceutical Society—Journal, July–Oct. 1880. 8vo.
Photographic Society—Journal, New Series, Vol. V. No. 1. 8vo. 1880.
Physical Society of London—Proceedings, Vol. III. Part 4. 8vo. 1880.
Preussische Akademie der Wissenschaften—Monatsberichte: Marz–Juni, 1880. 8vo.
Royal College of Surgeons of England—Calendar, 1880. 8vo.
Royal Irish Academy—Transactions: Vol. XXVI. Science, No. 22. 4to. 1879.
„ XXVII. Polite Literature, No. 3. 1879.
Proceedings, Series II. Vol. II. No. 1. Vol. III. No. 4. 8vo. 1875–9.
Irish Manuscript Series, Vol. I. Part 1. 4to. 1880.
"Cunningham Memoirs," No. 1. 4to. 1880.
Royal Society of London—Proceedings, Nos. 205, 206. 8vo. 1880.
Royal Society of New South Wales—Journal of Proceedings, Vol. XII. 8vo. 1879.
St. Bartholomew's Hospital—Statistical Tables for 1879. 8vo. 1880.
St. Pétersbourg, Académie des Sciences—Mémoires, Tome XXVII. Nos. 2, 3, 4. 4to. 1879.
Sandys, R. Hill, Esq. M.A. (the Author)—In the Beginning: Remarks on Certain Modern Views of Creation. 2nd Edition. 16to. 1880.
Sanitary Institute of Great Britain—Transactions, Vol. I. 8vo. 1880.
Schäfer, E. A. Esq. (the Author)—Some Teachings of Development. (K 103) 8vo. 1880.
Smithsonian Institution, Washington—Annual Report for 1878. 8vo. 1879.
Smithsonian Miscellaneous Collections, Vols. XVI. XVII. 8vo. 1880.
Smithsonian Contributions to Knowledge, Vol. XXII. 4to. 1880.
Society of Arts—Journal for Feb. 1880. 8vo.
Squire, Peter, Esq. F.L.S. M.R.I. (the Author)—Companion to the Latest Edition of the British Pharmacopœia. 12th Edition. 8vo. 1880.
Statistical Society—Journal, Vol. XLIII. Parts 2, 3. 8vo. 1880.
Symons, G. J.—Monthly Meteorological Magazine, July–Oct. 1880. 8vo.
Telegraph Engineers, Society of—Journal, Part 33. 8vo. 1880.
Tokio University, Japan—Memoirs of the Science Department, Vol. III. Part I. Meteorology of Tokio. By T. C. Mendenhall. 4to. Tokio, 1880.
Trafford, F. C.—Souvenir de l'Amphiorama. (K 104) 8vo. 1880.
United Service Institution, Royal—Journal, No. 106. 8vo. 1880.
Upsal, Société Royal des Sciences—Nova Acta, Series III. Vol. X. Fasc. 2. 4to. 1879.
Bulletin Météorologique Mensuel de l'Observatoire Météorologique. Vols. VIII. IX. 4to. 1877–8.
Verein zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, 1880: Heft 7, 8.
Victoria Institute—Journal, No. 54, 55. 8vo. 1880.
Bishop H. Cotterell on the Relation of Science and Religion. 8vo. 1880.
Thomas Wardle, Esq. F.C.S. F.G.S. &c. (the Author)—Monographs of the Wild Silks of India. 8vo. 1878.
The Wild Silks of India, principally Tusser. (L 18) 8vo. 1879.
Yorkshire Archæological and Topographical Association—Journal, Part 22. 8vo. 1880.
Zoological Society—Proceedings, 1880. Parts 2, 3. 8vo. 1880.
Transactions, Vol. XI. Part 2. 4to. 1880.

GENERAL MONTHLY MEETING,

Monday, December 6, 1880.

William Bowman, Esq. F.R.S. Vice-President, in the Chair.

William Henry Bennett, Esq. F.R.C.S.E.
Mrs. Sarah Sophia Butler,
Edwin Cutler, Esq.
Frederick James Mirrilies, Esq.

were elected Members of the Royal Institution.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Lords of the Admiralty—Greenwich Observations for 1878. 4to. 1880.
Nautical Almanac for 1884. 8vo. 1880.
The Governor General of India—Geological Survey of India:
Records. Vol. XIII. Part 4.
The Secretary of State for India—Account of the Great Trigonometrical Survey of India. Vol. I. 4to. 1870.
R. Sewell, Report on the Amarávati Tope and the Excavations on its site in 1877. 4to. 1880.

Agricultural Society of England, Royal—Journal: Second Series. Vol. XVI. Part 2. 8vo. 1880.
Astronomical Society, Royal—Monthly Notices, Vol. XL. No. 9. 8vo. 1880.
Bankers, Institute of—Journal, Parts 11, 12. 8vo. 1880.
Batavia Observatory—Magnetical and Meteorological Observations. By Dr. P. A. Bergsma, the Director. 8vo. Batavia, 1880.
British Architects, Royal Institute of—Proceedings, 1880–1. Nos. 3, 4, 5. 4to.
Chemical Society—Journal for Nov. 1880. 8vo.
Civil Engineers' Institution—Minutes of Proceedings, Vol. LXII. 8vo. 1880.
Clinical Society—Transactions. Vol. XIII. 8vo. 1880.
Devonshire Association for the Advancement of Science, Literature and Art—Report and Transactions. Vol. XII. 8vo. 1880.
Editors—American Journal of Science for Nov. 1880. 8vo.
Analyst for Nov. 1880. 8vo.
Athenæum for Nov. 1880. 4to.
Chemical News for Nov. 1880. 4to.
Engineer for Nov. 1880. fol.
Horological Journal for Nov. 1880. 8vo.
Iron for Nov. 1880. 4to.
Journal of Applied Science for Nov. 1880. fol.
Nature for Nov. 1880. 4to.
Revue Scientifique and Revue Politique et Litteraire, Nov. 4to. 1880.
Telegraphic Journal for Nov. 1880. 8vo.
Franklin Institute—Journal, No. 659. 8vo. 1880.
Geographical Society, Royal—Proceedings, New Series. Vol. II. No. 11. 8vo. 1880.
Journal. Vol. XLIX. 8vo. 1880.
Geological Society—Quarterly Journal, No. 144. 8vo. 1880.

Kerslake, Thomas, Esq. (the Author)—The Word "Metropolis," &c. (O 17) 12mo. 1880.
Liverpool Polytechnic Society—Journal: Nov. 1880. 8vo.
Manchester Geological Society—Transactions, Vol. XV. Parts 16, 17, 18. Vol. XVI. Part 1. 8vo. 1880.
Manchester Literary and Philosophical Society—Memoirs: Third Series. Vol. VI. 8vo. 1879.
Proceedings. Vols. XVI. XVII. XVIII. XIX. 8vo. 1877–80.
Medical and Chirurgical Society, Royal—Medico-Chirurgical Transactions. Vol. LXIII. 8vo. 1880.
Newcastle-upon-Tyne Free Libraries—Catalogue. 8vo. 1880.
North of England Institute of Engineers—Transactions. Vol. XXIX. 8vo. 1880.
Pharmaceutical Society of Great Britain—Jacob Bell and Theophilus Redwood: Historical Sketch of the Progress of Pharmacy in Great Britain. 8vo. 1880.
Journal, Nov. 1880. 8vo.
Photographic Society—Journal, New Series, Vol. V. No. 2. 8vo. 1880.
Preussische Akademie der Wissenschaften—Monatsberichte: Juli, 1880. 8vo.
Siemens, C. William, Esq. D.C.L. F.R.S. M.R.I. (the Author)—The Smoke Question. (K 104) 8vo. 1880.
St. Pétersbourg, Académie des Sciences—Bulletins, Tome XXVI. No. 3. 4to. 1879.
Mémoires: Série VII. Tome XXVII. Nos. 5–12. 4to. 1879–80.
Swan, J. W. Esq. (the Author)—Lecture on Electric Lighting. (K 104) 8vo. 1880.
Symons, G. J.—Monthly Meteorological Magazine, Nov. 1880. 8vo.
Telegraph Engineers, Society of—Journal, Part 33. 8vo. 1880.
Tokio University, Japan—Memoirs of the Science Department, Vol. I. Part I. Vol. II. 4to. Tokio, 1879.
United Service Institution, Royal—Journal, No. 107. 8vo. 1880.
Verein zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, 1880: Heft 9.

The following Lecture Arrangements were announced:

CHRISTMAS LECTURES.

PROFESSOR DEWAR, M.A. F.R.S.—Six Lectures (adapted to a Juvenile Auditory) on ATOMS; on Dec. 28 (Tuesday), Dec. 30, 1880; Jan. 1, 4, 6, 8, 1881.

BEFORE EASTER, 1881.

PROFESSOR EDWARD A. SCHÄFER, F.R.S. Fullerian Professor of Physiology, R.I.—Twelve Lectures on THE BLOOD; on Tuesdays, Jan. 18 to April 5.

FRANCIS HUEFFER, Esq.—Four Lectures on THE TROUBADOURS; on Thursdays, Jan. 20 to Feb. 10.

PROFESSOR ERNST PAUER.—Two Lectures on THE HISTORY OF DRAWING-ROOM MUSIC; on Thursdays, Feb. 17 and 24.

REV. WILLIAM HOUGHTON, M.A. F.L.S. Rector of Preston-on-the-Weald Moors, Shropshire.—Two Lectures on THE PICTURE ORIGIN OF THE CUNEIFORM CHARACTERS; on Thursdays, March 3, 10.

H. H. STATHAM, Esq.—Four Lectures on ORNAMENT, HISTORICALLY AND CRITICALLY CONSIDERED; on Thursdays, March 17, 24, 31, and April 7.

SIDNEY COLVIN, Esq. M.A. Slade Professor of Fine Art, Cambridge.—Four Lectures on THE AMAZONS: A CHAPTER IN THE STUDY OF GREEK ART AND MYTHOLOGY; on Saturdays, Jan. 22 to Feb. 12.

REGINALD STUART POOLE, Esq.—Four Lectures on ANCIENT EGYPT IN ITS COMPARATIVE RELATIONS; on Saturdays, Feb. 19, 26, and March 5, 12.

REV. H. R. HAWEIS, M.A.—Four Lectures on AMERICAN HUMORISTS; on Saturdays, March 19, 26, and April 2, 9.

PROFESSORS TYNDALL and DEWAR will give Courses after Easter.

Royal Institution of Great Britain.

WEEKLY EVENING MEETING,

Friday, January 21, 1881.

William Bowman, Esq. LL.D. F.R.S. Vice-President, in the Chair.

Warren De La Rue, Esq. M.A. D.C.L. F.R.S. Sec. R.I.
Cor. Mem. Inst. France, Hon. Mem. Impl. Academy of St. Petersburg, &c.

The Phenomena of the Electric Discharge with 14,400 *Chloride of Silver Cells.*

For the last six years I have, in conjunction with my friend Dr. Hugo Müller, been engaged with experiments on the electric discharge, using as the source of electricity a constant voltaic battery which we devised.* It is in principle the same as that invented by Daniell, but in our battery a solid electrolyte, insoluble in water or a weak saline solution, namely, chloride of silver, replaces the soluble sulphate of copper, so that no porous cell is needed in the chloride of silver battery. The results of our experiments my colleagues think of sufficient interest to be brought under the notice of the Members of the Royal Institution, and I will endeavour to make them as clear as possible in the limited time at our disposal. I must, however, ask your kind indulgence if I fail, as I have not the practice of lecturing. It is true that it is not the first time that I have had the honour to occupy this chair, which I did upwards of forty years ago.†

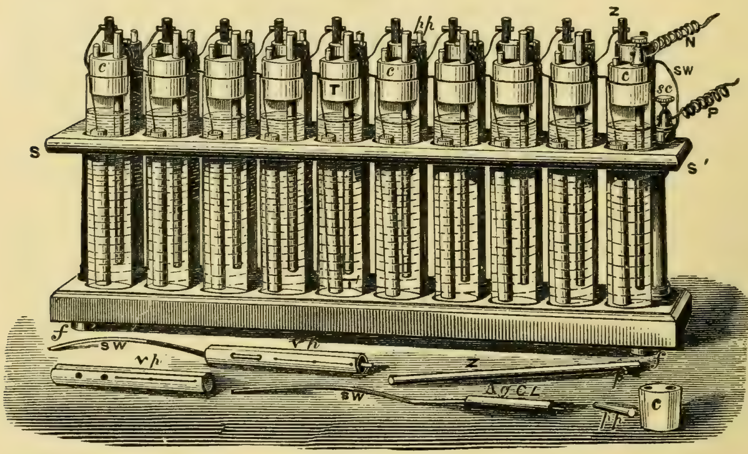
I may as well commence by describing the tool which I am about to use in the experiments: the diagram will help you to understand it. The chloride of silver battery is made up as follows: A glass tube $1\frac{1}{8}$ inch in diameter, $5\frac{1}{2}$ inches long, and containing about 2 fluid ounces of liquid; into this is fitted a paraffin stopper with two holes perforated through it; through one of these a zinc rod $\frac{3}{16}$ inch diameter and $5\frac{1}{2}$ inches long is inserted, and fastened by melting a little of the paraffin around it; the other element is formed of a flattened silver wire, which passes between the stopper and the glass; so that the metallic elements are zinc and silver. On the flattened silver wire is cast the electrolyte—namely, a rod of chloride of silver $2\frac{1}{8}$ inches long and $\frac{5}{16}$ diameter, and the cell is charged through the second perforation in the stopper with a solution of chloride of ammonium containing $2\frac{1}{2}$ per cent. of salt (Fig. 1). When the circuit is not closed—that is, when the silver element is not connected by means of a conductor to the zinc, no action whatever takes place; and in proof of this I may state that I have a battery which was made

* 'Phil. Trans.' Part I. vol. clxix. pp. 55–121, pp. 155–241; vol. clxxi. pp. 65–116.

† May 19, 1837.

up more than six years ago, and is still in action, loss of the fluid by evaporation having been from time to time made up. But as soon as connection is established, then the chloride of silver parts with its chlorine and the zinc dissolves, and metallic silver is separated, in a

FIG. 1.



spongy state, from the chloride, and remains attached to the silver wire, retaining still the form of a rod. Such an element has the electromotive force of a volt,* nearly (1·03 volt).

A Volt is that electromotive force which, working through a resistance of one Ohm, would deposit 0·0011363 gramme of silver from a salt of silver; or decompose 0·0000947 gramme (0·00146 grain) of water in one second.

A column of mercury at 0° Cent., one square millimetre in section and 1·05 metre high, offers a resistance of 1 ohm; a pure copper wire $\frac{1}{16}$ inch diameter and 129 yards long offers a resistance of an ohm.

These cells are grouped together in trays containing twenty or more, and the trays are placed in cabinets containing in some instances 1200 cells, in others 2160 cells; a cabinet of 1200 cells is shown in Fig. 2. The total number of elements I am about to use is 14,400, and these possess a potential of 14,832 volts, which is con-

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* The units adopted for electrical measurements are those of the Centimetre Gramme Second (C. G. S.); where the length is 1 centimetre, the mass 1 gramme, and the interval of time 1 second.

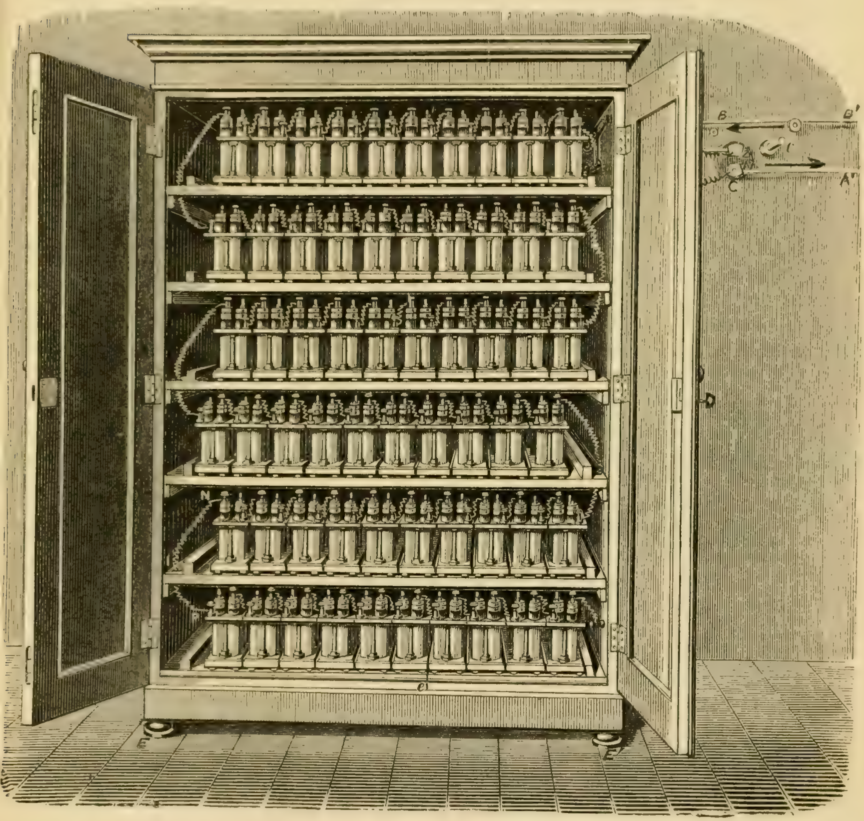
The expression for the volt in this system is 10^8 C. G. S.

"	"	ohm	"	10^9	"
"	"	microfarad	"	10^{-15}	"

For a complete account of these and other units see 'Everett's Units and Physical Constants,' 1879. Macmillan.

siderably greater than that of any battery hitherto united in series. The illustrious Sir Humphry Davy used in 1808, in this theatre, a battery of 2000 plates 4 inches square, with double plates of copper, the battery being charged with a dilute mixture of sulphuric and nitric acids. With this magnificent instrument, placed at his disposal by the subscriptions of a few patrons of science, he obtained a spark $\frac{1}{40}$ to $\frac{1}{30}$ of an inch, when the terminals were made to approach each

Fig. 2.



other (a striking distance of $\frac{1}{40}$ of an inch would accord with our experiments with the chloride of silver battery, if the difference of potential of the two batteries is taken into account). When the discharge had once taken place, then the terminals might be separated 4 inches without causing its discontinuance.

My friend the late Mr. Gassiot constructed several batteries of

high potential, and at the time of his death there were 3000 Leclanché cells in action at his laboratory; on January 26th, 1875, I measured the length of the spark between points and found it to be 0·025 inch; 3000 of our cells produced a spark of more than twice this length, namely 0·0564 inch, on account of its better insulation.

I propose, in order to show the power we have at command, in the first instance to accumulate, by means of a condenser, the electricity from 3240 cells, and to send its charge through a platinum wire $\frac{1}{80}$ of an inch thick. In charging the condenser I will pass the current through a voltameter, in order that you may judge of the very small chemical force concerned in the production of the enormous mechanical effect of the electric discharge. I may as well at once tell you that the current necessary to charge the condenser I am employing would decompose merely $\frac{1}{5000}$ of a grain of water. I will first of all pass the current from twenty cells through the voltameter; you will see that there is a rapid evolution of mixed gases (oxygen and hydrogen) into which the water is resolved. The evolution of gas, you will at once perceive, is very much slower when the current is charging the condenser; also it is more rapid at first and then gradually lessens, and would entirely cease if there were no leakage of the charge.

When I send the charge of the condenser, which has the enormous capacity of 42·8 microfarads * (or equal to 6485 Leyden jars, like that I have before me, which has coatings of 442 square inches), through $2\frac{1}{2}$ inches of gold wire $\frac{1}{80}$ inch diam. strained on a glass plate, it will be violently deflagrated with a loud report, and the metal will be scattered into dust, which the microscope shows to be composed of minute metallic globules, and not an oxide resulting from combustion. Faraday proved that the quantity of electricity necessary to produce a powerful flash of lightning would result from the decomposition of a single grain of water. This can be realised when it is remembered that it would be 5000 times as great as the charge of the 42·8 m.f. condenser just shown you. If we place the glass plate on which the wire was strained before the microscope, then it will be perceived that the distribution of particles of gold is not uniform along the space which the wire occupied, but on the contrary, they present a stratified appearance, indicating a series of pulsations during the apparently instantaneous discharge. I hope to show you shortly that the most steady discharge through a vacuum tube is in reality intermittent.

As I shall for this purpose cause the current passing through the tube to pass at the same time through an induction coil, so as to induce a secondary current, I will render evident to you, in a striking manner, that when electricity is caused to pass through a wire, it induces another or secondary current in an adjacent wire. I have

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* A condenser, which holds the charge of a current produced by 1 volt working for 1 second through a resistance of 1 ohm to a potential of 1 volt, has the capacity of 1 farad. The farad is too large a quantity for practical purposes, therefore the millionth part of it, or the microfarad, is employed as the unit of capacity.

here two insulated wires, each 350 yards long, coiled side by side on a reel; to the extremities of one coil is attached a platinum wire six inches long and $\frac{1}{500}$ inch diameter; through the other coil I will send the charge of electricity from a condenser of seven microfarads capacity (about the sixth of that just used) charged with 10,800 cells. You perceive that the platinum wire is violently deflagrated with a loud report by the induced current.

The mechanical effects produced by the charge of a condenser are as the square of the number of cells used to charge it, and although the condenser which I have just used has only one-sixth of the capacity of that I first showed you, yet its mechanical effects are nearly twice as great; for the square of 10,800 is to the square of 3240 as 11 to 1. In order to show the enormous power of its charge I will send it through 29 inches of platinum $\frac{1}{100}$ of an inch in diameter; this is immediately deflagrated. And if I allow the charge to pass between the terminals of a discharger the loud report of the spark renders evident the enormous power stored up by the condenser. I had hoped to show you the condenser charged with 14,400 cells, but it is not capable of withstanding this potential, for one after the other of the coated glass plates, of which it is made up, has broken down with the charge shortly before the lecture.

In order to afford you an opportunity of forming a pictorial conception of that which it is wished to convey, respecting the stratified discharge, I will recall to your recollection an experiment often shown to you by Dr. Tyndall (Fig. 3). With a reservoir of water, placed at a height of a few feet, when the tap at the lower portion is turned on the water flows out, apparently in a continuous stream; but when the thread of water is examined by means of an intermittent beam of light, it is at once seen that the flow is not continuous, but (in consequence of the tendency of water to assume a globular form) the stream as it descends breaks up into a series of drops, one following the other in rapid succession. It is not my purpose here to refer to the cause of the phenomenon, which has been explained to you by Dr. Tyndall in his lectures on Sound, but only to recall this elegant experiment in order to present a mental picture of what may occur in the aggregation of the molecules of gases conveying electricity.

Now I will cause a discharge of electricity to pass through a vacuum tube containing residual carbonic acid at a pressure of 0·5 millim. (Fig. 4), and you will at once perceive that the residual gas groups itself into a series of luminous strata, the molecules which compose them being held together by the balance of electric forces, whereas in the case of the water stream the particles composing the globules are held together by cohesive attraction.

The strata do not flow on like the drops of water, but remain stationary; they are, as it were, so many Leyden jars charged on one side with positive and the other with negative electricity; each imparts say its positive charge to the next negative end of the succeeding stratum, and receives a charge from that behind it; and thus the flow

of electricity goes on from one terminal to the other without any movement of the strata necessarily taking place.

Fig. 3.

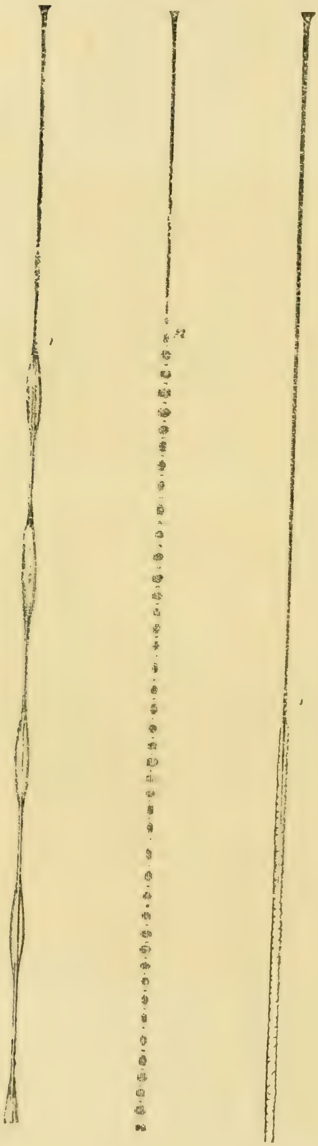
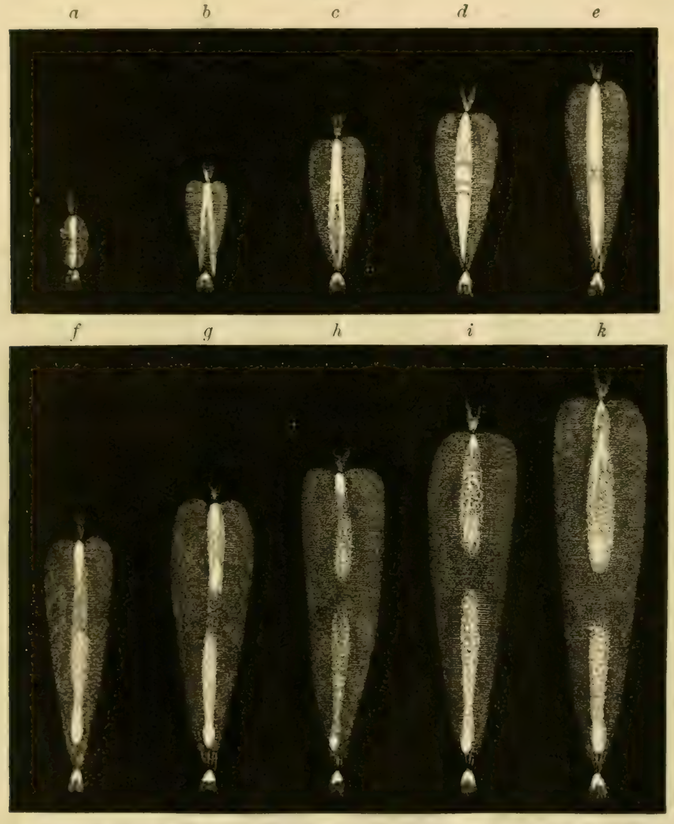


Fig. 4.

If we examine the electric arc passing between the terminals of a battery, either at ordinary atmospheric pressure or at other less pressures, it is seen that there is a resemblance to the discharge in vacuum tubes, the light emitted by different parts of it not having the same intensity throughout, and that under most circumstances there is a tendency to break up into distinct entities of the nature of strata and ultimately to take a stratified appearance like the discharge in vacuum tubes; from this we may infer that the discharge in a vacuum tube is in reality a magnified arc.

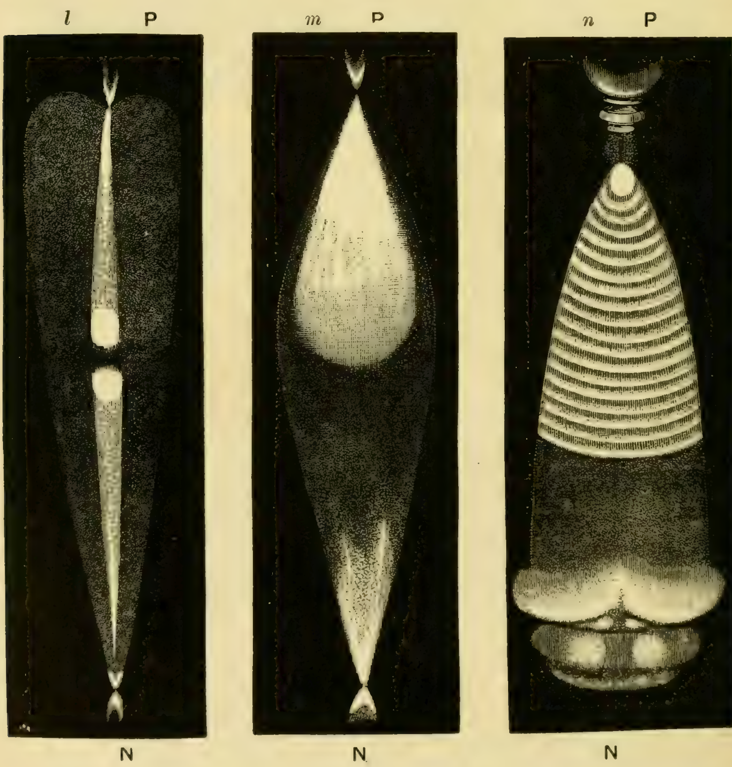
Fig. 5.



I cannot show these phenomena in a way that you could make them out at the distance you are from me, but I will, with the assistance of Mr. Cottrell, exhibit to you copies of photographs of the arc in atmospheric air (*a* to *n*, Figs. 5 and 5A) taken in my laboratory under various conditions as to distance between the terminals and pressure, as set forth in the following table:—

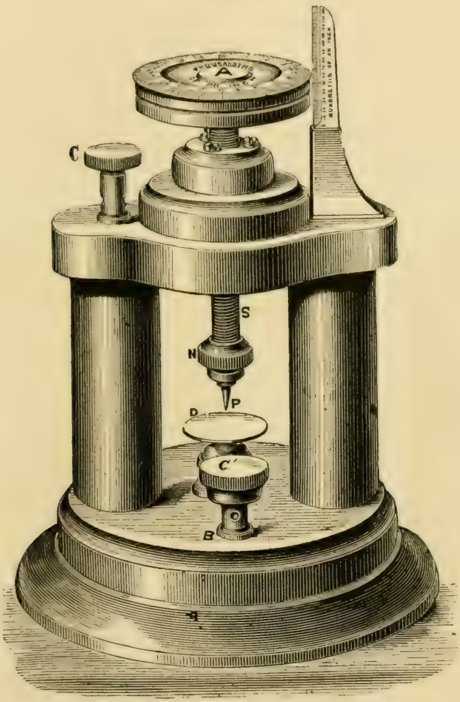
Fig.	Distance.	Pressure.		Cells.	Current.
		mm.	M.		Weber.
	inch.				
a	0·58	748·6	985,000	10,940	
b	× 2 1·16	294·9	388,026	,,	0·02881
c	× 3 1·74	191·3	251,711	,,	0·04060
d	× 4 2·32	142·6	187,631	,,	0·04474
e	× 5 2·90	112·6	148,157	,,	0·03459
f	× 6 3·48	99·4	130,789	,,	0·03071
g	× 7 4·06	85·9	113,026	,,	0·03259
h	× 8 4·64	71·6	94,210	,,	0·02693
i	× 9 5·22	65·5	86,184	,,	0·02693
k	× 10 5·80	64·4	84,737	,,	0·03071
l	× 6	67	88,158	11,000	Too small to measure.
m	× 6	8	10,526	11,000	0·01771
n	× 6·3	2	2,632	2,400	Not measured.

Fig. 5A.



I can let you see the arc, although I am unable to show the details of its structure; thus, when I move the discharging key the arc passes between the two points 0·7 inch apart fixed in the micrometer-discharger (Fig. 6), in which, however, the terminals shown consist of a point and disc, instead of two points, which I am now using. And I may mention that before the discharge takes place there is neither condensation nor dilatation of a gaseous medium in contiguity with the charged terminals, as has been suggested,

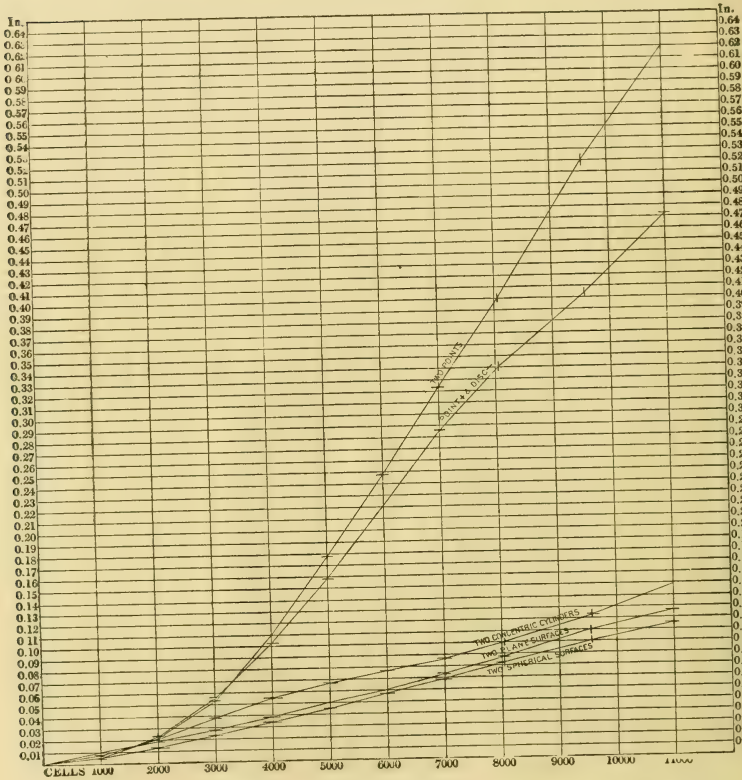
FIG. 6.



whatever may be their form. The length of the arc varies with the potential of the battery, and with the form of the terminals; between points, the length of the striking distance increases as the square of the number of cells employed. Thus, with 1000 cells the striking distance is 0·0051 inch; with 11,000 cells it is 0·62 inch, as shown in the diagram (Fig. 7). The potential of 11,000 cells put our means of insulation to a severe test, and 14,400 cells overcomes it to such an extent as to interfere so seriously with the striking distance that I only obtain a spark 0·7 inch long.

On the supposition that a cloud would act very much as a mere point at the great distance at which a lightning discharge occurs between clouds or a cloud and the earth, we may from these data

FIG. 7.



calculate the potential necessary to produce a lightning flash a mile, or 63,360 inches, long. It would require nearly 243 units of 14,400 cells united in series, or say 3,500,000 cells about.

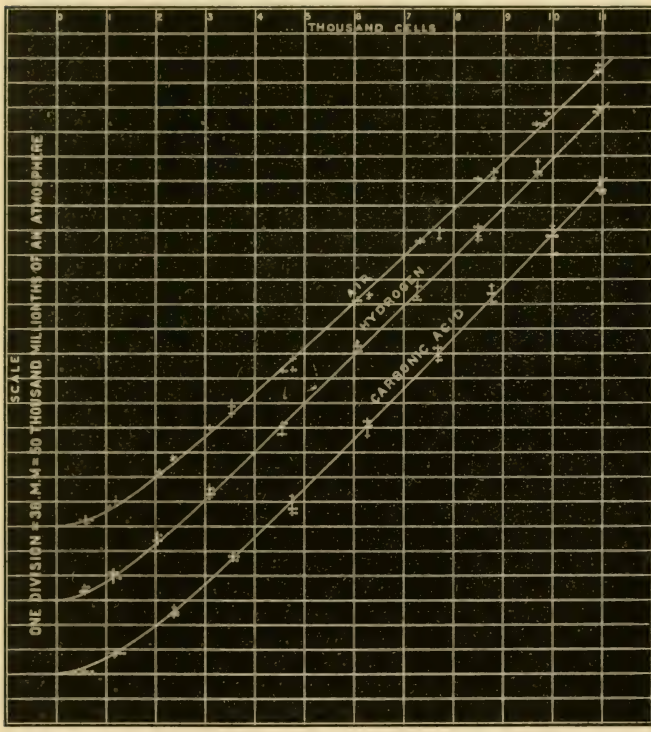
The striking distance may be increased by an arrangement of condensers to form what is called a cascade,* and in this way I shall be able to produce a spark an inch long with only 1200 cells. Such a battery I now use to charge twenty-five plates of a small condenser, and by means of a rotating commutator, connect, so to speak, the outside of one plate to the inside of the next, and thus multiply the potential

* The battery itself is a cascade.

twenty-five times; 1200 cells have, as you see, a very short striking distance; it is only 0·00608 inch, so that the spark obtained with the cascade is 164 times as long as with the battery alone. If there were no loss in converting quantity into potential, it would be 625 times or the square of 25. The apparatus I am using is the so-called Rheostat of Gaston-Planté. Franklin, it will be remembered, was the inventor of the cascade. It is not impossible that the effects of lightning may at times be increased by a kind of cascade arrangement formed by the charged layers of cloud floating one over the other.

Between discs the law of the electric discharge is not the same as between points; its length does not increase nearly so rapidly, as

Fig. 8.

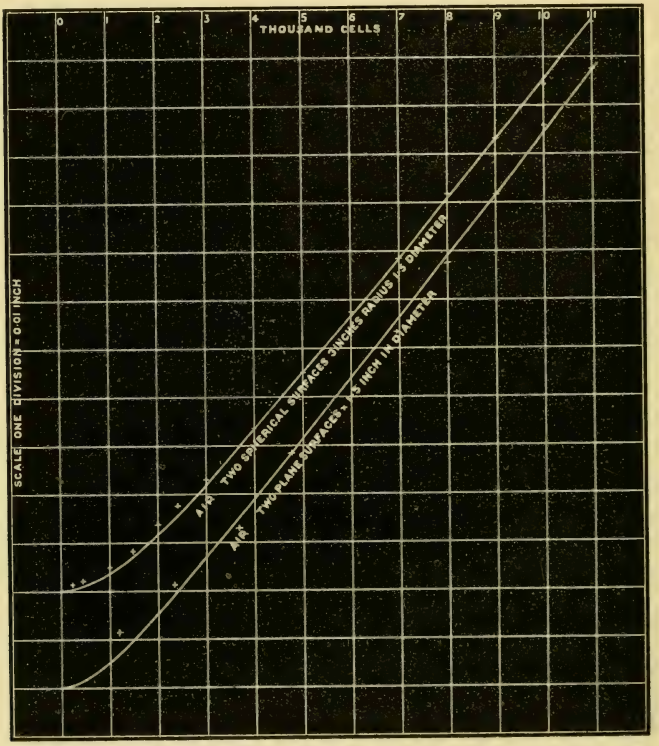


will be seen by a reference to the diagram (Fig. 7), which represents the discharge between two points, a point and disc, between spherical surfaces, and between concentric cylinders. But the increment of potential necessary to produce a discharge for a given distance between

discs, say a centimetre, becomes less as the distance between discs, and consequently the potential, are increased. Thus the electrostatic force per centimetre with 1000 volts and a striking distance of 0·0205 centimetre, is 163 electrostatic units, while it is only 113 units with 11,000 volts and a striking distance of 0·3245 centimetre.

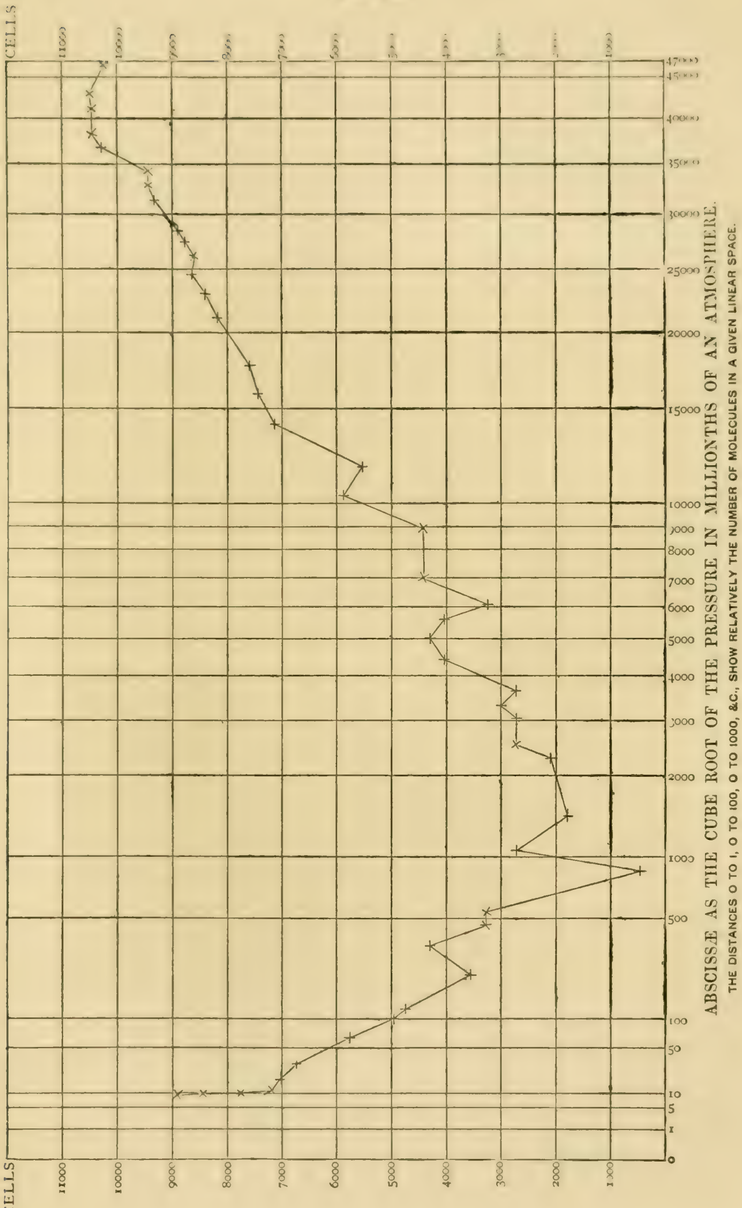
We have found, moreover, that the discharge between discs in air, hydrogen, carbonic acid, and probably also in other gases, may be represented by a hyperbolic curve, and this is the case whether we send the discharge through the gas at a constant pressure and increase

Fig. 9.



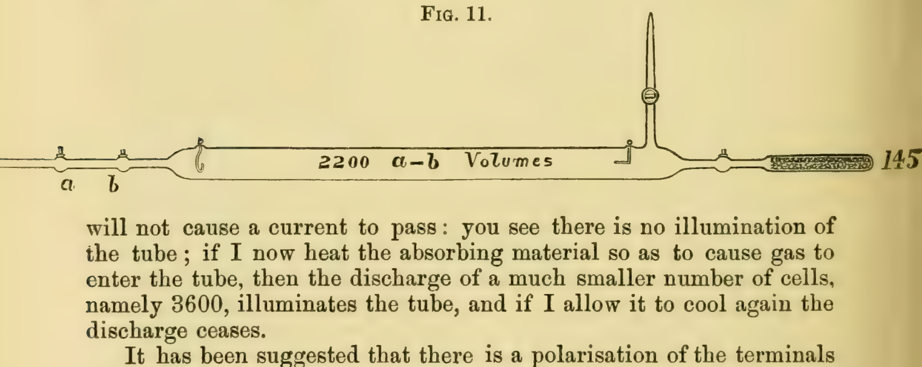
the distances between the terminals and also the number of cells, or send the discharge at a constant distance and vary the pressure and number of cells; the obstacle in the way of a discharge being as the number of molecules between the terminals *up to a certain point*, as will be seen in the diagrams (Figs. 8 and 9), in which the cross marks represent the actual observations. For although the potential necessary to produce a discharge diminishes as the pressure decreases, yet

FIG. 10.


CELLS
CELLS
ABSCISSÆ AS THE CUBE ROOT OF THE PRESSURE IN MILLIONTHS OF AN ATMOSPHERE.
THE DISTANCES 0 TO 1, 0 TO 100, 0 TO 1000, &C., SHOW RELATIVELY THE NUMBER OF MOLECULES IN A GIVEN LINEAR SPACE.

this is true only up to a certain limit; after this has been reached it rapidly increases, and ultimately the resistance becomes so great, as the exhaustion is carried further, that it is easier for a spark to pass between terminals placed at the same distance outside the tube in air at atmospheric pressure. The diagram, Fig. 10, in which the abscissæ are the cube roots of the pressures in millionths of an atmosphere, and the ordinates the number of cells necessary to produce a discharge in a hydrogen tube 30 inches between the terminals, shows the results of experiment. The pressure of minimum resistance varies for different gases. We have determined it to be 0·642 mm. = 845 millionths of an atmosphere for hydrogen, at which pressure the potential necessary to produce a discharge through a tube with terminals 30 inches apart was found to be only 430 cells. At a pressure of 0·0065 mm., 8·6 millionths, it requires as high a potential, 8937 cells, as at a pressure of 21·7 mm., 28,553 millionths, to cause the discharge to take place. At 0·00137 mm., 1·8 millionth, 11,000 cells will not pass. The greatest exhaust we have obtained in a hydrogen vacuum and an absorption by spongy palladium was 0·000055 mm., 0·07 millionth, which offered so great a resistance that a 1-inch spark from an induction coil could not traverse the tube. I will now exemplify what I have said by showing you a tube with an absorption chamber (Fig. 11). I expect that the vacuum will prove to be so good that the whole of the battery, 14,400 cells,

Fig. 11.



will not cause a current to pass: you see there is no illumination of the tube; if I now heat the absorbing material so as to cause gas to enter the tube, then the discharge of a much smaller number of cells, namely 3600, illuminates the tube, and if I allow it to cool again the discharge ceases.

It has been suggested that there is a polarisation of the terminals of a vacuum tube during the passage of electricity just like that which occurs in a voltameter, and that this increases the obstacle to the discharge; but by an elaborate series of experiments we have proved that such is not the case under the conditions of the experiment. It is quite true that, after the connection between the battery and the terminals of the tube has been broken, there is a deflection of the needle when they are connected with a galvanometer, but we have shown that this is entirely due to a minute static charge proportionate to the capacity of the terminals.*

* 'Roy. Soc. Proc.' No. 205, 1880.

Our experiments * enable us to throw some light on another atmospheric electrical phenomenon—namely, the probable height of the aurora borealis, which the accompanying figure (Fig. 12) of a discharge roughly resembles. I will now pass the current of the whole 14,400 cells through the large tube 199, containing a residual charge of atmospheric air at a pressure of 1 millimetre, and you will perceive a carmine luminosity touching the positive pole and reaching halfway down the tube. This reminds one of those ruddy glows frequently seen in auroral displays. Fig. 12 in the plate is copied from a photograph since taken in my laboratory of this appearance. Around the bright luminosity is a dark band which shuts off a portion of the fluorescence of the glass tube, a blue fluorescence produced by the ruddy light of the luminosity, showing that around the luminosity there is an absorbent zone of less elevated temperature. Many estimates have been made from time to time of the height of auroræ, founded upon observations made by persons at a distance from each other, and supposed to be observing the same feature in the display; but it must be remarked that there is always much uncertainty in these estimates, from the difficulty of knowing whether the different observers have noticed the self-same streamer. Frequently very considerable altitudes have been assigned to these displays; for example, as much as 281 miles. We shall presently see that it is very improbable that any electrical discharge could occur at such a height. We have calculated from experiment that the pressure of least resistance for air is 0·397 millimetre, 498·6 millionths, and therefore in air it results that a maximum electric discharge, and consequent brilliancy, of the aurora, would occur at an elevation where the atmosphere has that pressure—namely, 37·67 miles. The greatest exhaust we have produced—and this has not been surpassed—is 0·000055 millimetre, 0·07 millionth, which is the pressure the atmosphere would have at 81·47 miles; and as 11,000 cells failed to produce a discharge even in hydrogen at this low pressure, it may be assumed that at this height the discharge would be considerably less brilliant than at 37·67 miles, should such occur.†

Fig. 12.

At a height of 281 miles the atmosphere would only

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* 'Roy. Soc. Proc.' No. 203, 1880.

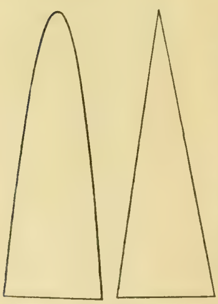
† It is conceivable that the aurora may occur at times at an altitude of a few thousand feet.

have a pressure of 0·000000000000000000018 millimetre, or 0·0000000000000000024 millionth, and even at 124·15 miles in height the atmospheric pressure would be only 0·00000001 millimetre, or 0·00001 millionth. It is highly improbable that a display of aurora would occur at a height even of 124 miles, and it is difficult to conceive that an electrical potential could possibly exist necessary to overcome the enormous resistance that would be offered at 281 miles, the tenuity of the air being 54,000,000,000,000 (54 million million) times as great as at 124 miles.

Pressure mm.	Pressure M.	Height, miles.	Visible at miles.	Remarks.
0·00000001	0·000013	124·15	1061	No discharge could occur.
0·000055	0·07	81·47	860	Pale and faint.
0·379	499·0	37·67	585	Maximum brilliancy.
0·800	1053·0	33·96	555	Pale salmon.
1·000	1316·0	32·87	546	Salmon coloured.
1·500	1974·0	30·86	529	„ „
3·000	3947·0	27·42	499	Carmine.
20·660	27184·0	17·86	403	„
62·000	81579·0	12·42	336	„
118·700	156184·0	9·20	289	Full red.

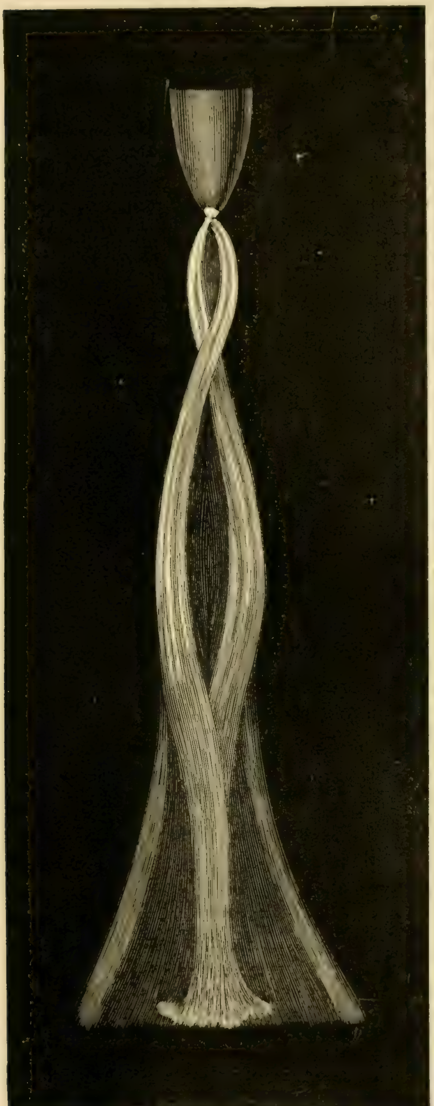
There are some phenomena connected with the discharge from the voltaic battery which I will bring under notice before we proceed to the study of the discharge in vacuum tubes. I have already spoken of the difference in the length of the discharge between points and discs; and I have now to call your attention to the influence of the form of the point on the length of the spark. At first it would naturally be supposed that the longest discharge would occur with the sharpest point, but this is not the case; a great number of experiments with various forms of points have shown that a point in the form of a paraboloid gives the longest spark; and longer in the proportion 1·29 to 1 than one in the form of a cone of the same length and diameter at the base. It is difficult to account for this difference in the length of the spark, but it is evident the potential must be greater at the extremity of a paraboloidal point than it is at the extremity of a conical one (Fig. 13).

Fig. 13.



If a point and a disc be used together as terminals and the point be made alternately positive and negative, the spark is longest when the point is negative for low tensions up to 3000 cells, and longest when the point is positive beyond that number.

Fig. 14.

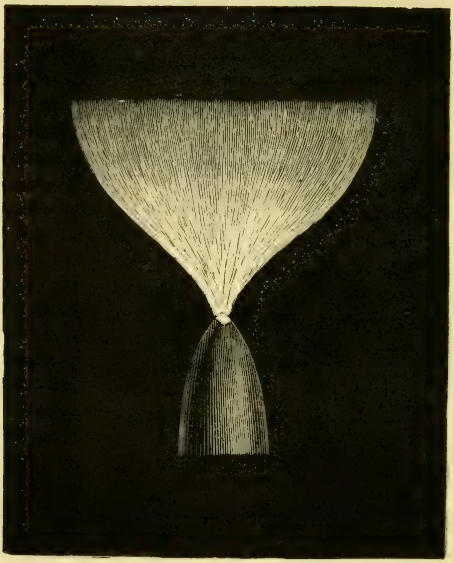


The nature of the metal makes not the slightest difference on the length of the spark, with one exception. Brass, copper, silver, steel,

platinum, magnesium, and graphite, all give, under similar circumstances, precisely the same length of spark: aluminium, however, gives a spark longer in the proportion of five to four.

Before the spark jumps and the arc forms it is preceded by what we have called the streamer discharge. This is different in appearance at the positive and negative terminals. You are not able to see the characteristics now that I produce the streamer discharge before you, but they are represented enlarged on the diagram; the terminals being supposed to be a point and a disc, and the point being made alternately positive and negative. When the point is positive the discharge takes the form of a series of twisted streamers (Fig. 14), when negative it is in the form of a brush (Fig. 15). The current which

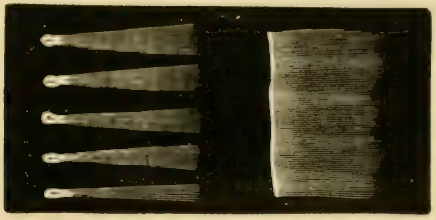
Fig. 15.



takes place in the form of streamers at a distance but a very little beyond the true striking distance, namely, that at which the arc is formed, is only the $\frac{1}{2500}$ part of the current which passes with the arc, and this is only one-half of that of the battery when short-circuited. When the streamer discharge is examined in a rotating mirror with a microscope suitably constructed, it is seen that the negative current is much the more continuous, for with the same velocity of the rotating mirror the positive discharge breaks up into a series of distinct images, whilst the light of the negative is spread out into a sheet, as you will see on the diagram (Fig. 16). The above effects were produced with

8000 cells, but with 11,000 cells we obtained a further confirmation of them, the effects being shown in another figure on the diagram (Fig. 17), which represents a streamer discharge between two points. The negative discharge is a brush which is seen continuously on the lower terminal, while the positive consists of a series of intermittent, ever-

Fig. 16.



changing spiral streamers which envelop the negative brush discharge without in the least disturbing its form. They go past the negative point and then curl upwards towards it.

If I insert a very high resistance between the battery and the terminals the streamer discharge ceases, and a static spark passes from time to time which is exactly like that from an ordinary frictional machine; it pierces a thin strip of paper just as a static charge would do. The battery gathers up at intervals a charge at the terminals, and the discharge occurs as soon as the potential is sufficient to force its way across the obstacle opposed by the intervening air.

The same thing occurs if I attach a condenser to the terminals of the battery: it takes a longer time for the battery to charge it, and consequently the discharge occurs at longer intervals, shorter or longer according as the terminals are adjusted to a less or greater distance. The condenser I am now using has a capacity of 1·5 microfarad, and hence the accumulated charge is very considerable, and the discharge is like that of a powerful electric battery. But whether the capacity of the accumulator be large or by comparison infinitesimally small like that of the points in the discharging micrometer, there is always an interval of time which elapses between successive discharges; the interval may be so extremely minute that thousands of millions of discharges may occur in a second, but the flow is nevertheless discontinuous, like the drops constituting the stream of water before referred to.

I will endeavour to prove to you that an apparently perfectly steady discharge through a vacuum tube, in which there is no apparent motion of the strata, and in which even the rotating mirror would fail to detect any intermittence, is nevertheless discontinuous. It is true that the period of pulsation must be of a very high order, millions in

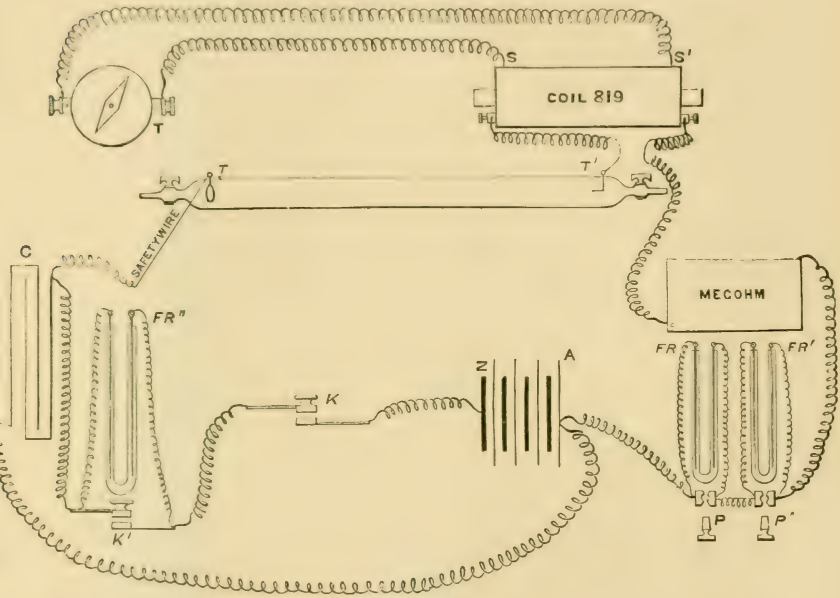
a second, and it is necessary, therefore, to have recourse to special means in order to detect it, which I will describe.

The current from the battery of 2400 cells is made to pass through the primary of an induction coil, and then through a vacuum tube (No. 142, Fig. 11 in the plate—the figure is copied from a photograph obtained in $2\frac{1}{2}$ seconds) containing a residue of carbonic acid at a pressure of 0·4 millimetre. The arrangement is shown in Fig. 18, where T T′ is the vacuum tube connected in circuit with the primary of coil 819; Z A the battery, on the A side connected with fluid resistances F R, F R′, and wire resistances amounting in all to a megohm. On the Z side is shown a condenser C connected through a fluid resistance F R″ with the Z pole. The terminals of the secondary wire of the induction coil are connected with a sensitive Thomson galvanometer. If there is any intermittence in the current through the tube, an effect will be produced on the galvanometer *under certain circumstances*, that is, provided the rise and fall of the current occur in unequal periods. I will now make connection with the battery; you perceive that there is a deflection of the galvanometer to the left on making contact; I will now allow the spot of light to come to rest, and then break contact, and there is a deflection in the reverse direction, that is, to the right. The first is called the inverse current, the last the direct current. I will now send the current again through the primary of the induction coil and thence through the tube, but in the first instance I will short-circuit the secondary current so that the galvanometer may not be disturbed when the connection with the battery is made; now that the strata are perfectly steady, I will allow the induced current to go through the galvanometer by removing the short-circuit plug, and you see that there is a slight *permanent* deflection

FIG. 17.

of the galvanometer. This shows that the discharge in the vacuum tube, although apparently quite steady, is a pulsating one; as the swing is to the right we know that the current is a direct or break contact one, thus indicating that the discharge through the tube increases compara-

Fig. 18.



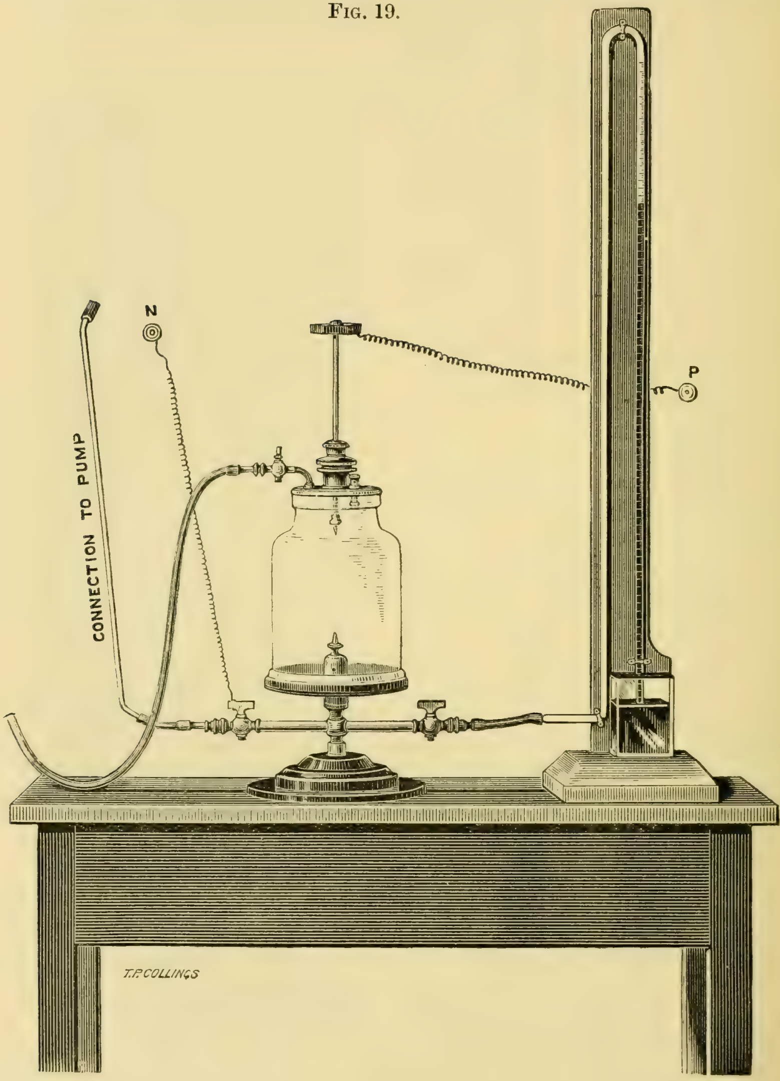
tively slowly, then drops more suddenly. If the rise and fall were in equal times, there would be no deflection of the galvanometer.

If the terminals of a telephone are placed in the circuit between the battery and a vacuum tube, the pulsations are sometimes sufficiently slow to produce audible sounds when the telephone is placed to the ear. But the telephone is not adapted to render evident intermittences of a very high order.

There is a remarkable phenomenon which occurs when a charge is sent through a closed vessel containing air or gas, within certain limits of pressure, which I will endeavour to show you. As soon as the connection is made between the battery and the terminals a sudden expansion of gas takes place, as you will see (Fig. 19) by the depression of the mercury in the gauge connected with the bell jar, and as soon as the connection with the battery is broken the gas returns suddenly almost exactly to its original volume, showing only a small increase due to a slight elevation of temperature; the mercury in the gauge rises, therefore, nearly to its original position. The effect is similar to that which would be produced if an empty

bladder suspended between the terminals had been suddenly inflated and as suddenly emptied. The ratio of the increased volume (pres-

Fig. 19.



sure) to the normal volume in our experiments rose sometimes as 1·71 to 1, and in others there was scarcely any appreciable increase; in the present instance it is as 1·4 to 1.

The discharge, in the bell jar, was photographed on one occasion and the central spindle or arc proper was measured on the photograph, and a calculation made of what its temperature must have been on the supposition that the sudden dilatation might be due to it, and the result was 16000° Cent. Experiments were also made to ascertain the temperature of different parts of the arc, and it was found that platinum wire $\frac{1}{1000}$ of an inch in diameter was immediately fused, but there was no vaporisation of the platinum, which certainly would have occurred had such a temperature as 16000° Cent. existed. It was ultimately concluded, from a number of experiments and considerations, that the enormous and sudden dilatation could not be attributed to a sudden increase of temperature, but must be caused by the scattering of the gas molecules away from the terminals, and their projection by electrification against the walls of the containing vessel.

We have proved experimentally that the discharge in a vacuum tube does not differ essentially from that in air and other gases at ordinary atmospheric pressures; it cannot be considered as a current in the ordinary acceptation of the term, nor as at all analogous to conduction through metals, and must consequently be of the nature of a disruptive discharge, the particles acting as carriers of electrification. For example, a wire having a given difference of potential between its ends, can permit one, and only one current to pass; whereas, we have found by accurate measurements that with a given difference of potential between the terminals of a vacuum tube, currents of strength varying from 1 to 135 can flow.

We have found, moreover, that the resistance of a vacuum tube, unlike that of a wire, does not increase in the ratio of the distance between the terminals. As an example may be cited that, in a Spottiswoode tube (Fig. 20) with one shifting terminal, which can be placed at any required distance from the other, for seven times the distance between the terminals the resistance was found to be only twice as great. Moreover the fall of potential is not uniform for equal increments of distance between the terminals of a vacuum tube as it is for equal increments of the length of a wire. In order to determine this we used a tube with seventeen rings inserted in it at equal distances (Fig. 20); to these were attached wires which projected through the tube, and were soldered to it. One pole of the battery was connected to No. 1 ring of the tube, and the last ring as well as the second pole of the battery were connected to earth and stood at zero. By means of an electrometer, shown in Fig. 21, the induction plate of which could be made to communicate with each ring successively, it was found that the difference of potential between the first pair of rings, reckoning from the terminal connected with the battery, was five times as great as that between the eighth and ninth; again, that between the sixteenth and seventeenth it was twice that between the eighth and ninth. If I, by way of illustration, suspend a number of pith balls to a wetted thread, one end of

which I connect with one pole of the battery and the other to earth, the other pole of the battery being also connected to earth, you will notice a uniform decrease of divergence of the balls, because

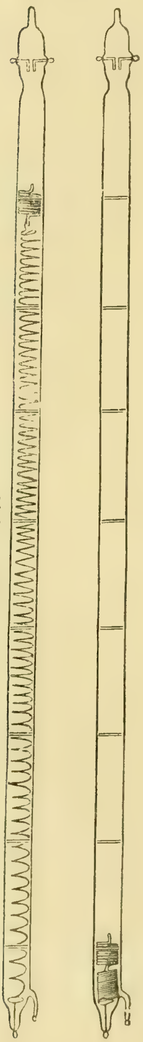


Fig. 20.

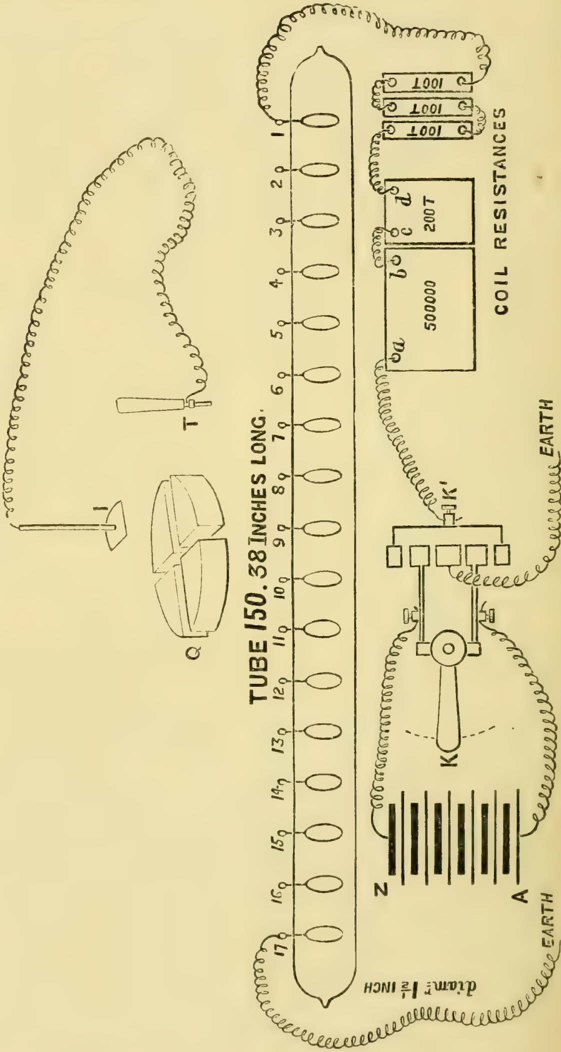


Fig. 21.

the potential decreases uniformly for equal distances, whereas this does not occur when pith balls are suspended to the rings of a

vacuum tube, as you will see when I connect the first ring to one pole of the battery and the other to earth.

We will now take up the phenomena exhibited by vacuum tubes. It will be seen that the strata have their origin at the positive pole. Thus in a given tube, with a certain gas, there is produced at a certain pressure, in the first instance, only one luminosity,* which forms at the positive terminal; then, as the exhaustion is gradually carried further, it detaches itself, moving towards the negative, and being followed by other luminosities, which gradually increase in number up to a certain point. This I will show you, with Mr. Cottrell's aid, by projecting copies of photographs, made in my laboratory, from tubes containing hydrogen at gradually decreasing pressures.

If I now connect the fixed terminal of the Spottiswoode tube, containing residual carbonic acid at a pressure of 1 millimetre, with the positive pole of a battery of 2400 cells, having first caused the movable terminal (which I have connected previously to the negative pole) to approach quite close to the positive wire, you will see only one stratum. I incline the tube and allow the negative terminal to recede. Now there are three strata (Fig. 10 in the plate), and as the negative recedes further and further fresh strata pour in one by one from the positive until the whole tube is filled to within a constant distance from the negative with our electric drops (Fig. 9 in the plate).

I may here pause to draw attention to the resemblance of the strata produced by an electrical discharge in a vacuum tube to the lycopodium records of sound-pulsations in air which are given in Tyndall's work on 'Sound' (Fig. 22).

FIG. 22.

With the same potential the phenomena vary with the amount of current which is allowed to pass through the tube, and this amount we can easily regulate by inserting resistances between the battery and the tube. As the current is increased the number of strata in some tubes increases, but with other tubes the number decreases.

A change in the amount of current frequently produces an entire change in the colour of the strata. For example, in a hydrogen tube from a cobalt blue to a pink (Figs. 4 and 3 in the plate). It also changes the spectra of the strata. Moreover, the spectra of the illuminated terminals and those of the strata differ; usually the most brilliant spectra are obtained from the negative terminal.

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* It is not improbable that ball-lightning may be of the nature of this single luminosity or stratum, charged like it as a Leyden jar, and projected by an electric discharge taking place behind it; in the same way that a mechanical impulse sends forth a vortex ring.

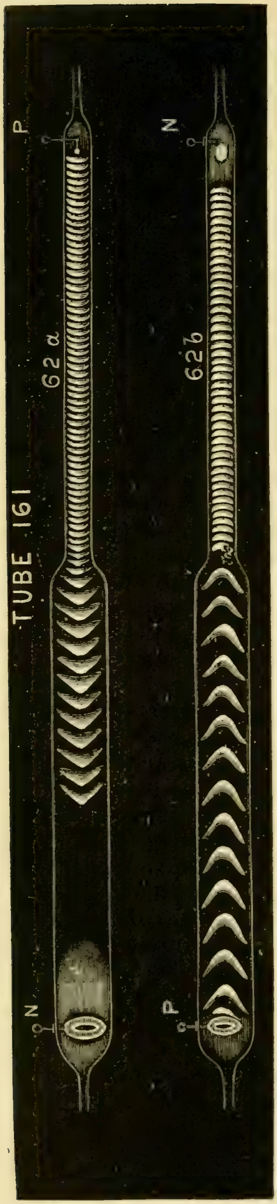


Fig. 23.

If the discharge is irregular and the strata indistinct, an alteration of the amount of current makes the strata distinct and steady; most frequently a point of steadiness is produced by the careful introduction of external resistance; subsequently, with the introduction of more resistance, a new phase of unsteadiness, and still more resistance, another phase of steady and distinct stratification.

At the same pressure, and with the same current, the diameter of the tube affects the character and closeness of the stratification (Fig. 23), as will be seen when I cause the current to pass in tube No. 161, which contains residual hydrogen. It consists of two portions, one 18 inches long and 1·15 internal diameter, the other 17·5 inches long and 0·975 inch diameter. The battery I am using consists of 4800 cells, and you perceive that whether the terminal in the small tube is positive or negative there is a marked difference in the form and closeness of the strata in the two tubes.

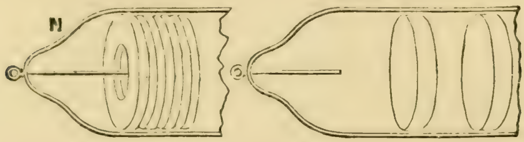
The greatest heat is developed in the vicinity of the strata. This fact we established most easily when the tube contained only one stratum, or a small number separated by a broad interval. There is reason to believe that even in the dark discharge like that in the neighbourhood of the negative terminal there may be a kind of stratified formation, for we have found a development of heat in part of a tube in which there was no illumination except on the terminals.

There are vacuum tubes made which are not open from end to end, and which consist of a number of separate chambers, some inserted into the others. The induction coil illuminates these very beautifully, but the battery will not do so, as no discharge can take place through them. On the other hand, the alternating currents of

an induction coil charge up such a tube first with positive, then with negative, electricity, and produce an illumination in consequence of the alternating charge and discharge of the walls of it. When I turn on the battery current you will perceive a flash, then if I reverse the current another flash, and if I do this quickly I make the illumination a little more persistent. But I have a rapidly reversed commutator by which I can reverse the current 350 times in a second, and you see that with its help I can illuminate the tube very beautifully.

In almost every case there is a dark interval near the negative terminal, but occasionally we have met with tubes in which the strata completely fill the tube, the last ones threading themselves on the wire used for the negative terminal (Fig. 24). Unfortunately I have not one which I can show you, as these tubes which have shown this phase completely change after the current had passed a very short time.

Fig. 24.



I now propose to show, with the aid of my two assistants, Messrs. James Fram and Ernest Davis, some tubes with various gases at different degrees of exhaust, in order that you may see the strata in all their beauty and witness the changes of which I have spoken.

I will in the first place show a tube in which there are produced a series of luminosities like those in one of the photographs which were projected on the screen. It is No. 148, with residual hydrogen at a pressure of 4 millimetres, and connected with 7920 cells. Fig. 8 in the plate shows the phenomenon; it is from a photograph obtained in four seconds.

Tube 201, shown by Fig. 7 in the plate, is a hydrogen vacuum at a pressure of 0·8 millimetre; with 3600 cells and an interposed resistance of 1,500,000 ohms a perfectly steady close stratification is produced. The figure is copied from a photograph obtained in three seconds.

Tube 139, shown in the plate by Fig. 4, is a hydrogen vacuum, pressure 0·8 millimetre, with 3600 cells and an interposed resistance of 200,000 ohms. A series of beautiful blue double strata are produced, with a carmine line between the double strata. The figure is copied from a photograph obtained in one and a half second.

Tube 139.—On interposing 500,000 ohms resistance in the circuit,

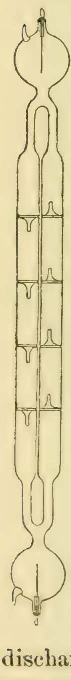
instead of 200,000, the strata are reduced in number and turn pink. The phenomenon, except as regards colour, is shown in the plate (Fig. 6), copied from a photograph obtained in three seconds.

Tube 130, a hydrogen vacuum 0·8 millimetre, with 2400 cells and an interposed resistance of 60,000 ohms. A series of tongue-shaped strata are produced, which cross each other like the components of the letter X and remain perfectly steady. The phenomenon is precisely like that shown by another tube, the photograph of which you saw projected on the screen. Tube 130 is represented in the plate (Fig. 6); it is copied from a photograph obtained in two and a half seconds.

Tube 333 is a hydrogen vacuum, pressure 0·8 millimetre, with 3600 cells and an interposed resistance of 1,500,000 ohms. There are produced a series of double tongue-shaped strata, united at their narrowest parts. This phenomenon is represented in Fig. 5 in the plate, which is copied from a photograph obtained in three seconds.

Before showing the next tube, I will exhibit one (No. 51) with a carbonic acid vacuum, in which the negative terminal consists of a wire nineteen inches long formed into a helix, the positive being a ring. On passing the current from a battery of 1200 cells through the tube, first interposing a resistance of 500,000 ohms, about two inches only of the negative is illuminated; on gradually, however, removing the resistance, more and more of the spiral negative glows until at last the whole of it is brilliantly illuminated. It will be seen by this that the negative discharge requires a greater outlet than the positive.

Fig. 25.



I will now exhibit a tube (No. 163), Fig. 25, consisting of two branches united at the top and bottom. In each of these is a series of funnels, the broad end of which fills the branch; in one branch the mouths of the funnels are placed in a contrary direction to that in the other. On connecting the terminals with the battery of 3600 cells, the current is free to pass either in both branches, or through one or the other, but it invariably passes down that branch in which the wide mouth of the funnel is towards the negative. It traverses alternately the right or left hand branch, according as I make the top or bottom terminal negative; thus again exemplifying the necessity for a greater space for the negative discharge to pass than is required for the positive. The phenomenon is shown in Figs. 11 and 12 in the plate.

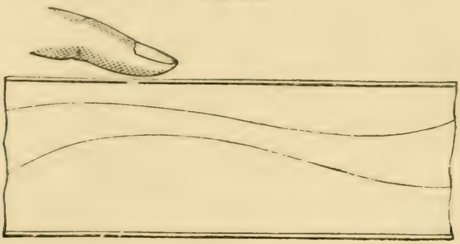
The photographs from which the figures in the plate are copied were taken in my laboratory, by Mr. H. Reynolds, on dry plates.

Very frequently, when the exhaust is very great, the discharge becomes most sensitive to the approach of the finger or any conductor in connection with the earth, or charged by a separate source of electricity; the same thing occurs if the current is

made intermittent by an interval of air in one of the connections (an air spark). See Fig. 26.

The preparations which were necessary in order that this lecture could be given have occupied a considerable time. In the first place, most of the tubes I have shown you had to be specially prepared, during the last three months, and reserved for this occasion. For all tubes completely alter their character if a current is repeatedly passed through them, and then they no longer present the beautiful phases of stratification you have witnessed. Moreover, it was not possible to

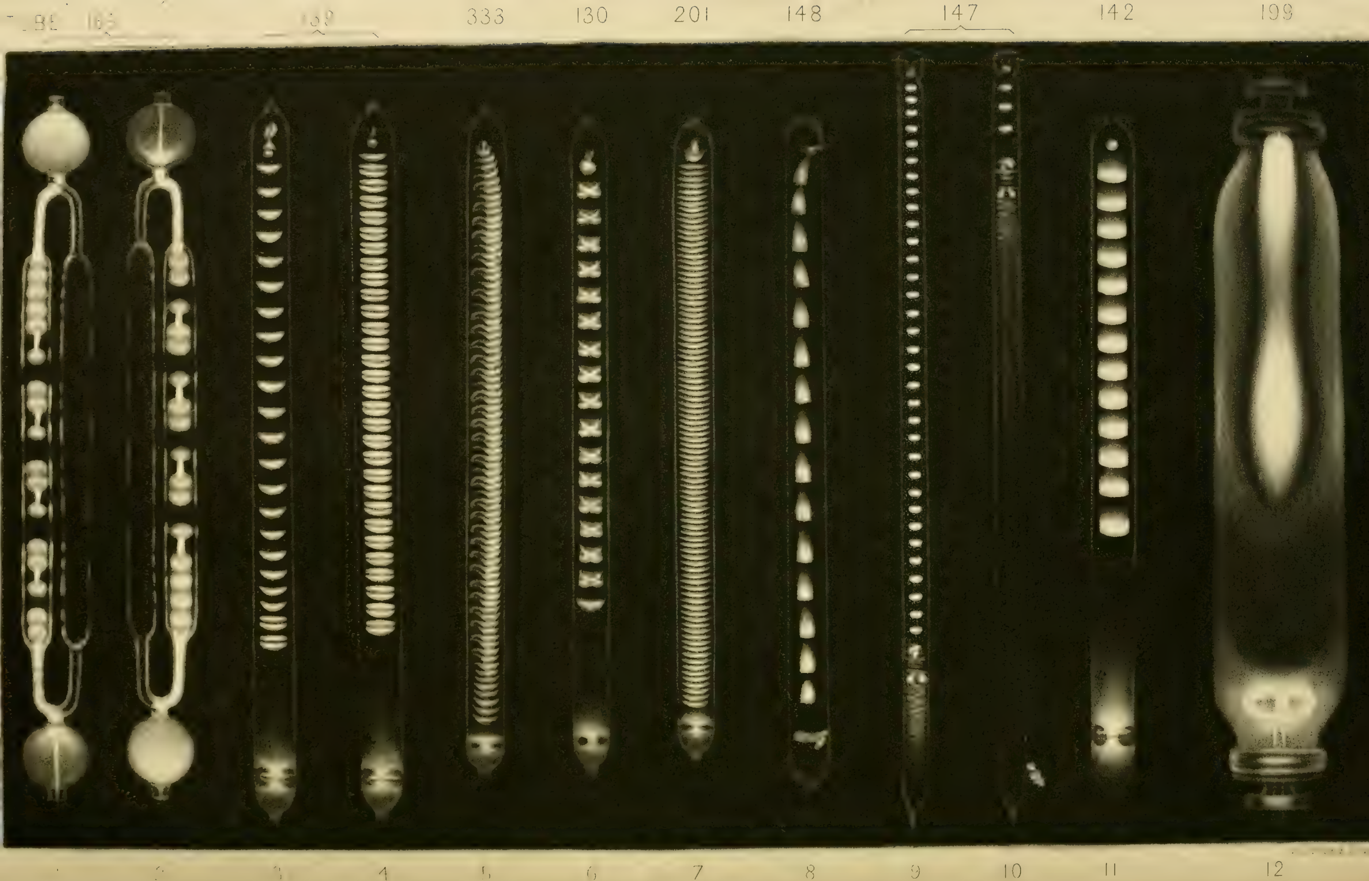
Fig. 26.



remove the battery from my laboratory, as its construction would not permit it. I therefore have had built up by Messrs. Tisley and Co. an entirely new series, in such a way that the battery can be carried away, when requisite, without injury. The construction of the battery was commenced in June 1879, and was finished in August 1880. The charging of the battery occupied a fortnight, and was finished in the second week in December.

It only remains for me to thank you for your flattering attendance under such adverse meteorological conditions.*

* Occasioned by a very heavy snowstorm on the 18th of January.



FAC-SIMILES OF PHOTOGRAPHS.

INDEX.

WEEKLY EVENING MEETING,

Friday, January 28, 1881.

Thomas Boycott, Esq. M.D. F.L.S. Vice-President, in the Chair.

Arthur Schuster, Esq. Ph D. F.R.S.

The Teachings of Modern Spectroscopy.

A science, like a child, grows quickest in the first few years of its existence; and it is therefore not astonishing that, though twenty years only have elapsed since Spectrum Analysis first entered the world, we are able to speak to-day of a modern spectroscopy, with higher and more ambitious aims, striving to obtain results which shall surpass in importance any of those achieved by the old spectroscopy, to the astonishment of the scientific world.

A few years ago the spectroscope was a chemical instrument. It was the sole object of the spectroscopist to find out the nature of a body by the examination of the light which that body sends out when it is hot. The interest which the new discovery created in scientific and unscientific circles was due to the apparent victory over space which it implied. No matter whether a body is placed in our laboratory or a thousand miles away—at the distance of the sun or of the furthest star—as long as it is luminous and sufficiently hot, it gives us a safe and certain indication of the elements it is composed of.

To-day, we are no longer satisfied to know the chemical nature of sun and stars; we want to know their temperature, the pressure on their surface; we want to know whether they are moving away from us or towards us; and still further, we want to find out, if possible, what changes, in their physical and chemical properties, the elements with which we are acquainted have undergone under the influence of the altered conditions which must exist in the celestial bodies. Every sunspot, every solar prominence, is a study in which the unknown quantities include not only the physical conditions of the solar surface, but also the possibly changed properties, under these conditions, of our terrestrial elements. The spectroscope is rapidly becoming our thermometer and pressure gauge; it has become a physical instrument.

The application of the spectroscope to the investigation of the nature of celestial bodies has always had a great fascination to the scientific man as well as to the amateur; for in stars and nebulæ one may hope to read the past and future of our own solar system. But it is not of this application that I wish to speak to-day.

As there is no other instrument which can touch the conditions of the most distant bodies of our universe, bodies so large that their size

surpasses our imagination, so is there no other instrument which equals it in the information it can yield on the minute particles at the other end of the scale, particles which in their turn are so small, that we can form no conception of their size or number. The range of the spectroscope includes both stars and atoms, and it is about these latter that I wish to speak.

The idea that all matter is built up of atoms, which we cannot further divide by physical or chemical means, is an old one. As a scientific hypothesis, however—that is, an hypothesis which shall not only qualitatively, but also quantitatively, account for actual phenomena —it has only been worked out in the last thirty years. The development of molecular physics was contemporaneous with that of spectroscopy, but the two sciences grew up independently. Those who strove to advance the one paid little attention to the other, and did not trouble to know which of their conclusions were in harmony, which in discordance, with the results of the sister science. It is time, I think, now that the bearing of one branch of inquiry on the other should be pointed out: where they are in agreement, their conclusions will be strengthened, while new investigations will lead to more perfect truths where disagreement throws doubt on apparently well-established principles.

What I have ventured to call modern spectroscopy, is the union of the old science with the modern ideas of the dynamical theory of gases, and includes the application of the spectroscope to the experimental investigation of molecular phenomena, which without it might for ever remain matters of speculation or of calculation.

A body, then, is made up of a number of atoms. These are hardly ever, perhaps never, found in isolation. Two or more of them are bound together, and do not part company as long as the physical state of the body remains the same. Such an association of atoms is called a molecule. When a body is in the state of a gas or vapour, each molecule for the greater part of the time is unaffected by the other molecules in its neighbourhood, and therefore behaves as if these were not present. The gaseous state, then, is the one in which we can best study these molecules. They move about amongst each other, and within each molecule the atoms are in motion. Each atom, again, has its own internal movement. But if the world was made up of atoms and molecules alone, we should never know of their existence; and to explain the phenomena of the universe, we must recognise the presence of a continuous universal medium penetrating all space and all bodies. This medium, which we call the luminiferous ether or simply the ether, serves to keep up the connection between atoms or molecules. All communications from one atom to another and from one molecule to another are made through this ether. The internal motions of one atom are communicated to this medium, propagated through space, until they reach another atom; attraction, repulsion, or some other manifestation takes place; and if you examine any of the changes which you see constantly going on

around you, and follow it backwards through its various stages, you will always find the motion of atoms or molecules at the end of the chain.

The importance of studying the motion of molecules is therefore clear; and it is the special domain of the modern spectroscopy to investigatate one kind of these motions.

When a tuning-fork or a bell is set into vibration, its motion is taken up by the surrounding air, waves are set up, they spread and produce the sensation of sound in our ears. Similarly when an atom vibrates, its motion is taken up by the ether, waves are set up, they spread, and if of sufficient intensity produce the sensation of light in our eyes. Both sound and light are wave motions. A cursory glance at a wave in water will lead you to distinguish its two most prominent attributes. You notice at once that waves differ in height. So the waves both of light and sound may differ in height, and to a difference in height corresponds a difference in the intensity of the sound you hear or of the light you see. The higher the wave the greater its energy, the louder is the sound or the brighter is the light. But in addition to a difference in height you have noticed that in different waves the distance from crest to crest may vary. The distance from crest to crest is the length of the wave, and waves not only differ in height but also in length. A difference in the length of a wave of sound corresponds to a difference in the pitch of the sound; the longer a sound-wave is, the lower is the tune you hear. In the case of light a difference in the length of the wave corresponds to a difference in the colour you see. The longest waves which affect our eyes produce the sensation of red, then follows orange, yellow, green, blue, and the shortest waves which we ordinarily see seem violet. If a molecule vibrates, it generally sends out a great number of waves which vary in length. These fall together on our retina, and produce a compound sensation which does not allow us to distinguish the elementary vibrations, which we want to examine. A spectroscope is an instrument which separates the waves of different lengths before they reach our retina; the elementary vibrations after having passed through a spectroscope no longer overlap, but produce their impressions side by side of each other, and their examination and investigation is therefore rendered possible.

The elements of spectroscopy will be familiar to most of you, but you will forgive me if I briefly allude to some points, which, though well known, are of special importance in the considerations which I wish to bring before you to-night.

When a body is sufficiently hot it becomes luminous, or to speak in scientific language, the vibrations which are capable of producing a luminous sensation on our retina, are increased in intensity as the temperature is raised, until they produce such a sensation. By means of a strong electric current I can in the electric lamp raise a piece of carbon to a high temperature. When looked at with the unaided eye it seems white hot, but when I send the rays through

a prism and project them, as I do now, on a screen, you see a continuous band of light. This fact we express by saying that the spectrum of the carbon poles in the electric lamp is a continuous one. You see side by side the different colours known to you by the familiar but incorrect name of "the rainbow colours"; and the experiment teaches you that the carbon pole of the electric lamp sends out rays in which all wave lengths which produce a luminous sensation are represented.

But if now I introduce into the electric arc a small piece of a volatile metal you see no longer a continuous band of light. The band is broken up into different parts. Narrow bands or lines of different colours are separated by a space sometimes black, sometimes slightly luminous. The metal has been converted into vapour by the great heat of the electric current, and the vibrations of its molecules take place in distinct periods, so that the waves emanating from it have certain definite lengths. If the molecule could only send out one particular kind of waves, I should in its spectrum only see one single line. We know of no body which does so, though we know of several in which the possible periods of vibration are comparatively few; the spectrum of these will therefore contain a few lines only. Thus we have two different kinds of spectra, continuous spectra and line-spectra. But there is a certain kind intermediate in appearance between these two. The spectra of "fluted bands," as they are called, appear, when seen in spectroscopes of small dispersive powers, as made up of bands, which have a sharp boundary on one side and gradually fade away on the other. When seen with more powerful instruments each band seems to be made up of a number of lines of nearly equal intensity which gradually come nearer and nearer together as the sharp edge is approached. This sharp edge is generally only the place where the lines are ruled so closely that we can no longer distinguish the individual components. The edge is sometimes towards the red, sometimes towards the violet end of the spectrum. Occasionally, however, the fluted bands do not show any sharp edge whatever, but are simply made up of a series of lines which are, roughly speaking, equidistant. No one who has seen a spectrum of fluted bands can ever fail to distinguish it from the other types of spectra which I have described.

What, then, is the cause for the existence of these different types? The first editions of text-books in which our science was discussed stated that a solid or liquid body gave a continuous spectrum, while a gaseous body had a spectrum of lines; the spectra of bands were not mentioned. The more recent editions give a few exceptions to this rule, and the editions which have not appeared yet, will—so I hope, at least—tell you that the state of aggregation of a body does not directly affect the nature of the spectrum. The important point is not whether a body is solid, liquid, and gaseous, but how many atoms are bound together in a molecule, and how they are bound together. This is one of the teachings of modern spectroscopy. A

molecule containing a few atoms only gives a spectrum of lines. Increase the number of atoms and you will obtain a spectrum of fluted bands; increase it once more, and you will obtain a continuous spectrum. The scientific evidence for the statements I have made is unimpeachable. In the first place, I may examine spectra of bodies which I know to be compound. Special precautions often are necessary to accomplish this purpose, for too high a temperature would invariably break up the compound molecule into its more elementary constituents. For some bodies I may employ the low temperature of an ordinary Bunsen burner. With others, a weak electric spark taken from their liquid solutions will supply a sufficient quantity of luminous undecomposed matter to allow the light to be analysed by a spectroscope of good power. The spectrum of a compound body is never a line spectrum. It is either a spectrum of bands or a continuous spectrum. The spectra of the oxides, chlorides, bromides, or iodides of the alkaline earths, for instance, are spectra of fluted bands. All these bodies are known to contain atoms of different kinds, the metallic atoms of calcium, barium, or strontium, and the atoms of chlorine, bromine, iodine, or oxygen.

But to obtain these spectra of bands we need not have necessarily recourse to molecules containing different kinds of atoms. Elementary bodies show these spectra, and we must conclude therefore that the dissimilarity of the atoms in the molecule has nothing to do with the appearance of the fluted bands. Similarity in the spectrum must necessarily be due to a similarity in the forces which bind the atoms together, and this at once suggests that it is the compound nature of the molecule which is the true cause of the bands, but that the molecule need not be necessarily a compound of an atom with an atom of different kind, for it may be a compound of an element with itself. We have ample proof that this is the true explanation of the different types of spectra. I shall presently give you a few examples in support of the view which is now nearly unanimously adopted by spectroscopists.

I have hitherto left unmentioned one important method of investigating the periods of molecular vibrations, a method which is applicable to low temperatures. If I have a transparent body and allow light sent out by a body giving a continuous spectrum to fall through it, I often observe that the transparent body sifts out of the light falling through it certain kind of rays. Spectra are thus produced which are called absorption spectra, because the body which is under examination does not send out any light, but absorbs some vibrations which are made to pass through it. It is an important fact that a molecule absorbs just the rays which it is capable itself of sending out. I can therefore investigate the spectrum of a body just as well by means of the absorption it produces as by means of the light which it sends out.

Vapours like bromine or iodine examined in this way give us a spectrum of fluted bands. A powerful spark in these gases gives,

however, a line-spectrum. Here, then, a change of spectrum has taken place. The same body at different temperatures gives us a different spectrum, and the change which takes place is the same as that observed in the spectrum of a compound body the moment the temperature has risen sufficiently to decompose that body. I conclude from spectroscopic observations, therefore, that the molecules of bromine and iodine just above their boiling-point are complex molecules, which are broken up at the temperature of the electric spark. At high temperatures the molecules of these bodies contain a smaller number of atoms, and it follows from this that the gases must be lighter or that their density must be smaller. These conclusions, which on spectroscopic grounds have been definite and clear for some years, have recently, by independent methods, been confirmed by Victor Meyer and others. It has been directly proved that at high temperatures the molecules of iodine and bromine contain a smaller number of atoms than they do just above their boiling-point. In other cases the change of density has not been directly proved, only because these necessary measurements are difficult or even impossible at very high temperatures, but we may be perfectly sure that chlorine, as well as the metallic vapours of silver, sodium, potassium, &c., which show an analogous change of their spectra, will ultimately be proved to undergo a change of density at high temperatures.

As we can trace the change from a line-spectrum to a band-spectrum taking place simultaneously with an increase of density, so may we follow the change from a band-spectrum to a continuous spectrum indicating the formation of a molecule still more complex.

Sulphur vapour, at a temperature just above its boiling-point, contains three times the number of atoms in one molecule that it does at a temperature of a thousand degrees. The spectrum of sulphur vapour observed by absorption is continuous when the heavier molecule only is present. At the higher temperatures, when each molecule is decomposed into three, the spectrum belongs to the type of fluted band-spectra. From the cases in which we can thus prove the change in the spectra and in the densities to go on simultaneously, we are justified in concluding that also in other cases, where no such change of density has yet been observed, it yet takes place; and it is not a very daring generalisation to believe that a change in spectra is always due to a change in molecular arrangement, and generally, perhaps always, accompanied by a change in the number of atoms which are bound together into one molecule.

With regard to the well-known statement, that solids and liquids give continuous spectra, while gases give line-spectra, it must be remarked that metallic vapours show in nearly all cases a continuous spectrum before they condense. Oxygen gives a continuous spectrum at the lowest temperature at which it is luminous. Examining liquids and solids by the method of absorption, we find that many of them show discontinuous spectra, presenting fairly narrow bands. It is not denied that the nearness of molecules does not affect the spectrum. It

may render the bands more wide and indistinct at their edges, but its influence is more of a nature which in gas spectra is sometimes observed at high pressures when the lines widen, and does not consist of an alteration in type. Though in a solid or liquid body the molecules are much nearer together, they are less mobile; and hence the number of actual collisions need not be necessarily much increased. The fact that a crystal may show a difference in the absorption spectrum according as the vibrations of the transmitted light take place along or across the axis, shows, I think, that mutual impacts cannot much affect the vibrations, but that each molecule, at least in a crystal, must be kept pretty well in its place.

We have divided spectra into three types, but in all attempts at classification we are met by the same difficulty. The boundaries between the different types are not in all cases very well marked. Every one will be able to distinguish a well-defined band-spectrum from a line-spectrum, but there are spectra taking up intermediate positions both between the line- and band-spectra and between band-spectra and continuous spectra. With regard to these it may be difficult to tell to which type the spectrum really belongs. It may happen that a change of spectrum takes place, the spectrum retaining its type; but in these cases, as a rule, the more complex molecule will have a spectrum approaching the lower type, although it may not actually belong to that lower type. To be perfectly general, we may say that a combination of atoms always produces an alteration in the spectrum in the direction of the change from the line-spectrum, through the band-spectrum to the discontinuous spectrum.

If we accept the now generally received opinion as to the cause of the different types of spectra, we may obtain information on molecular arrangement and complexity where our ordinary methods fail. At high temperatures, or under much diminished pressure, measures of density become difficult or impossible; and it is just in these cases that the spectroscope furnishes us with the most valuable information. If we find three spectra of nitrogen and the same number for oxygen, we must accept the verdict, and conclude that these gases can exist in three different allotropic states.

Amongst the remarkable phenomena observed in vacuum tubes, perhaps not the least curious is the spectrum observed at the negative pole, which in several cases is only observed there, and under ordinary circumstances in no other part of the tube. Both oxygen and nitrogen have a spectrum which is generally confined to the negative glow. Some years ago I tried to prove that also in these cases we have only to deal with a special modification of the gases which, curiously enough, only exists near the negative pole, and is broken up and decomposed in every other part of the tube. The experiments I then made seem to me to prove the point conclusively. After a current of electricity had passed through the tube for some time in one direction, the current was suddenly reversed; the negative pole now became positive, but the spectrum still was visible for some time in its neigh-

bourhood, and only gradually disappeared. This experiment shows that the spectrum may exist in other parts of the tube, and that it is therefore due to a peculiar kind of molecule, and not to anything specially related to electric phenomena taking place in the neighbourhood of the negative pole. Other experiments supported this view.

The classification of spectra, according to the complexity of the vibrating molecule, is of great theoretical importance; for by its means we may hope to obtain some information on the nature of the forces which bind together the atoms into one molecule. Our whole life is a chemical process, and a great part of the mysteries of nature would be cleared up if we could gain a deeper insight into the nature of chemical forces. I believe no other line of investigation to be as hopeful in this respect as the one which examines directly the vibrations of the molecules which take place under the influence of these chemical forces. If we could find a connection between the vibrations of a compound molecule and the vibrations of the simpler elements which it contains, we should have made a very decided step in the desired direction. I need not say that various attempts have been made to clear up so important a point; but we have to deal with complicated forces, and the attempts have as a rule not been crowned with much success.

There are, however, a few exceptions, a few cases of greater simplicity than the rest, where we are able to trace to their mechanical causes, the spectroscopic changes which take place on chemical combination. These few and simple cases may serve as the fingerposts which show us the way to further research, and we may hope, to further success. To make the spectroscopic changes of which I am speaking clear to you, I must have recourse to the analogy between sound and light, and remind you of the fact that when the prongs of a tuning-fork are weighted its tone is lowered, which means that the period of vibration is increased, and consequently that the length of the wave of sound sent out is lengthened. Now, suppose a molecule or atom, the spectrum of which I am acquainted with, enters into combination with another. And suppose that the vibrations of the second molecule are weak or lie outside the visible range of the spectrum, then the most simple assumption which I could make would be that the addition of the new molecule is equivalent to an increase of the mass of the other. An increase of mass without alteration of the force of the molecule, will, as in the case of the tuning-fork, lengthen the period of vibration, and increase the wave length. If a case of that kind were actually to happen, I should observe the whole spectrum shifting towards the red; and this is what is observed in the few simple cases to which I have referred. The first observation to that effect is due to Professor Bunsen, of Heidelberg. Examining the absorption spectra of different didymium salts, he found such a close resemblance between them, that no difference could be detected with instruments of small powers; but with larger instruments it was found that the bands varied slightly in position, that in the

chloride they were placed most towards the blue end of the spectrum, that when the sulphate was substituted for the chloride, a slight shift towards the less refrangible end took place, and that a greater shift in the same direction occurred on examining the acetate. Professor Bunsen remarks that the molecular weight of the acetate is larger than that of the sulphate, and that the molecule of the sulphate again is heavier than that of the chloride. He adds: "These differences in the absorption spectra of different didymium compounds cannot in our present complete state of ignorance of any general theory for the absorption of light in absorptive media, be connected with other phenomena. They remind one of the slight gradual alterations in pitch which the notes from a vibrating elastic rod undergo when the rod is weighted, or of the change of tone which an organ pipe exhibits when the tube is lengthened." The accompanying woodcut, copied from Professor Bunsen's paper, may serve to illustrate the shift observed in one of the absorption bands.

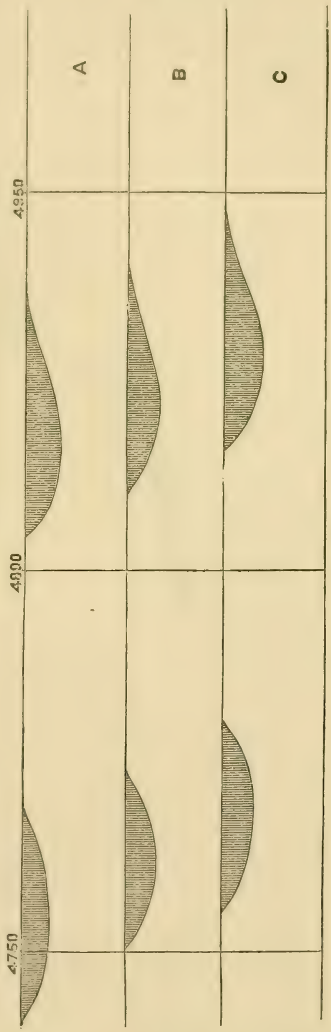


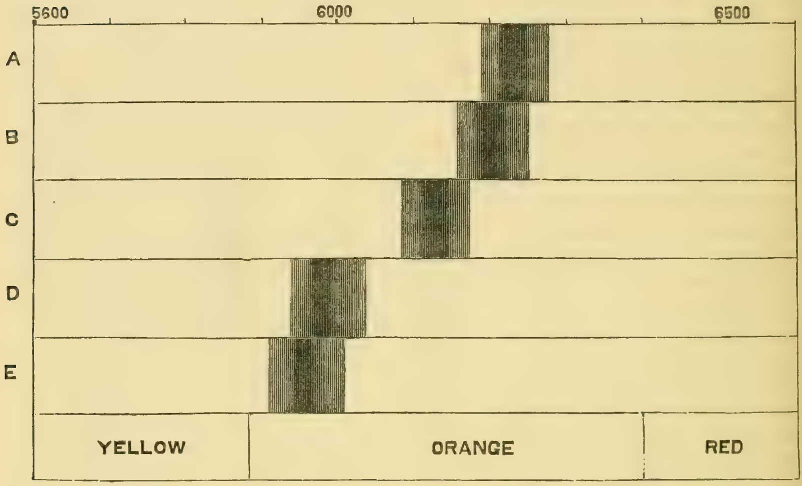
Fig. 1.

A = Absorption Bands in the Blue of Solution of Didymium Chloride.
B = " " " Sulphate.
C = " " " Acetate.

Similar changes take place when some substances like cyanin and chlorophyll are dissolved in different liquids. Absorption bands characteristic of these various substances appear, but they slightly vary in position. Professor Kundt, who has carefully examined this displacement of absorption bands, has come to the conclusion that as a rule the liquids of high dispersive powers were those which shifted the bands most towards the red end

of the spectrum. But though there is an apparent tendency in this direction, no rule can be given which shall be absolutely true whatever the substance which is dissolved. Fig. 2 shows the absorption spectrum of cyanin when dissolved in different liquids. The measurements made by Claes * are employed. We have here an interesting

Fig. 2.



A = Absorption of Cyanin in Bisulphide of Carbon.
B = " " Nitrobenzene.
C = " " Benzene.
D = " " Ether.
E = " " Alcohol.

proof that a solution is sometimes much more of a chemical compound than is generally supposed. The solvent and the substance must, indeed, be closely connected in order to produce a shifting of the absorption band. On the other hand, it is not astonishing that no general law can be given which connects the displacement with the physical properties of the solvent, for the closeness of connection depending on the special chemical affinity for each solvent has as much to do with the amount of shifting observed, as the molecular weight or the dispersion or refractive power may have. The shifting of the absorption bands in different solutions of the same substances is only one of many applications of spectroscopes to the examination of molecular phenomena in liquids. Into the interesting researches of Professor Russell, who has greatly extended this field of inquiry, we have no time to enter.

* Wied. Ann., iii. p. 388, 1878.

The changes of spectra due to molecular combinations and re-arrangements have in addition to their theoretical importance a great practical interest, for they will afford us some day a means of answering approximately a great many questions relating to the temperature of sun and stars. The gases and vapours in the solar atmosphere are for the greater part in the molecular condition in which they give a line-spectrum, and we know of stars the spectra of which resemble our solar spectrum very nearly. We shall not be far wrong in ascribing to such stars a temperature similar to that of our sun. Other stars have absorbing envelopes showing spectra of fluted bands. We know that fluted bands belong to a more complex molecular condition, which only can exist at lower temperatures. These stars, therefore, must have a lower temperature than our sun. Dr. Huggins, who has succeeded in obtaining most valuable photographs of star-spectra, has been able to classify and arrange star-spectra; and it is more than likely that in the series of stars arranged in order by means of their spectra, we have at one end those of the highest, at the other those of the lowest temperature. We are as yet far from being able to assign any particular temperature to a star, but the question by means of the spectroscope has been reduced to one which can be decided in our laboratories, and however difficult it may be, we may rest assured that it will ultimately be solved. As to our sun, its temperature has been the subject of many investigations. Attempts have been made to deduce it (at least approximately) from the amount of heat it sends out. Different experimental laws have been proposed to connect together the heat radiation of a body, and the temperature of that body. The first law which was thus proposed gives ten million degrees Centigrade as a lower limit; the second law reduces that lower limit to a little over 1500 degrees. Both these laws we now know to be wrong. More accurate laws give something like ten or twenty thousand degrees, but the whole method employed is one which is open to a great many objections.

We measure the combined heat radiation of different layers on the solar surfaces, all of which are at different temperatures, and we observe only an average effect which is much influenced by the absorption in the outer layers of the solar atmosphere and in the corona, and does not admit of easy interpretation. The spectroscopic method, which is yet in its infancy, has the advantage that we can observe separately each layer of the sun, and we thus examine the temperature not as an average, but for every part of the solar body. Our way to proceed would consist in carefully observing the spectra in different layers of the sun. Supposing we observe a change at one point, we may investigate at what temperature that change takes place, and we may then ascribe the same temperature to that particular place at the solar surface, if no other cause has interfered which may have affected our result. This last conditional limitation leads us to the discussion of the important but difficult question, whether we can determine any such interfering cause, which, not being temperature,

yet produces the same change in a spectrum which we have hitherto only ascribed to changes of temperature.

I must here remark that a change in type is not the only spectroscopic change in the spectrum which is observed to take place on varying the temperature. Line spectra especially are subject to curious variations in the relative intensities of their lines. These variations follow no general rule, and must be investigated separately for each element. The cause of this variation is a subject on which there exists a great difference of opinion; but, whatever this cause may be, if the changes always take place at one fixed temperature, we can turn them into account in measuring that temperature. However strong our wish that such a spectroscopic measurement of temperature may ultimately be obtained, a remarkable complication of facts has delayed the realisation of this hope for at least a considerable period of time.

We have to enter partly into a theoretical question, and I must necessarily allude to some of the facts recognised by all who believe in the molecular theory of gases. Each molecule, which, as we have seen, sends out rays of light and heat on account of its internal motion, is surrounded by other molecules. These are, indeed, very closely packed, and continually moving about with enormous velocities. Generally they move in straight lines, but it must necessarily happen that often they come very near, and then affect and deflect each other. Perhaps they come into actual contact, perhaps they repel each other so strongly when near, that contact never takes place. The time elapsing between two such collisions is very small. If you can imagine one second of time to be magnified to the length of a hundred years, it would only take about a second, on the average, from the time a molecule has encountered one other molecule until it encounters the second. During the greatest part of this very short time, it moves in a straight line, for the forces between molecules are so small that they do not affect each other, unless their distance is exceedingly small. It is, therefore, only during a very small fraction of time that one molecule is under the influence of another, and it is one of the greatest problems of molecular physics to find out what happens during that short element of time. I should like to explain to you how I believe the spectroscope may contribute its share to the settlement of that question. In his first great paper on the molecular theory of gases, the late Professor Clerk Maxwell assumed that two molecules may actually come into contact, that they may strike each other, as two billiard balls do, and then separate, according to the laws of elastic bodies. This theory is difficult of application when a molecule contains more than one atom, and especially as it did not in the case of conduction of heat give results ratified by the experimental test, Maxwell abandoned it in favour of the idea that molecules repel each other according to the inverse fifth power of the distance. This second theory not only gave what at the time was believed to be the correct law for the dependence of the coefficient of conduction on temperature, but it also helped its author over a considerable mathe-

matical difficulty. Further experiments have shaken our faith in the first of these two reasons, and the second is not sufficient to induce us to adopt without further inquiry the new law of action between two molecules.

It is exceedingly likely that the forces acting between two molecules when they are in close proximity to each other are partly due to, or at least modified by the vibrations of the molecules themselves. Such vibrations must, as in the case of sound, produce attractive and repulsive forces, and vibrating molecules will affect each other in a similar way as two tuning-forks would. Now, if the forces due to vibrations play any important part in a molecular encounter, the spectroscope will, I fancy, give us some information. If two molecules of the same kind encounter, the periods of vibration are the same, and the forces due to vibration will remain the same during, perhaps, the whole encounter. If two dissimilar molecules encounter, the relative phase of the vibrations, and hence the forces, will constantly change. Attraction will rapidly follow repulsion, and the whole average effect will be much smaller than in the case of two atoms of the same kind. We have no clear notion how such differences may act, and we must have recourse to experiment to decide whether any change in the effect of an encounter is observed when a molecule of a different kind is substituted for a molecule having the same periods of vibration.

When a body loses energy by radiation, that energy is restored during an encounter; the way in which this energy is restored will profoundly affect the vibrations of the molecule, and hence the observed spectrum. I have endeavoured by means of theoretical considerations, or speculations, as you may perhaps feel inclined to call them, to lead you on to an experimental law which I believe to be of very great importance. The spectrum of a molecule is in fact variable at any given temperature, and changes if the molecule is surrounded by others of different nature.

Placing a molecule in an atmosphere of different nature without change of temperature produces the same effect as would be observed on lowering of temperature.

Let me give you one example. Lithium at the temperature of the Bunsen flame has almost exclusively one red line in its spectrum. At the high temperature of the arc or spark the red line becomes weak, and almost entirely disappears. It is replaced by a strong orange line, which is already slightly visible, though weak, at low temperatures, and by additional green and blue lines.

But even at the high temperature of the spark we may obtain again a spectrum containing the red line only if we mix a small quantity of lithium with a large quantity of other material. The same spark, for instance, will give us the low temperature spectrum of lithium when taken from a dilute solution of a lithium salt, and the high temperature spectrum when that solution is concentrated.

The spectra of zinc and tin furnish us other examples in the same direction, but the spectra of nearly all bodies show the same law in a more or less striking way.

If this law which I have given you is a true one,* and I believe it will stand any test to which no doubt it will be subjected, we shall be able to draw some important conclusions from it. In the first place, it will be proved that the forces between atoms do depend on their vibrations. If this is true, any change in the vibrations of the spectrum, however small, will entail a corresponding change in all the other properties of the body. On the other hand, any change in the affinities of the element observed by other means will be represented by a change in the spectrum.

It is also possible that the introduction of forces due to vibratory motion will help us over a considerable difficulty in the molecular theory of gases. Some of the conclusions of that theory are at present absolutely contrary to fact. A spectroscopist, for instance, who is acquainted with the mercury spectrum and all the changes in that spectrum which can take place, feels more than sceptical when he is told that the molecule of mercury contains only one atom, which neither rotates nor vibrates.

Nor can it be of advantage to science to pass silently over this difficulty, or to neglect it as unessential, as is often done by modern writers. The late Professor Maxwell, at least, was well aware of its importance, and has often expressed in private conversation how serious a check he considered the molecular theory of gases to have received. This is not the place to enter more fully into this point and to consider how the vibratory forces may affect some of the suppositions on which the theoretical consequences are founded.

However important the effects of concentration or dilution on the spectra may be, they render the spectroscope less trustworthy as a thermometric instrument; for if the company in which a molecule is placed changes the spectrum in the same way as temperature would, it will be difficult to interpret our results. But although the discussion of our observations may be rendered more arduous and complicated, we need not on that account despair. It is one of the problems of spectroscopy to find out the composition of bodies, not only qualitatively, but also quantitatively, and when we shall know in what proportion different bodies are distributed in the sun, we may

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* Lockyer, 'Studies in Spectrum Analysis,' p. 140, draws attention to the fact that an admixture of a second element dims the spectrum of the first, and he expresses this fact by saying: "In encounters of dissimilar molecules the vibrations of each are damped." Later he has shown that the lines of oxygen and nitrogen, which are wide at atmospheric pressure, thin out when the gases are only present in small quantities. Lecoq de Boisbaudran in his 'Atlas' gives several examples of the differences in the relative brilliancy of lines produced by concentrating or diluting the solution from which the spark is taken. The complete parallelism of this change to the changes produced by increased temperature has, however, never received sufficient attention.

reduce the problem of finding out this temperature to the much simpler one of finding out the temperature of a given electric spark.

I hope that the few facts which I have been able to bring before you to-night have given you some idea of the important questions which have been brought under the range of spectroscopic research. Many of these questions still await an answer, some have only been brought into the preliminary stage of speculative discussion, but the questions have been raised, and the student of the history of science knows that this is an important step in its development and progress. The spectrum of a molecule is the language which that molecule speaks to us. This language we are endeavouring to understand. The unexperienced in a new tongue which he is trying to learn does not distinguish small differences of intonation or expression. The power over these is only gradually and slowly acquired. So it is in our science. We have passed by, and no doubt still are passing by, unnoticed differences which appear slight and unimportant, but which when properly understood will give us more information than the rough and crude distinctions which have struck us at first. We have extended our methods of research; we have extended our power over the physical agents; we can work with the temperature of sun and stars almost as we can with those in our laboratories. No one can foretell the result, and perhaps in twenty years' time another lecturer will speak to you of a spectroscopy still more modern in which some questions will have received their definite answer, and by which new roads will have been opened to a further extension of science.

[A. S.]

WEEKLY EVENING MEETING,

Friday, February 4, 1881.

THOMAS BOYCOTT, M.D. F.L.S. Vice-President, in the Chair.

DR. ANDREW WILSON, F.R.S.E. &c.

The Origin of Colonial Organisms.

EVERY animal develops, directly or indirectly, from an "ovum" or egg, and the plant springs, directly or indirectly, from the germ or seed. One chief difference between low and high forms of life consists in the fact that the development of the former ceases at a stage when the development of the latter has barely begun. The *Gregarina* is a microscopic speck of protoplasm living parasitically within the bodies of earthworms and other Articulated animals. When development takes place, the body becomes oval, develops a wall or cyst, and the internal protoplasm breaks up into small spindle-shaped masses. The body then ruptures, and the small segments escape, each to become a Gregarina, without further change, save the development of a nucleus. Each Gregarina at first appears as a single animal or *persona*, which converts itself by segmentation into an aggregation of such beings. There is thus a temporary development of a compound or colonial state. Similarly the *Amœbæ* (which are low protozoa, living in stagnant water and infusions, and moving as do the white corpuscles of our blood by emitting *pseudopodia*, or processes of their protoplasmic substance), when undergoing development, exhibit segmentation or internal division of their substance, and thus exhibit a compound state as a transitory feature of their reproductive phases.

It is noteworthy that in developing from the egg the embryos of all higher animals exhibit a like process of segmentation or division, as a preliminary phase of their reproduction. There are also forms of protozoa—*Myxodictyum*—which are truly "colonial" as adults, and which consist of masses of protoplasm aggregated together to form compound organisms. The Foraminifera are likewise "colonial"; since the shells of these minute protozoa exhibit, as a rule, a division into chambers, each occupied by a distinct protoplasmic unit, organically connected to its neighbours from which it was produced by budding.

The *Volvox globator*, formerly known as the "Globe animalcule," but now ascertained to be a free-swimming lower plant, is composed of distinct units, each provided with two cilia, and resembling a *Chlamydomonas.* Volvox is, in fact, a colony of monads. A Sponge is a compound or "colonial" organism, in that it consists of an aggre-

gation of protoplasmic units, some of which resemble Amœbæ in nature, whilst others resemble Chlamydomonads. The protoplasmic units of a sponge-colony are, as a rule, united together by a common skeleton they have helped to elaborate. Each sponge grows from an egg, the process of reproduction by "budding" being also represented in the group. Two *Spongillæ*, or common fresh-water sponges, will unite if placed in contact, or may separate spontaneously. The sponge arising from an egg, like a higher animal, thus exhibits segmentation and segregation of its parts, and comes to retain this segregate and colonial nature as a permanent feature of their race.

The Hydræ of the fresh-water pools, lead us to a type of animals nearly related to the sponges. Each is a tubular animal which may be artificially divided, and which throws off *gemmæ* or buds naturally. Each Hydra-bud grows into the exact likeness of its parent, and ultimately detaches itself from the parent body.

The zoophytes are simply *Hydræ* which have budded, but whose buds remain permanent to form a veritable tree, whose growth is ever increasing, and through whose branches a continual store of nutriment is continually circulating. Many zoophytes produce eggs which simply and directly develop into the compound adults by budding. Others develop eggs through the media of jelly-fish or medusoid-buds, which break away from the parent tree and live an independent existence in the sea. In some zoophytes there may be seven different kinds of units in the colony, all referable, however, to one type.

A *Flustra*, or "sea mat," grows upon shells and resembles a piece of pale brown seaweed. Each organism is an animal colony, but its units, which may number several thousands in one organism, are not structurally connected together like those of the zoophytes, but are contained each in a separate cell.

The *Tæniada* or tapeworms consist each of a linear series of similar "joints." Each "joint" is in reality comparable to the unit of zoophyte or "sea mat," for it is essentially a distinct member of a colony, and possesses a complete set of generative and other organs, and is produced from the head and neck by budding. According to Haeckel, starfishes and sea urchins are each compound or "colonial" animals. Structurally, it is provable that each ray of a starfish corresponds with worm-structure in broad details. The Naïs and other fresh-water worms produce young forms by a new head being budded out amongst their joints. There is here seen a tendency to become doubly "colonial"; inasmuch as the single worm is typically a "colonial" animal, and the new head-development causes this compound body to detach a new colony.

Amongst insects, the *Aphides*, or plant-lice, produce by veritable "budding" new generations, and the queen bee does not fertilize those eggs which are destined to become "drone" bees. Thus, the homology of an egg with a "bud" appears demonstrable.

It is the business of philosophy to correlate and arrange facts to form a harmonious and scientific system. The philosophy of biology

leads us firstly to define an "individual" structurally as a being whose parts and organs are so closely and intimately connected, that separation of even a limited structural area means disintegration of the individual as a whole. Physiologically, an "individual" animal or plant is the *total* development of a *single* egg or seed. As the whole zoophyte, sea mat, and tapeworm arise, each from a single egg, each, *in toto*, is an "individual." The separate units of each are named "zoöids." A new personality does not enter into the life cycle of any animal or plant, until a new egg or seed has been produced. Even in the case of the *Hydra*, although the buds become detached and to all appearances are each as truly an "individual" as their parent, they possess nevertheless no true personality. They are merely units or *zoöids* of a colony; they were produced by budding, and as such are not "individuals" but parts of an "individual." If we assumed that the buds of a zoophyte or tapeworm were "individuals," we might with equal correctness speak of the joints of a lobster or worm as "individuals" likewise. Even in human structure itself, there are to be seen traces of a fundamentally "colonial" nature. The tissues of the highest animals are but aggregations of cells. As such, they have a semi-independent constitution; and there are certain protoplasmic cells (e. g. the white or amœboid corpuscles of the blood) which roam independently at will through the body, and possess powers of movement exactly resembling those of the *Amœba* and its kind.

A progressive tendency according to the theory of evolution marks the organic series. The conversion of the "colony" into the "individual," in other words the concentration of originally separate and independent "units" or "zoöids," is the chief developmental cause of the differences between high and low organisms. The primitive condition of all organisms is the "colonial" condition. Egg-segmentation (or in *Protozoa* body-division) is universal in the animal world; and cell-multiplication begins the development of plant life likewise. Arrest of development at an early stage distributes the separate units thus formed (as in *Gregarina*); arrest at a later stage gives us the sponge colony (a series of similar aggregated cells), or the tapeworm colony (a collection of essentially similar "joints"). Physiologically, the higher organism devotes less time to pure reproduction and becomes more explicitly busied with individual interests. Hence the increased concentration of energy which results in the formation of the highest "individuals," that yet retain, in the "colonial" and cellular structure of their tissues, the evidence of an originally compound nature.

In the plant world, such "individualization" is seen as a secondary tendency in the close aggregation of flowers in *Compositæ*, and in the transformation of uniform composites (e. g. thistle) into individualized forms (e. g. daisy) through such intermediate steps as the centauries.

The conclusions of our study of "colonial" organisms are as

follows: 1. The original condition of organisms is colonial; the universal segmentation of the egg is a proof of this inference, and the development of new forms by this so-called process in low forms like gregarinæ, &c., supports this conclusion. 2. The lower we proceed in the scale of being the more marked is the tendency to form "colonial" organisms. 3. Arrest of development, by causing an organism to cease progressing at a segregated stage, will tend to produce a "compound" and "colonial" constitution. 4. The plant world is "colonial" in its highest types. Plant development has not proceeded towards any marked increase of "individuality" over the colonial nature of lower forms. A tree is in many respects as markedly "colonial" as a volvox. 5. The highest animals exhibit lingering traces of an originally "colonial" nature in their histological composition. 6. The tendency of life-development is towards concentration, and the conversion of the "colony" into the true "individual."

It is suggested by way of final inference, and by way of incursion into a biological byepath, that the theory and idea of an originally "colonial" constitution may explain the existence in man and higher animals generally of those tribal and family associations which mark the upper strata of existence. The semi-independent action of many parts of the higher brain, for instance, receives an explanatory hint as to causation, from the idea of an originally independent and colonial constitution.

[A. W.]

GENERAL MONTHLY MEETING,

Monday, February 7, 1881.

William Bowman, Esq. LL.D. F.R.S. Vice-President, in the Chair.

Miss Adalbertha F. Dubort,
Robert Hamilton Few, Esq. F.R.G.S.
Thomas Gabriel, Esq.
Mrs. Caroline Gabriel,
Mrs. Elizabeth F. Hight,
John Henry Knight, Esq.
Edward W. Lane, M.D. M.A.
Walter Farquhar Larkins, Esq.
Lieut.-Col. Llewellyn Wood Longstaff, F.R.G.S. F.Z.S.
Arthur Vacher, Esq.

were elected Members of the Royal Institution.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Deputy Master of the Mint—Report, 1879. 8vo. 1880.

Accademia dei Lincei, Reale, Roma—Atti, Serie Terza: Transunti: Tome V. Fasc. 1, 2, 3. 4to. 1880.
Asiatic Society of Bengal—Journal, Vol. XLVIII. Part I. Extra No. 8vo. 1880. Vol. XLIX. Part I. Nos. 2, 3. Part II. No. 2. 8vo. 1880.
Asiatic Society, Royal—Journal, New Series, Vol. XIII. Part 1. 8vo. 1881.
Astronomical Society, Royal—Monthly Notices, Vol. XLI. Nos. 1, 2. 8vo. 1880–1.
Memoirs, Vol. XLV. 4to. 1880.
Bankers, Institute of—Journal. Vol. I. Part 13. 8vo. 1880. Vol. II. Part 1. 8vo. 1881.
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Abhandlungen. Band XIII. 3te Abtheilung. 4to. 1880.
Dr. K. A. Littel: Testrede. 4to. 1880.
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Chemical Society—Journal for Jan. and Feb. 1881. 8vo.
Colebrook, John, Esq. M.R.I.—Joseph Haydn, Dictionary of Dates and Universal Reference. (1st Edition.) 8vo. 1841.
Crisp, Frank, Esq. LL.B. F.L.S. &c. M.R.I. (the Editor)—Journal of the Royal Microscopical Society, Series II. Vol. I. Part 1. 8vo. 1881.

Dax: Société de Borda—Bulletins, 2e Série, Cinquième Année: Trimestre 4. 8vo. Dax, 1879.
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Analyst for Dec. 1880 and Jan. 1881. 8vo.
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Chemical News for Dec. 1880 and Jan. 1881. 4to.
Engineer for Dec. 1880 and Jan. 1881. fol.
Horological Journal for Dec. 1880 and Jan. 1881. 8vo.
Iron for Dec. 1880 and Jan. 1881. 4to.
Nature for Dec. 1880 and Jan. 1881. 4to.
Revue Scientifique and Revue Politique et Littéraire, Dec. 1880 and Jan. 1881. 4to.
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Franklin Institute—Journal, Nos. 660-661. 8vo. 1880.
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Goodeve, T. M. Esq. (the Author).—The Elements of Mechanism. New Edition. 12mo. 1880.
Houzeau, J. C. et Lancaster, A. (the Authors)—Bibliographie Générale de l'Astronomie. Tome II. Fasc. I. 8vo. Bruxelles, 1880.
Liverpool Polytechnic Society—Journal: Dec. 1880. 8vo.
Mechanical Engineers, Institution of—Proceedings, August and Oct. 1880. 8vo.
Meteorological Office—Report of the Meteorological Council of the Royal Society for 1879-80. 8vo. 1881.
Meteorological Society—Quarterly Journal, No. 36. 8vo. 1880.
Middlesex Hospital—Reports for 1878. 8vo. 1880.
Midland Institute of Engineers—Transactions, Vol. VII. Part 51. 8vo. 1880.
Montpellier Académie des Sciences et des Lettres—Mémoires. Tome IX. Fasc 3. 4to. 1880.
National Association for Social Science—Sessional Proceedings. Vol. XIV. No. I. 8vo. 1881.
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WEEKLY EVENING MEETING,

Friday, February 11, 1881.

William Bowman, Esq. F.R.S. Vice-President, in the Chair.

Robert S. Ball, Esq. LL.D. F.R.S.
ASTRONOMER ROYAL OF IRELAND.

The Distances of the Stars.

Every one who is acquainted with the rudiments of astronomy knows that the sun with its attendant planets is merely an island group in the vast realms of space.

An island the size of this room in the middle of the Atlantic would be over a thousand miles from the coasts of Europe and America on either side. Yet that island would not be more remotely apart from the surrounding shores than is our solar system from the bodies which surround it in space. To determine the distance from this solar island to the stars which surround it, is the problem for our consideration to-night.

Recent Researches on 61 *Cygni.*

It is now almost exactly forty years (February 12, 1841) since the gold medal of the Royal Astronomical Society was awarded to Bessel for his discovery of the annual parallax of 61 Cygni. On that occasion Sir John Herschel delivered an address, in which he glanced at the labours of Struve and Henderson as well as Bessel. The discovery of the distances of the stars was alluded to as "the greatest and most glorious triumph which practical astronomy has ever witnessed." From this date the history of our accurate knowledge of the subject may be said to commence. Each succeeding race of astronomers takes occasion to investigate the parallax of 61 Cygni anew, with the view of confirming or of correcting the results arrived at by Bessel.

[The parallactic ellipse which the stars appear to describe, having been briefly explained, the method of deducing the distances of the stars was pointed out.]

The attention of Bessel was directed to 61 Cygni by its proper motion of five seconds per annum. When Bessel was at his labours in 1838, the pair of stars forming the double were in the position indicated on Fig. 1. When O. Struve undertook his labours in 1853 the pair of stars forming 61 Cygni had moved considerably, as is shown on the figure. Finally, when the star was observed at Dunsink in 1878, it had made another advance in the same direction as before. In forty years this object had moved over an arc of the heavens upwards of three minutes in length.

The diagram contains four other stars besides the three positions of 61 Cygni. These are but small telescopic objects, they do not parti-

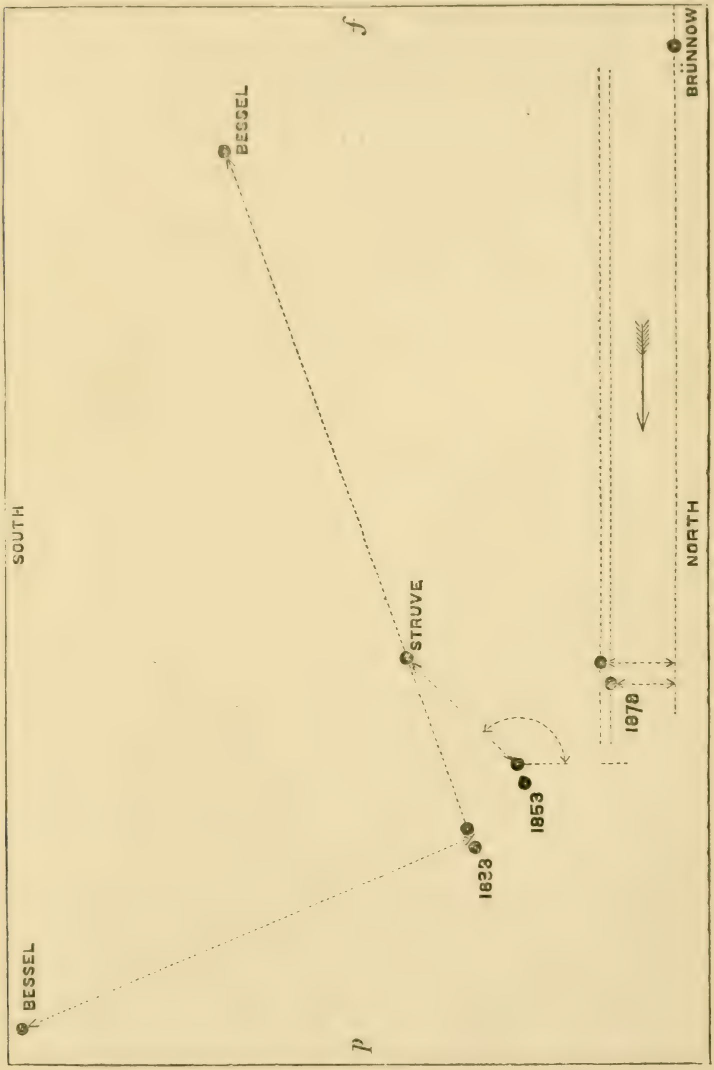


FIG. 1.—61 CYGNI AND PARALLAX COMPARISON STARS.

cipate in the large proper motion of 61 Cygni, and they may be presumed to be much more remote from us. Bessel chose as the

comparison stars the two objects marked with his name. He measured the distance from the central point of 61 Cygni to each of the two comparison stars. From a series of such measures he discovered the parallactic ellipse of 61 Cygni. He was led to the same ellipse by each of the two comparison stars.

Fifteen years latter (1853) Struve undertook a new determination. He chose a comparison star different from either of those Bessel had used. Struve's method of observing was also quite different from Bessel's. Struve made a series of measures of the distance and position of the comparison star from 61 (B) Cygni. Struve succeeded also in measuring the parallactic ellipse.

There was, however, an important difference between their results. The distance, according to Bessel, was half as much again as Struve found. Bessel said the distance was sixty billions of miles; Struve said it could not be more than forty billions.

The discrepancy may be due to the comparison stars. If Bessel's comparison stars were only about three times as far as 61 Cygni, while Struve's star was about eight or ten times as far, the difference between Struve's result and Bessel's would be accounted for.

To settle the question, observations were subsequently made by Auwers and others; the latest of these investigations is one which has recently been completed at Dunsink Observatory.

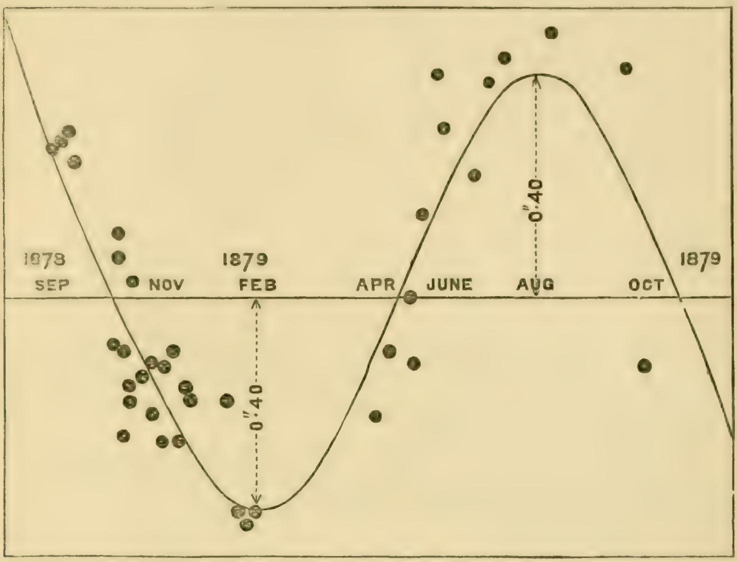
Dr. Brünnow commenced a series of measures of the difference in declination between 61 Cygni and a fourth comparison star. The carrying out of this work devolved on the lecturer, as Dr. Brünnow's successor. Two series of observations have been made, one with each of the components of 61 Cygni. The results agree very nearly with those of Struve.

On a review of the whole question, there seems no doubt that the annual parallax of 61 Cygni is nearer to the half second found by Struve, than to the third of a second found by Bessel.

To exhibit the nature of the evidence which is available for the solution of such a problem, a diagram showing the observations has been prepared. In the accompanying Fig. 2 the abscissæ are the dates of the second series of observations made at Dunsink. The ordinates indicate the observed effect of parallax on the difference of declinations between 61 (B) Cygni and the comparison star. Each dot represents the result of the observations made on the corresponding night. The curve indicates where the observations should have been with a parallax of $0''\cdot 47$. The discordances seem in many cases very considerable. They are not, however, intrinsically so great as might perhaps be at first thought. The distance from the top of the curve to the horizontal line represents an angle of four-tenths of a second. This is about the apparent diameter of a penny-piece at the distance of ten miles. The discordance between the observations and the curve is in no case much more than half so great. It therefore appears that the greatest error we have made in these observations amounts to but two or three tenths of a second. This is equivalent to

the error of pointing the telescope to the top edge of a penny-piece instead of to the bottom edge when the penny-piece was fifteen or twenty miles off.

FIG. 2.—PARALLAX IN DECLINATION OF 61 (B) CYGNI.



Ordinates indicate parallax.
Dots indicate observations.

Still, however, the entire quantity to be measured is so small that the errors, minute as they are, bear a large proportion to the parallax. In this lies the weakness of such work. By sufficiently multiplying the numbers of the observations, and by discussing them with the aid of the method of least squares, considerable confidence may be attached to the results.

Groombridge 1830.

This star has been the subject of much parallax work. It has a proper motion of seven seconds annually. Mr. Huggins or Mr. Christie could perhaps ascertain by the spectroscope what its motion may be in the line of sight. From the theory of probabilities it may not improbably be nine seconds. We shall, however, take it at seven seconds. The parallax has been determined by Struve and by Brünnow. It is very small, being one-tenth of a second. The actual velocity of 1830 Groombridge must therefore be 70 radii of the earth's orbit per annum, or 200 miles per second.

Newcomb has employed this result to throw light on the question

as to whether all our stars form one system. If an isolated body in our system is to remain there for ever, the theory of gravitation imposes the imperative condition, that the velocity of the body must not exceed a certain amount. Assuming that the stars are 100,000,000 in number, and that each star is five times as large as the sun, assuming also that they are spread out in a thin layer of such dimensions that a ray of light takes 30,000 years to pass it, Newcomb shows that the critical velocity is 25 miles per second.

As this is only the eighth-part of the velocity of Groombridge 1830, we are thus led to the dilemma that either the masses of the bodies in our system must be much greater than we have supposed, or Groombridge 1830 is a runaway star, which can never be controlled and brought back.

Search for Stars with Parallax.

The lecturer has been engaged for some years at Dunsink Observatory in a systematic search for stars which have an appreciable parallax. Up to the present about three hundred stars have been examined. In the majority of cases each of these stars has been observed only twice. The dates of the observations have been chosen so as to render the effects of parallax as manifest as possible. It is not of course expected that a small parallax of a few tenths of a second could be detected by this means.

The errors of the observations would mask any parallax of this kind. It seems, however, certain that no parallax could have escaped detection if it at all approached to that of α Centauri.

The stars examined have been chosen on various grounds. It had been supposed that some of the red stars were possibly among the sun's neighbours, and consequently many of the principal red stars were included in our list. No appreciable parallax has, however, been detected in any of the red stars up to the present. Many of the principal double stars are also included in the list. Other stars have been added on very various grounds, among them may be mentioned the Nova, which some time ago burst out in the constellation Cygnus, and dwindled down again to a minute point. The earth's orbit, however, does not appear any larger when seen from Nova Cygni than from any of the other stars on our list.

Groombridge 1618.

We have, however, found one star which seems to have some claim to attention as one of the sun's neighbours. The star in question is Groombridge 1618. It lies in the constellation Leo, and is 6·5 magnitude. Groombridge 1618 has a proper motion of $1''\cdot 4$ annually. From a series of measurements of its distance made on fifty-five nights from a suitable comparison star, the parallax of Groombridge 1618 appeared to be about one-third of a second. As this seemed to be a result of considerable interest, measures were renewed for a second series of forty nights. The result of the second series con-

firms the first. Measurements of the position angle were also made at the same time. Some difficulties not yet fully explained have arisen, but on the whole the measurements of the positive angle seem to confirm the supposition that the parallax of Groombridge 1618 is about one-third of a second. No doubt this is but a small quantity. The orbit of the earth viewed from Groombridge 1618 is about the same size as a penny-piece at the distance of seven miles.

Proper Motions of the Stars.

Geologists have made us acquainted with the enormous intervals of time which have elapsed since the earth first became the abode of living animals. Regarding a period of 50,000,000 of years as comparable with geologic time, some considerations were adduced as to the effect of proper motions during such an interval. It was pointed out that in all probability none of the stars now visible to the unaided eye can have then been visible from the earth.

The Nature of Space.

The possible connection of parallax work with the problems of the nature of space was then alluded to. It was shown that if space be hyperbolic the observed parallax is smaller than the true parallax, while the converse must be the case if space be elliptic. The largest triangle accessible to our measurements has for base a diameter of the earth's orbit, and for vertex a star. If the defect of such a triangle be in any case a measurable quantity, it would seem that it can only be elicited by observations of the same kind as those which are made use of in parallax investigations.

[R. S. B.]

WEEKLY EVENING MEETINGS.

Friday, February 18, 1881.

Thomas Boycott, M.D. F.L.S. Vice-President, in the Chair.

Sir John Lubbock, Bart. M.P. D.C.L. F.R.S. *M.R.I.*

Fruits and Seeds.

(The Discourse, with Illustrations, will be given in the next number of the *Proceedings.*)

Friday, February 25, 1881.

Thomas Boycott, M.D. F.L.S. Vice-President, in the Chair.

Dr. J. S. Burdon Sanderson, LL.D. F.R.S.

Excitability in Plants and Animals.

(Abstract deferred.)

WEEKLY EVENING MEETING,

Friday, March 4, 1881.

WILLIAM BOWMAN, Esq. F.R.S. Vice-President, in the Chair.

Sir WILLIAM THOMSON, LL.D. F.R.S. ETC.

Elasticity viewed as possibly a Mode of Motion.

WITH reference to the title of his discourse the speaker said: "The mere title of Dr. Tyndall's beautiful book, 'Heat, a Mode of Motion,' is a lesson of truth which has manifested far and wide through the world one of the greatest discoveries of modern philosophy. I have always admired it; I have long coveted it for Elasticity; and now, by kind permission of its inventor, I have borrowed it for this evening's discourse.

"A century and a half ago Daniel Bernouilli shadowed forth the kinetic theory of the elasticity of gases, which has been accepted as truth by Joule, splendidly developed by Clausius and Maxwell, raised from statistics of the swayings of a crowd to observation and measurement of the free path of an individual atom in Tait and Dewar's explanation of Crookes' grand discovery of the radiometer, and in the vivid realisation of the old Lucretian torrents with which Crookes himself has followed up their explanation of his own earlier experiments; by which, less than two hundred years after its first discovery by Robert Boyle, 'the Spring of Air' is ascertained to be a mere statistical resultant of myriads of molecular collisions.

"But the molecules or atoms must have elasticity, and *this* elasticity must be explained by motion before the uncertain sound given forth in the title of the discourse, 'Elasticity viewed as possibly a Mode of Motion,' can be raised to the glorious certainty of 'Heat, a Mode of Motion.'"

The speaker referred to spinning-tops, the child's rolling hoop, and the bicycle in rapid motion as cases of stiff, elastic-like firmness produced by motion; and showed experiments with gyrostats in which upright positions, utterly unstable without rotation, were maintained with a firmness and strength and elasticity as might be by bands of steel. A flexible endless chain seemed rigid when caused to run rapidly round a pulley, and when caused to jump off the pulley, and let fall to the floor, stood stiffly upright for a time till its motion was lost by impact and friction of its links on the floor. A limp disc of indiarubber caused to rotate rapidly seemed to acquire the stiffness of a gigantic Rubens' hat-brim. A little wooden ball which when thrust down under still water jumped up again in a moment, remained down as if embedded in jelly when the water was caused to rotate

rapidly, and sprung back as if the water had elasticity like that of jelly, when it was struck by a stiff wire pushed down through the centre of the cork by which the glass vessel containing the water was filled. Lastly, large smoke rings discharged from a circular or elliptic aperture in a box were shown, by aid of the electric light, in their progress through the air of the theatre when undisturbed. Each ring was circular, and its motion was steady when the aperture from which it proceeded was circular, and when it was not disturbed by another ring. When one ring was sent obliquely after another the collision or approach to collision sent the two away in greatly changed directions, and each vibrating seemingly like an indiarubber band. When the aperture was elliptic each undisturbed ring was seen to be in a state of regular vibration from the beginning, and to continue so throughout its course across the lecture-room. Here, then, in water and air was elasticity as of an elastic solid, developed by mere motion. May not the elasticity of every ultimate atom of matter be thus explained? But this kinetic theory of matter is a dream, and can be nothing else, until it can explain chemical affinity, electricity, magnetism, gravitation, and the inertia of masses (that is, crowds of vortices).

Le Sage's theory might easily give an explanation of gravity and of its relation *to inertia of masses*, on the vortex theory, were it not for the essential aeolotropy of crystals, and the seemingly perfect isotropy of gravity. No finger-post pointing towards a way that can possibly lead to a surmounting of this difficulty, or a turning of its flank, has been discovered, or imagined as discoverable. Belief that no other theory of matter is possible is the only ground for anticipating that there is in store for the world another beautiful book to be called "Elasticity, a Mode of Motion."

[W.T.]

GENERAL MONTHLY MEETING,

Monday, March 7, 1881.

THE DUKE OF NORTHUMBERLAND, D.C.L. LL.D. President, in the Chair.

Edward James Bevir, Esq. Q.C. M.A.
Francis Chalmers Crawford, Esq.
Mrs. Fanny Cutler,
The Hon. Cecil Duncombe,
Frederick Allen Gower, Esq.
Evan Hanbury, Esq.
Mrs. Isabella Ellen Leaf,
Paul Margetson, Esq.
Mrs. Marie Müller,
William Smith Norman, Esq.
Walter John Stanton, Esq. M.P.
William Tarn, Esq.
Alfred Tylor, Esq. F.G.S.

were elected Members of the Royal Institution.

The special thanks of the Members were voted to Mr. THOMAS FALL for his present of a life-sized Photograph Portrait of PROFESSOR FARADAY in a gilt frame.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

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Asiatic Society of Bengal—1880, Proceedings, No. 6. 8vo.

Astronomical Society, Royal—Monthly Notices, Vol. XLI. No. 3. 8vo. 1881.

Bashford, John L. Esq. M.A. M.R.I. (the Author)—Elementary Education in Saxony. (O 17) 16to. 1881.

British Architects, Royal Institute of—Proceedings, 1880–1. No. 11. 4to.

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Dax: Société de Borda—Bulletins: 2^e Série, Cinquième Année: Trimestre 4. 8vo. Dax, 1879.
Editors—American Journal of Science for Feb. 1881. 8vo.
Analyst for Feb. 1881. 8vo.
Athenæum for Feb. 1881. 4to.
Chemical News for Feb. 1881. 4to.
Engineer for Feb. 1881. fol.
Horological Journal for Feb. 1881. 8vo.
Iron for Feb. 1881. 4to.
Nature for Feb. 1881. 4to.
Revue Scientifique and Revue Politique et Litteraire, Feb. 1881. 4to.
Telegraphic Journal for Feb. 1881. 8vo.
Franklin Institute—Journal, No. 662. 8vo. 1881.
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Glasgow Philosophical Society—Proceedings, Vol. XII. No. 1. 8vo. 1879–80.
Holmes-Forbes, Avary W. Esq. M.A. M.R.I. (*the Author*)—The Science of Beauty: an Analytic Inquiry into the Laws of Æsthetics. 12mo. 1881.
Institute of Chemistry—Report on Standards of Strength and Purity, &c. 8vo. 1881.
Jordan, Mr. J. B. (*the Author*)—The Glycerine Barometer. (K 104) 8vo. 1881.
Lisbon, Sociedade de Geografia—Boletim: 2^e Serie, Nos. 1, 2. 8vo. 1880.
Manchester Geological Society—Transactions, Vol. XVI. Parts 2, 3. 8vo. 1880–1.
Mensbrugghe, M. Van der (*the Author*)—Voyages et Métamorphoses d'une Gouttelette d'Eau. (K 104) 8vo. 1880.
New South Wales, Royal Society—Journal and Proceedings, Vol. XIII. 8vo. 1880.
Annual Reports on the Department of Mines: for 1878 and 1879.
A. Liversedge: Report upon certain Museums. fol. 1880.
Pharmaceutical Society of Great Britain—Calendar for 1881. 8vo.
Journal, Feb. 1881. 8vo.
Photographic Society—Journal, New Series, Vol. V. No. 5. 8vo. 1881.
Physical Society of London—Proceedings, Vol. IV. Part 1. 8vo. 1881.
Royal Society of London—Proceedings, No. 208. 8vo. 1881.
Sanitary Institute of Great Britain—Transactions, Vol. II. and Calendar for 1881. 8vo. 1880.
St. Bartholomew's Hospital—Reports, Vol. XVI. 8vo. 1880.
St. Petersbourg, Académie des Sciences—Bulletins, Tome XXVII. No. 1. 4to. 1881.
Mémoires: Série VII. Tome XXVII. No. 13. 4to. 1880.
St. Petersburg Central Physical Observatory (*through Dr. H. Wild, Director*)—Annalen. 1879. 4to. 1880.
Sullivan, John M.D. (*the Author*)—The Endemic Diseases of Tropical Climates, with their Treatment. 12mo. 1877.
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Tidy, C. Meymott, M.B. F.C.S. M.R.I. (*the Author*)—Handbook of Modern Chemistry, Inorganic and Organic. 8vo. 1878.
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WEEKLY EVENING MEETING,

Friday, March 11, 1881.

William Spottiswoode, Esq. M.A. D.C.L. Pres. R.S. &c.
in the Chair.

Shelford Bidwell, M.A. LL.B. *M.R.I.*

Selenium and its applications to the Photophone and Telephotography.

Before entering upon my subject, I must claim your indulgence upon two grounds. A week ago I had not the remotest idea that I was to have the honour of addressing you here this evening; the time which I have had for preparation has, therefore, been exceedingly limited. In the second place, it is my desire (in accordance with the traditions of this Institution) not merely to give a description of the experiments in which I have for the last few months been engaged, but, as far as possible, to reproduce them before you. Now these experiments are mostly of a very delicate nature. In the quiet of a laboratory—where time is practically unlimited, and where an operation, if it should fail at first, may be repeated an indefinite number of times—success is tolerably certain to be finally obtained; but in exhibiting delicate experiments before an audience, one is working under the most unfavourable conditions, and, in case of failure in the first instance, the attempt cannot generally be repeated. Moreover, the substance with which we are chiefly concerned, selenium, is apparently extremely capricious in its behaviour. This appearance is, of course, really due to our present ignorance of its properties; but the fact remains that, on account of the great uncertainty of its action, it is a very difficult substance to deal with.

Selenium is a rare chemical element which was discovered in the beginning of the present century. In many of its properties it closely resembles sulphur, and, like sulphur and some other substances, it is capable of existing in more than one form.

The ordinary form is that called vitreous. Selenium in this condition is as absolutely structureless as glass, and in appearance resembles nothing so much as bright black sealing-wax, with, perhaps, somewhat of a metallic lustre; its real colour, however, when seen in thin films, is ruby red. Its melting-point is a little higher than 100° C. In its second modification selenium is crystalline. When in this form its surface is dull, its fracture is metallic (not unlike that of cast iron), its colour is grey or leaden, and it is

quite opaque to light; its melting-point also is considerably higher, being 217° C.

Vitreous selenium, if melted and kept for a certain length of time at a temperature between its own fusing-point and that of crystalline selenium, will crystallise; and I think I am right in saying, from casual observation, though I have made no experiments to verify the point, that the length of time necessary for crystallisation depends upon the degree of temperature, being proportionately shorter as the temperature approaches 217° C.

Vitreous selenium is an exceedingly bad conductor of electricity; it is, indeed, an almost perfect insulator. Crystalline selenium is a moderately good conductor, and it possesses this very remarkable property, which has been utilised in the photophone and other inventions, that it conducts better in the light than in the dark, the change in its resistance to the passage of a current of electricity through it varying, according to Professor W. G. Adams, as the square root of the illuminating power.

Let a galvanometer be connected to the two poles of a battery by means of two copper wires. The passage of a current of electricity will at once be denoted by the deflection of the magnetic needle; or, if a little mirror is attached to the needle, and a beam of light be reflected from it upon a scale, the movement of the spot of light will indicate the movement of the needle. Let now one of the wires be cut, and the two ends be joined together by a piece of crystalline selenium. The spot of light will again move, but its deflection will be very much less than it was before, showing that the resistance of the selenium is very much greater than that of the wire. Moreover, if the piece of selenium be alternately exposed to and screened from a beam of light, the deflection will be greater when it is in the light than when it is in the dark, showing a corresponding variation in its resistance. This remarkable property of selenium was first announced and exhibited by Mr. Willoughby Smith in 1873. But the effects produced by the simple arrangement which I have just described are small, and very delicate instruments are required for their observation.

Since that date several devices have been proposed for exaggerating the effect, but they all depend upon the fact that the amount of the variation increases with the extent of the selenium surface acted upon. It has lately been the fashion to call these arrangements "cells," which, in most cases at all events, seems to be a very inappropriate name. It has been suggested that they should be termed "rheostats," a name which well expresses the purposes for which they are generally used, and is less likely to lead to confusion than the other. In deference to custom, however, I shall to-night call them by the usual name.

The simplest selenium cell which could be devised, would be made by placing two short pieces of copper wire parallel to each other, and very near together, and connecting them by a narrow strip

of selenium. The effect produced by light is increased by lengthening this arrangement. We might go on increasing the wires with advantage until they were 10 or 12 feet long or more, every additional inch of length producing an increase of "sensitiveness" as it is called. But a cell of this length would be cumbersome and unwieldy to use, and in fact could hardly be lifted without being destroyed. Dr. Werner Siemens therefore adopted the device (among others) of coiling up the wires so as to form a double spiral, and thus made a convenient and portable cell of great sensitiveness. But it is very difficult indeed, as I know by experience, to produce these double wire spirals of any considerable size without the two wires touching one another at some point. After many attempts I succeeded in producing spiral cells about $\frac{3}{4}$ inch in diameter, but I found it impossible to exceed this size, and as it was not large enough I adopted a simple and very effective variation of Siemens's method, which I believe has not been previously suggested. A copper wire is wound after the fashion of a flat screw around a narrow slip of mica, the threads of the screw being about $\frac{1}{16}$ inch apart. Beside this wire, and at a distance of $\frac{1}{32}$ inch from it, a second wire is wound in exactly the same manner, each of its turns coming midway between two consecutive turns of the first. Care is taken that the two wires do not touch each other at any point. Over one surface of the mica a film of melted selenium is spread, and after being worked smooth and uniform it is crystallised. By this means the two wires are connected with each other through half their entire length, by a series of very narrow strips of crystalline selenium.

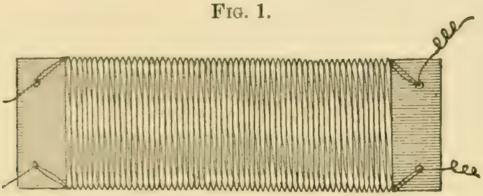
I have here a tiny selenium cell which has been constructed in this manner. Each wire makes about six turns, and the area of the selenium upon its surface is about half that of a threepenny piece, its thickness not much exceeding that of a sheet of ordinary note-paper. Its resistance, though it is very high relatively to that of good conductors, is, compared with anything of the kind that I had ever seen before, remarkably low, and its sensitiveness to light is great. When a batswing gas-flame is held at a distance of three inches from it, its resistance is less than one-third of that which it measures in the dark. Larger and more carefully constructed cells, of course, show better results. No form, however, that I have tried (and I have made several dozens) has in my hands been superior to that of a double flat screw.

When cells are to be made of any considerable size, the labour of winding on the wires with sufficient regularity is very great. By the help of a lathe this difficulty may be reduced to a minimum. The method of proceeding is this:—A cylinder is turned of hard wood, of length and diameter slightly greater than that of the proposed cell. This cylinder is cut longitudinally into two equal parts, and between the two semi-cylinders thus formed a slip of mica is placed sandwich-like. The ends are secured with screws, and the whole is smoothed down in the lathe. When the edges of the mica are quite flush with

the surface of the wood, a screw of from thirty to forty threads to the inch is cut upon the cylinder. On removing the mica from the wood its two edges are found to be beautifully and regularly notched. The first wire is then wound into alternate notches, and the second into the others.

I will throw upon the screen the image of a slip of mica with the two wires wound upon it, and ready for the reception of the selenium coating. It will be seen that the turns are perfectly regular, and close as the wires are to each other, they do not touch at any point. (Fig. 1.)

Fig. 1.



Mica Plate, wound with Two Copper Wires ready for Selenium Coating.

The next step is to apply the selenium, and to do this properly is an operation which requires a certain amount of practice and patience. The mica is heated to a temperature slightly above 217° C., and melted selenium is spread over its surface as evenly as possible with a metal spatula. The cell is then cooled, and its surface should be smooth and lustrous. Before you is an embryo cell which has reached this stage of its preparation. The selenium being still in the vitreous condition, is a perfect insulator, and when the cell is connected in circuit with a battery and a reflecting galvanometer, the spot of light is found to be absolutely motionless. I now propose to crystallise the selenium in your presence. The mere crystallisation occupies a very short time. It is only necessary to place the cell upon a brass plate, and raise it by means of a Bunsen burner to a temperature somewhat below the fusing point of crystalline selenium. The method described by Professor Adams in his classical paper published in the 'Philosophical Transactions,' is entirely different. He heated a bucket of sand by placing in it a red-hot iron ball. At the expiration of an hour he removed the ball, and placed in the heated sand his pieces of vitreous selenium, wrapped up in paper. After remaining for twenty-four hours, the selenium was generally found to have attained the crystalline form, and the resistance of some of his specimens thus prepared was far lower than that of any which have been made by myself. Their sensitiveness, however, does not appear to have been great. The method of crystallisation which I generally adopt, and which is due to Professor Graham Bell, has at all events the merits of simplicity and rapidity. In two or three minutes the whole surface of the selenium film becomes dull and slate-coloured, and if, when the

cell has attained this condition, it be removed from the hot metal plate, I have little doubt that on testing it with a galvanometer, it will be found to conduct electricity and to be sensitive to light. (Exp.) According to Professor Graham Bell, nothing more is necessary for obtaining the greatest degree of sensitiveness. The old-fashioned process of long heating and slow cooling may, he says, be altogether dispensed with. In this matter my experience differs entirely from his, for I find that cells which have been kept for some hours at a temperature just below the point of fusion, and then allowed to cool very gradually, are vastly more sensitive to light than those which have not been thus annealed.

The following table shows the resistances in the dark, and under different degrees of illumination, of a few cells taken from my stock. The resistance of No. 6, when exposed to a lime-light at 10 inches, is less than one-fiftieth of its resistance in the dark.

RESISTANCES IN OHMS.

Cell No.	In Dark.	Gas Jet at			
		12 Inches.	6 Inches.	3 Inches.	
4	400,000	190,000	150,000	80,000	Lime-light at 10 Inches,
6	290,000	80,000	54,000	29,000	5,700
7	430,000	160,000	110,000	64,000	
9	87,000	52,000	42,000	33,000	With Alum Cell,
10	62,000	32,000	26,000	17,000	14,000
11	100,000	63,000	50,000	33,000	24,000
12	22,700	9,100	6,600	4,500	..

It is an interesting question, which of the coloured components of white light has the greatest power in effecting these changes in the resistance of selenium; or, again, whether the effect is produced by light at all, or is due simply to heat. Captain Sale came to the conclusion, on moving a piece of selenium through the solar spectrum, that the maximum effect was produced at or just outside the extreme end of the red, at a point nearly coinciding with the maximum of the heat rays. Professor Adams performed the same experiment with the spectrum both of the sun and of the electric light, and found that the action on the selenium was greatest "in the greenish-yellow and in the red portions of the spectrum." The greenish-yellow is the point of maximum illumination, which is a remarkable fact; but his words seem to imply that there was a second maximum in the red. The violet and the ultra-violet rays, he says, produced very little, if any, effect. In consequence of the discrepancies in these results, I determined to repeat the experiment for myself. The source of light

which I used in the first instance was an oxy-hydrogen lime-light, and the spectrum was formed with a bisulphide of carbon prism. The experiment was repeated six times, and three different selenium cells were used. The results were precisely the same in every case, and proved in the most marked manner that the greatest influence occurred in the boundary-line between the red and the orange; thus differing completely from the results obtained both by Captain Sale and by Professor Adams. Moreover, the resistance when the selenium cells were placed in the ultra-red, two inches beyond the limits of the visible spectrum, was in every case lower than when it was in the blue, indigo, and violet But even in the ultra-violet the resistance of the cells was lower than when they were quite removed from the spectrum.

My friends Mr. Preece and Mr. W. H. Coffin, who were present during these experiments, suggested that it would be desirable to vary them by making use of different sources of light, and different methods of dispersion; and a few days afterwards, by the great kindness of Mr. Norman Lockyer, they were repeated in Mr. Lockyer's laboratory by Mr. Preece and myself with the electric light and a magnificent diffraction grating. Nine experiments were made with three cells, and the results were as absolutely concordant as those which we had previously obtained; but they all concurred in placing the maximum at the extreme edge of the red, thus agreeing with Captain Sale's observations. One other remarkable effect must be noticed. In the case of a single cell—that which I distinguish as No. 6—with which three experiments were made, a second maximum was observed in every case in the greenish yellow, though the effect was about 20 per cent. smaller than at the extreme red. The electric light, however, is from its great unsteadiness most unsuitable for experiments of this nature, and since no such exceptional phenomenon was ever observed before or since, I am inclined to believe that, by a coincidence which however remarkable is by no means impossible, the light happened to be unusually intense just on the three occasions when this particular cell was in the greenish yellow. A third series of experiments made with a gas flame and a bisulphide of carbon prism, agreed with the first in placing the maximum at the orange end of the red. Many more combinations of sources of light and dispersion remain to be tried, but time for these and for innumerable other experiments which have suggested themselves has hitherto been wanting: for an operation which may be described in a dozen words not unfrequently requires as many hours for its performance.

By the help of a reflecting galvanometer I now propose to show you the various effects produced by different parts of the spectrum of the electric light formed by a bisulphide prism upon the resistance of a selenium cell. The maximum deflection is seen to occur when the selenium is at the extreme outer edge of the red.

The effect of interposing various coloured glasses between a gas flame and the selenium cell was also tried. The greatest effect was

produced by orange glass, the smallest by green. It was, too, observed as a remarkable fact that the light transmitted by a dark-blue glass produced a greater effect than that which had been passed through a blue glass of much lighter tint. But on a spectroscopic examination the darker one was found to transmit a certain portion of red light. I also tried the effect of radiation from a black-hot poker held at a distance of about 6 inches from the selenium, and upon the first trial found that the resistance, instead of being diminished, was increased by several thousand ohms. I imagined this to be due to a rise of temperature in the selenium, and was thus led to experiment upon the effect of temperature. In this matter, too, there is a remarkable discrepancy between the authorities. Professor Adams says that an increase of temperature *increases* the resistance of selenium, and even suggests that a selenium bar should be used for the construction of a very delicate thermometer. Dr. Guthrie, Messrs. Draper and Moss, and others, make the directly opposite assertion that the resistance of selenium *diminishes* with heat. I repeated my poker experiment, which had in the former case apparently corroborated Professor Adams, and now to my utter astonishment I found that the resistance was greatly diminished. This second experiment, therefore, seemed to support Dr. Guthrie's statement. A great number of experiments were now undertaken for the purpose of arriving at the truth of the matter, with the details of which I will not weary you. Solutions of alum in water, of iodine in bisulphide of carbon, plates of glass and of ebonite were interposed between the selenium and the sources of light and heat. The selenium was now fried, now frozen; and the most contradictory results were obtained. At one moment I felt convinced that Professor Adams was right, at the next there appeared to be no shadow of doubt that Dr. Guthrie's was the true theory. In fact, it seemed as if the selenium was possessed by a demon which produced the variations in accordance with the caprices of its own unaccountable will. At length, when the confusion was at its height and the demon most bewildering, the true explanation was suddenly revealed, and so exceedingly simple is it that now the only marvel is that it should have so long eluded discovery. The secret of the matter is this: and it discloses one of the most remarkable properties of this most remarkable substance. There is a certain degree of temperature at which a piece of crystalline selenium has a maximum resistance. If a piece of selenium at this temperature is exposed to either heat or cold—it matters not which—its resistance will at once be diminished; and extremes of either produce a far greater variation than is ever effected by the action of light. A selenium cell which at the ordinary temperature measured in a dim light 110,000 ohms, was reduced by immersing it in oil at 115° C. to 18,000 ohms. The resistance of the same cell was reduced by immersing it in turpentine at $-6°$ C. to 49,000 ohms. In the case of the single cell with which I have hitherto made the experiment, the temperature on each side of which the resistance is diminished is

24° C.* Let this piece of selenium be gradually raised from a temperature of zero to a temperature of 100° C. While passing from zero to 24°, its resistance will rapidly increase. Passing from 24° to 100° its resistance will again rapidly diminish. (This experiment was successfully shown.)

Until Professor Bell directed his attention to selenium, all observations concerning the effect of light upon its conductivity had been made by means of the galvanometer. But it occurred to him that the marvellously sensitive telephone which he has invented might with advantage be used for the purpose, and on the 17th May, 1878, he announced in this theatre "the possibility," to use his own words, "of hearing a shadow by interrupting the action of light upon selenium." A few days afterwards Mr. Willoughby Smith informed the Society of Telegraph Engineers that he had carried this idea into effect, and had heard the action of a ray of light upon a piece of crystalline selenium.

When a selenium cell, a telephone, and a battery are connected in circuit, a uniform current of electricity will, under ordinary circumstances, flow through the telephone, and a person listening would hear nothing. Suppose now that a series of flashes of light were allowed to fall upon the selenium. In the intervals of darkness the selenium cell would offer a greater resistance to the passage of the electric current than during the intervals of light. The strength of the current would be constantly varying; and if the flashes succeeded one another quickly enough and with sufficient regularity, a musical note would now be heard by a person listening at the telephone. The exact pitch of this note would of course depend upon the rate at which the flashes succeeded one another, being high when the succession is rapid, low when it is slow. The nature of this sound is very peculiar, reminding one of the moaning of a syren or the rising and falling of the wind. With a sufficiently sensitive cell, powerful battery, and delicate telephone, the sound may be heard at a distance of many feet.

I shall interrupt the steady beam of light which is now falling upon the cell by causing a zinc disk with radial slits cut in it to rotate in the path of the beam, and the sound produced by the rapid succession of light and shade upon the selenium cell will be heard in the telephone. When the cell is screened from the light, the sound at once ceases. When the screen is removed, the sound is again heard as before. By using a system analogous to that of dots and dashes, an intermittent beam of light might be employed to convey photophonic messages to a distance.

But Professor Bell has gone further than this. He was not satisfied with merely *interrupting* a steady beam of light, producing alternately strong light and total darkness, but he aimed at graduating its

* The experiment has since been repeated with five other cells, and their temperatures of maximum resistance were found to be 23°, 14°, 30°, 25°, and 22° respectively.

intensity in correspondence with the varying phases of the complex sound-waves produced by the human voice. It is evident that if a beam so regulated were allowed to fall upon the selenium cell, the exact words spoken, with their articulation unimpaired, would be reproduced in the telephone. Professor Bell adopted a device which is equally marvellous for its extraordinary simplicity and for its perfect efficiency. The beam of light is made to fall upon the face of a small flexible mirror, whence it is reflected to the distant selenium cell, lenses being used for the purpose of rendering the rays parallel and condensing them where required. The speaker directs his voice upon the back of the mirror, which takes up the sound-waves and is thrown into a state of vibration, thus becoming alternately concave and convex. Now, when it is concave the light reflected by it is more concentrated, and the selenium cell more brightly illuminated. On the other hand, when it is convex, the opposite effect is produced: the rays are more dispersed and the illumination of the cell less intense. And since the movements of the mirror are in exact correspondence with the sound-waves of the voice, so also will be the intensity of the illumination of the selenium cell. The strength of the current passing through it will vary in the same proportion, and will cause the telephone plate to vibrate in consonance with the mirror, and thus to reproduce the exact sounds by which the mirror was set in motion.

In the small experimental photophone which is before you, the receiving station is within 20 feet of the transmitter, and any sounds heard in the telephone would of course be utterly drowned by the actual voice of the speaker at the mirror. It is necessary, therefore, to prolong the telephone wires, and carry them to a distant room, where the sounds that have travelled along the beam of light can be heard without interruption. Professor Bell, using instead of a lens a large reflector for receiving the beam of light, has heard words which were spoken when the mirror was 700 feet away from the selenium cell.

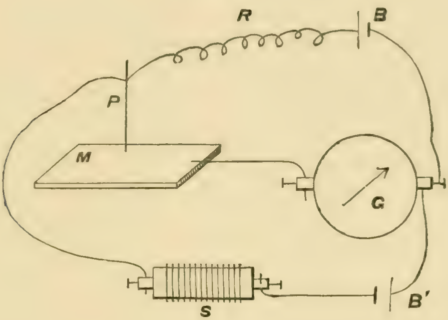
It is impossible to exhibit the photophone in action to an audience, because the effects can only be heard by a single person at a time. But I may mention, that in the course of some experiments with this little instrument which Professor Tyndall very kindly permitted me to make here on the 7th of December last, every word transmitted by it was perfectly understood.

I propose now to say a few words upon another and very different application of selenium. In point of interest and importance it cannot be compared with the photophone: but since it is a child of my own I naturally regard it with a certain amount of affection. It occurred to me a few months ago that the wonderful property of selenium, which we have been discussing this evening, might be applied in the construction of an instrument for transmitting pictures of natural objects to a distance along a telegraph wire. I have constructed a rough experimental apparatus in order to ascertain whether my ideas could be carried out in practice, and it is so far

successful, that although the pictures hitherto transmitted are of a very rudimentary character, I think there can be little doubt that further elaboration of the instrument would render it far more effective.

Iodide of potassium is very easily decomposed by a current of electricity. If a piece of paper which has been soaked in a solution of this substance be laid on a piece of metal M, Fig. 2, which is connected to the negative pole of a battery B, and a piece of platinum

FIG. 2.

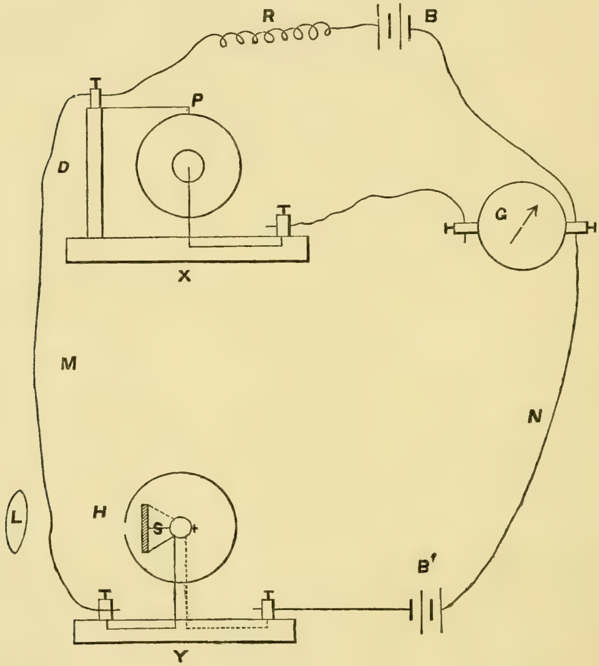


wire P, which is connected with the positive pole, be drawn over its surface, the path of the point will be marked by a brown line, due to the liberation of iodine. Let the platinum wire and the metal plate be connected to a second battery B′ in such a manner that a current of electricity may pass through the paper in the opposite direction; and let a variable resistance R be inserted between the platinum wire and the first battery B, and a selenium cell S between the platinum wire and the second battery. And let the resistance be so adjusted that when the selenium cell is exposed to a strong light, the two opposite currents through the paper and the galvanometer G neutralise each other; then the point when drawn over the paper will make no mark. But if the selenium cell is shaded, its resistance will be immediately increased, and the current from the first battery will predominate. The point, if moved over the paper, will now trace a strong line, which, if the selenium is again exposed, will be broken off or enfeebled according to the intensity of the light. (Exp.)

If a series of these brown lines were drawn parallel to one another and very close together, it is evident that by regulating their intensity and introducing gaps in the proper places any design or picture might be represented. This is the principle of Bakewell's copying telegraph, which will transmit writing or pictures drawn upon tinfoil with a non-conducting ink. My instrument differs from his in that the current is varied simply by the action of light. The transmitting instrument Y, Fig. 3, consists of a small cylindrical box 2 inches

deep, mounted upon a horizontal spindle, upon which is cut a screw having sixty-four threads to the inch. This works in two bearings 4 inches apart, one of which has an inside screw corresponding to that upon the spindle. At a point midway between the two ends of the cylinder a pin-hole H is drilled, and behind the hole a selenium

Fig. 3.



cell S is fixed. One terminal of the selenium cell is connected (through the spindle and stand of the instrument) with the negative pole of a battery B′, the other with the line wire M to the distant station. The receiving instrument X contains a similar brass cylinder, similarly mounted. A platinum point P presses gently upon its surface, and is connected both to the line wire and, through a variable resistance R, with the positive pole of a local battery B, the negative pole of which is connected through the galvanometer G with the cylinder. A wire or earth connection N, between the negative pole of the local battery and the positive pole of the other, completes the arrangement.

To prepare the instruments for work the cylinder of the transmitting instrument is brought to its middle position and a picture not more than 2 inches square is focussed upon its surface by means of

a photographic lens L. The hole H in the cylinder is then brought to the brightest point of the focussed picture, and a scrap of sensitised paper being placed under the platinum point of the receiver, the variable resistance is adjusted so that the two opposite currents through the paper neutralise each other. When this is accomplished the two cylinders are screwed back as far as they will go, the cylinder of the receiver is covered with sensitised paper, and all is ready to commence operations.

The two cylinders are caused to rotate slowly and synchronously. The little hole in the transmitting cylinder will in the course of its spiral path cover successively every point of the focussed picture, and the amount of light falling at any moment upon the selenium cell will be proportional to the illumination of that particular spot of the picture which, for the time being, is occupied by the pin-hole. During the greater part of each revolution the platinum point will trace a uniform brown line upon the prepared paper, but when the pin-hole happens to be passing over a bright part of the picture, this line is enfeebled or broken. The spiral traced by the point is so close as to produce, at a little distance, the appearance of a uniformly coloured surface, and the breaks in the continuity of the line constitute a picture which, if the instrument were perfect, would be a counterpart of that projected upon the transmitter.

The pictures upon which I have hitherto operated have been mostly simple designs, such as diamonds and squares cut out of thin metal, and projected by a magic lantern (see Fig. 4). But the instrument is in its earliest stage of infancy. It is at present hardly a

Fig. 4.

Image Focussed upon Transmitter.

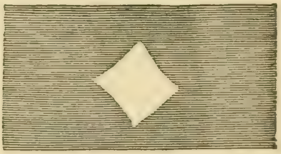


Image as Reproduced by Receiver.

single month old, and I regret to say that since its birth it has been shamefully neglected, circumstances having prevented me from giving it even the ordinary care and attention which all young creatures ought to receive. Nevertheless, I cannot but think that it is capable of indefinite development; and should there ever be a demand for telephotography, it may in time turn out to be a useful member of society.

[S. B.]

WEEKLY EVENING MEETING,

Friday, March 18, 1881.

William Bowman, Esq. LL.D. F.R.S. Vice-President, in the Chair.

William H. Stone, Esq. M.A. M.B. Oxon.

On Musical Pitch and its determination.

The Lecturer began by observing that the subject he had chosen, though at first sight technical, was one which should be taken up by the general public, not only on account of its scientific interest, but also since the special musicians were inclined to neglect it. Indeed, music itself had in this country, until quite lately, fallen into the hands of a limited class, and that not always highly educated or large in their views. It was as though England had characteristically handed over music-making to private contractors, as a monopoly, taking contentedly whatever was offered, and making no effort for larger and better supplies. Whereas music is really the most cosmopolitan of arts, springing up even where it might least be expected.

It was probably from this delegation to a few of what was the common property of all, that England had come to be regarded as an unmusical country, and that the remark made by a German on Sterndale Bennett, *Englischer componist, nicht componist*, had originated. The disesteem in which music had been held in this country was, doubtless, in part the inheritance of our Puritan ancestors, and in part the outflow of what might be termed "Chesterfieldism"; the tone adopted by would-be fine gentlemen, that it was undignified to be mixed up with "fiddles and fiddlers."

He affirmed, on the other hand, most strongly, that the nation possessed abundance of love for music; much talent also, which only needed fostering and cultivation; indeed, it might be severely but not untruly said, that all England was musical except the musicians. He admitted that this state of affairs had improved, and was improving. Music was no longer regarded only as a means of gaining a scanty livelihood, but as a branch of liberal education; the sense the word itself bore in the classical ages of Greece. It was the plain duty of such an audience as that he had the honour of addressing to assist in the revival.

Turning to the special subject of his discourse, he noticed that of the three fundamental elements of a musical note, pitch, intensity, and

quality, pitch was the most susceptible of accurate measurement, and that the recent great advances in physical science were mainly due to the substitution of quantitative for qualitative methods; of weighing and measuring for mere demonstration. He showed that absolute pitch did not exist in nature; a fact not negatived by the remarkable power exceptionally possessed by some ears of recognising a note by hearing. This so-called gift was really an acquirement, depending in some cases on the "muscular sense," as in the case of singers; or on a development of memory in others who, like organists, had sat for half a lifetime before a particular instrument, until its tones had penetrated into their inmost and instinctive consciousness. It was not dissimilar to the acquired habit of counting "*beats*," which was the foundation of piano- and organ-tuning, and which once established interfered seriously with the pleasure of listening to ordinary music. Examples of these *beats* and their causes were shown.

He proposed, after defining pitch as rapidity of vibration, to take three questions in succession: (1) the chief causes, and amount of variation in pitch in different sound producers; (2) scientific modes of measuring pitch; (3) the musical application of such methods, carried a stage farther in an artistic direction than was usual in treatises on acoustics.

It was shown experimentally that a metallic string through which a powerful current of electricity passes sinks more than an octave in pitch; that a tuning-fork heated over a lamp also sinks in pitch, though to a far less degree; that organ pipes vary greatly with heat, and also with watery or other vapour, rising rapidly with increased temperature. An instrument for measuring this phenomenon, made by the Lecturer, was shown. In it air from the same wind-chest was passed through two coils of metal pipe, one maintained at the temperature of melting ice, the other at that of boiling water. Rapid and distinct beating was thus produced in two pipes previously tuned to unison. Harmonium reeds moved in the same direction as tuning-forks, though in a greater degree; the former sinking about one vibration in 10,000 for each rise of a degree Fahrenheit, the latter about 1 in 16,000.

Both these quantities being small relatively to the changes undergone by other sources of sound, the tuning-fork furnished the best, and the free reed nearly as good a standard of pitch. The reed, however, depended somewhat on its material; a brass and steel reed on the same wind-chest, and in unison, beating distinctly when the air supply was raised to 212° Fahrenheit.

In orchestral wind-instruments a double action took place, the metal expanding with heat tending to flatten the note, whereas the hot and moist breath of the performer caused it to sharpen, the latter action greatly predominating in this climate at least.

(2) The scientific determination of pitch had been attacked by five principal methods. (1) mechanical, (2) optical, (3) photographic, (4) electrical, and (5) computative.

The following diagrammatic table was exhibited :—

I. Mechanical Methods.

1. Savart's toothed wheel.
2. Cagniard de Latour's siren.
3. Perronet Thompson's monochord.
4. Duhamel's vibroscope.
5. Leon Scott's phonautograph.
6. Edison's phonograph.

II. Optical Methods.

1. Lissajous' figures.
2. Helmholtz's vibration-microscope.
3. Kœnig's manometric flames.
4. McLeod and Clarke's cycloscope.

III. Photographic Methods.

1. Professor Blake's experiments.

IV. Electrical Methods.

1. Meyers' electrical tonometer.
2. Lord Rayleigh's pendulum.

V. Computative Methods.

1. Chladni's rod tonometer.
2. Scheibler's tuning-forks.
3. Appunn's tonometer with free reeds.
4. Kœnig's tuning-fork clock.

Under the first heading, an exact copy of Colonel Perronet Thompson's monochord, and the siren; under the second, Lissajous' figures, and McLeod's ingenious modification of these in the cycloscope were demonstrated, the latter having proved one of the most accurate and satisfactory instruments hitherto employed for this purpose. Considerable stress was laid on the fifth or computative method, on account of its extreme simplicity and accuracy, and also on the fact that by it, Absolute had first been obtained from Relative pitch.

The three instruments mainly adverted to were Scheibler's *Tonmesser*, Appunn's reed tonometer, and Kœnig's tuning-fork clock. The first and second of these were exhibited; of the third a photograph was projected on the screen. Scheibler was a silk manufacturer, of Crefeld, in Germany, who as early as 1834 published his system of pitch-measurement. In its simplest form, it consists of sixty-five tuning-forks, each beating with its two neighbours four times per second, the first and last producing together a true octave free from beats. It can easily be shown mathematically that if the product of 64×4 which $= 256$, and is the sum total of beats, be correct, it must equal the vibration-number of the deeper and half that of 512 the acuter fork. Thus absolute will have been deduced from relative vibrations, and the problem of pitch-determination will have been solved. Scheibler's excellent observations, however, seem to have failed to meet with the recognition they deserved, until they were disinterred by Helmholtz and his English translator, Mr. Alex. J. Ellis.

Appunn's reed tonometer proceeds on exactly the same principle as that of Scheibler, free reeds being substituted for forks. It is somewhat inferior in accuracy to the latter, for reasons named above, and also from the mutual influence of the reeds on one another, which has been shown to be considerable. On the other hand, its strident and coercive tone renders its indications more appreciable.

The third instrument, recently made by Kœnig, of Paris, and fully described in 'Wiedemann's Annalen' in 1880, has not yet reached this country. It consists essentially of Helmholtz's vibration-microscope, combined with a small clock of which the pendulum is a tuning-fork, causing the escapement to make 128 single vibrations per second.

It might be now considered that the problem of absolute pitch had been satisfactorily determined, and, a standard having been obtained, its artistic application was matter only of time and patience.

That it had not been so applied was a discredit to England, due chiefly to the rank and file of unmusical musicians named above. It was perfectly certain that since the time of Handel a rise of orchestral pitch amounting to about a semitone had occurred. The causes of this rise, in the Lecturer's opinion, were at least four: (1) the excess of true fifths, as tuned to by violins, over corresponding octaves; (2) the rise by heat of the increased number of modern wind-instruments; (3) the difficulty of appreciating slow beats, leading players, for the sake of prominence, to tune slightly above absolute unison; (4) the predominant effect on the ear of a sharper over a flatter note, causing a steady rise of the instruments which are susceptible of tuning.

It was obvious to any thoughtful man that the Voice, God's instrument, should be consulted in preference to man's less perfect contrivances of wood and brass. At the same time, the difference between the high orchestral pitch now in use to the detriment of singers' voices, and the French normal diapason, which had been proved by Kœnig to be an accurate as well as convenient standard, was really far less than would be thought. This fact was illustrated by playing alternately on clarinets tuned to the one pitch and the other; the ear, unassisted by beats, being all but unable to detect the difference between the two. In conclusion, the main need of modern English music was stated to be a greater familiarity with the physical principles upon which it rests. [W. H. S.]

WEEKLY EVENING MEETING,

Friday, March 25, 1881.

Warren De La Rue, Esq. M.A. D.C.L. F.R.S. &c. Secretary and Vice-President, in the Chair.

Alexander Buchan, Esq. M.A. F.R.S.E. Sec. Met. Soc. Scot.

The Weather and Health of London.

(Abstract deferred.)

WEEKLY EVENING MEETING,

Friday, April 1, 1881.

Joseph Brown, Esq. Q.C. Vice-President, in the Chair.

Sir Henry S. Maine, K.C.S.I. F.R.S. &c.

The King in his Relation to Early Civil Justice.

Wherever, in the records of very ancient societies belonging to races with which we have some affinity, we come upon the personage whom we call the king, he is almost always associated with the administration of justice. He is much more than a judge. He is all but invariably a military chief, and constantly a priest. But he rarely fails to be a judge, though his relation to justice is not exactly that with which we are familiar.

The law books claiming the highest antiquity are those of the Hindoos, of which one, and not the oldest, has long been known vaguely to Europeans as the code of Manu. These books only became law books by a process of specialisation, having at first dealt with all things human and divine; but they always assume a king to administer justice, who sits with learned Brahmans for assessors. This order of ideas may be traced in the westerly wing of the Aryan race, where the great Brehons who declared the ancient Irish law are kings or kings' sons, and where it is expressly laid down that a king, though of right a judge, may have an assessor to advise him. Still older is the conception of the king's relation to justice found in the poems attributed to Homer. There the king, as judge, pronounces judgments or "dooms," but though they are doubtless based on preexisting usage, they are supposed to be divinely and directly dictated to the king from on high.

The judges of the Hebrews represent an old form of kingship, but independently of the etymology of the name, they are clearly exponents of the law and administrators of justice. Deborah, who is counted among them, judged Israel in Mount Ephraim; Eli, the last but one of them, had judged Israel forty years; and Samuel, the last, expressly claims credit in his old age for the purity of his judgments. The decline of the system is marked by the misconduct of their children. Under the later hereditary kingship, the judicial function scarcely appears in Saul and David, but revives in Solomon.

By the side of the king there was another fountain of law and justice, the popular assembly. It is not necessary to enter on the

question, now much disputed, which was the older of the two, but it is to be observed that, however much a system of tribunals independent of the king might be organised, there was always supposed to be a residuary and complementary jurisdiction in the king. The Roman law, which supplies the law of the civilised world wherever English law does not prevail, is descended from this residuary jurisdiction. What we know as the Roman jurisprudence is not the primitive Roman law, but it is that law distilled through the jurisdiction of the Roman prætor, which jurisdiction had descended to him from the ancient half-fabulous kings of Rome.

In the ancient Teutonic administration of justice, which is specially interesting to us as a Teutonic people, we find the king and the popular tribunal side by side. The relations of the two are very difficult to trace in our own island, much as modern learning has done for the inquiry, but they are tolerably clear in the law of the Salian Franks, which has descended to us as the Salic law, and which is erroneously supposed to have something to do with the descent of crowns. The Salic law is really a manual of law and procedure for the ancient German Court of the Hundred. The king first appears merely as claiming a share of the fines; but as the history of law proceeds, it is his authority which gives to the administration of justice most of the characteristics which now belong to it. The ancient Court of the Hundred had no power to enforce a large class of its own decisions, the man who disobeyed them being at most outlawed. But if the litigants agree beforehand that the king's representative shall enforce the award of the court, he will do it; and so will the king himself if the litigant goes to him in person. As the Frankish kings become more powerful, they intervene more and more in the business of the Hundred Court. The Court, or King's Deputy, takes the place of the elective President, or Thingman; but then, on the other hand, all the judgments of the court are enforced. Finally, even popular justice comes to be administered in the king's name.

Except in communities living within walled towns, whose institutions followed a peculiar course of development, royal justice steadily grew at the expense of popular justice. What were the causes of this? First of all, the multitudinousness of the popular courts, and the great burden which the duty of attending them threw on the free cultivator. In England, the Reeve and four men attended the Hundred and Shire Courts, and an even larger number of freemen attended in the courts of the Continent. Even now a summons to serve on a jury is not received with complacency, but what must the duty of going to the Shire Court have been when most of England was forest or fen, and when there were few roads but the old Roman roads? Nor was the onerousness of the duty to be discharged in court very slight, since the judges had sometimes to fight on behalf of their own verdicts. There are councils of the Church which protest against the burden thrown on poor men. The feudal courts

descended from the popular courts were equally numerous at first and equally oppressive in consequence.

Meantime the justice which the king administered to all who applied to him was purer, more efficient, and more skilfully adapted to the facts, since he alone had the command of expert advice. But still, in order to understand the accessibility of royal justice, we must bring home to ourselves what the ancient Teutonic king was. He did not live at home in a distant castle or palace. He was, above all things, an ambulatory, itinerant personage, moving ever about his territory with surprising rapidity. The ancient Celtic king followed the same practice. The ancient Irish records show the Irish king perambulating the territory of his subordinate chiefs, making them presents, and feasting at their expense. By the end of the sixteenth century this had become a great abuse, and the "cutting and coshering" of the Irish chiefs is especially stigmatised as one of the curses of Ireland. The itinerancy of the English kings continued to a surprisingly late period, and was much more constant than is popularly known. One object, no doubt, was to live on the produce of their widely separated lands, but another was to administer justice and collect judicial fines and fees.

The Lecturer then referred to the Itineraries of King Henry II. and King John drawn up by Mr. Eyton and Sir T. Duffus Hardy. He gave as an example the movements of King John in May 1207, and showed that the king, in the course of that month, travelled over half of England. And though John passes as an effeminate sovereign, the same extraordinary activity went on through every month of nearly every year of his reign. Gradually, however, the itinerant king became a monarch of the modern type, the early stages of the change being traceable through the growth of the system of *missi*, of itinerant deputies or "justices in eyre," which was considerably older in England than King John's reign, but was much enlarged by its great event.

The rapid movements of the early Teutonic king probably left him time enough at each point for the settlement of primitive litigation. But as the law and men's affairs became more complicated, a new set of abuses arose. The litigant who desired the royal judgment had to hurry after the king over all parts of his dominions. The Lecturer referred to the efforts of Richard de Anersley to get Henry II. to "give him a day": the story of his trouble and expenses is printed in the second volume of Palgrave's 'Rise of the English Commonwealth.' It is easy for the reader of this paper to understand the importance of the provision of Magna Charta that "the Common Pleas are no longer to follow the king."

The struggle between royal and popular justice has determined the judicial and legal history of many different European countries. The judicial system of England is of royal origin. Except so far as it has been changed by the modern county courts, it is the most centralised system of judicial administration in the world. The

popular courts have practically perished. On the other hand, the law itself has been less changed than in France or Germany. It is still a modernised version of Teutonic usage.

In France, these characteristics are reversed, mainly owing to the authority obtained by the Roman law. The civil code is little more than a version of Roman jurisprudence. But the same cause which changed the law preserved the form of the judicial system, and hence superficially the French judicial system has much of the form of the old popular judicature. You find very little judicial centralisation, a large number of local courts, a multitude of judges.

The residuary authority of the king produced in England the Court of Chancery, which became a recognised portion of our system. It also produced the Star Chamber, whose jurisdiction became a proverb of oppression. The Star Chamber marks the exhaustion of what was once the most valuable of all sources of justice. The reforming authority of the king has descended to legislatures, now almost everywhere the children of the British Parliament.

[H. S. M.]

GENERAL MONTHLY MEETING,

Monday, April 4, 1881.

George Busk, Esq. F.R.S. Treasurer and Vice-President, in the Chair.

Louis Frank Cohen, Esq.
Alfred Baring Garrod, M.D. F.R.S.
Forster Graham, Esq.
Mrs. Charlotte Lassetter,
Hugh Leonard, Esq. M.I.C.E.
Mrs. Llewellyn W. Longstaff,
Mrs. Elizabeth Russel Müller,
Eugène de la Penha, Esq.
Mrs. Eugène de la Penha,
St. George Lane Fox Pitt, Esq.
Percy Spalding, Esq.
John Lawrence Sullivan, M.D. M.R.C.P. Lond.
George Wray, Esq.

were elected Members of the Royal Institution.

The Arrangements for the Lectures and Friday Evening Meetings after Easter were announced, viz.:—

Professor Dewar, M.A. F.R.S.—Six Lectures on The Non-Metallic Elements; on Tuesdays, April 26 to May 31.

Professor Tyndall, D.C.L. F.R.S.—Six Lectures on Paramagnetism and Diamagnetism; on Thursdays, April 28 to June 2.

Professor H. Morley.—Three Lectures on Scotland's Part in English Literature; on Saturdays, April 30, May 7, 14; and One Lecture on Thomas Carlyle; on Tuesday, June 7.

E. C. Turner, Esq. Lector at the University of St. Petersburg.—Five Lectures on The Great Modern Writers of Russia; on Saturdays, May 21, 28, June 4, Thursday, June 9, and Saturday, June 11.

The Special Thanks of the Members were given to the Committee of the Cobden Club for the Present of some of their Publications.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same.

FROM

The Governor-General of India:—
Geological Survey of India.
Records. Vol. XIII. Part 4. Vol. XIV. Part 1. 8vo. 1880–1.
Accademia dei Lincei, Reale, Roma—Atti, Serie Terza: Transunti: Tome V. Fasc. 6, 7, 8. 4to. 1880–1.
Actuaries, Institute of—Journal, No. 121. 8vo. 1880–1.
Catalogue of Library. 8vo. 1880.
List of Members. 8vo. 1880.
Andrews, John R. Esq. M.R.I. (the Author)—George Whitefield: a Light Rising in Obscurity. Fourth edition. 12mo. 1879.
Asiatic Society of Bengal—Journal, Vol. XLVIII. Part I. Extra No. 8vo. 1880. Vol. XLIX. Part I. No. 4. Part II. Nos. 3, 4. 8vo. 1880–1.
Proceedings, 1880, Nos. 7, 8, 9, 10. 1881, No. 1. 8vo.
Astronomical Society, Royal—Monthly Notices, Vol. XLI. No. 4. 8vo. 1881.
Bavarian Academy of Sciences, Royal—Sitzungsberichte: 1881, Heft 1. 8vo.
British Architects, Royal Institute of—Proceedings, 1880–1. Nos. 12–15. 4to.
Chemical Society—Journal for March, 1881. 8vo.
Cobden Club Committee:—
R. Cobden—Political Writings. Ed. Sir L. Mallet. 12mo. 1878.
H. Ashworth—Recollections of Richard Cobden. 16to. 1876.
Correspondence Respecting the Budget. Ed. J. W. Probyn. 16to. 1877.
The Duke of Argyll—Essay on the Commercial Principles applicable to Contracts for the Hire of Land. 16to. 1877.
H. Fawcett—Free Trade and Protection. Third Edition. 8vo. 1879.
Sir L. Mallet—Reciprocity. 8vo. 1879.
W. E. Baxter—Our Land Laws of the Past. 12mo. 1880.
A. Mongredien—The Western Farmer of America. 8vo. 1880.
History of the Free Trade Movement in England. 16to. 1881.
Sir T. Wedderburn—British Colonial Policy. 8vo. 1881.
G. C. Brodrick—English Land and English Landlords. 8vo. 1881.
Financial Reform Almanack. 8vo. 1881.
List of Members of the Cobden Club. 16to. 1880.
Domville, William Henry, Esq. M.R.I.—Reports of Hungarian Natural History Museum. 3 Parts. 8vo. Budapest. 1880.
Editors—American Journal of Science for March, 1881. 8vo.
Analyst for March, 1881. 8vo.
Athenæum for March, 1881. 4to.
Chemical News for March, 1881. 4to.
Engineer for March, 1881. fol.
Horological Journal for March, 1881. 8vo.
Iron for March, 1881. 4to.
Nature for March, 1881. 4to.
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Telegraphic Journal for March, 1881. 8vo.
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Geneva: Société de Physique et d'Histoire Naturelle—Mémoires. Tome XXVII. Partie 2. 4to. 1880.
Geographical Society, Royal—Proceedings, New Series. Vol. III. No. 3. 8vo. 1880–1.
Geological Institute, Imperial, Vienna—Verhandlungen, 1880, Nos. 1-18. 8vo.
Jahrbuch: Band XXX. No. 4. 8vo. 1880.
Abhandlungen: Band XII. Heft 2. fol. 1880.
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Haarlem, Société Hollandaise des Sciences—Archives Néerlandaises. Tome XV. Liv. 3, 4, 5. 8vo. 1880.
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National Association for the Promotion of Social Science—Transactions at the Edinburgh Meeting, 1880. 8vo. 1881.
Pharmaceutical Society of Great Britain—Journal, March, 1881. 8vo.
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Plateau, M. J. F.R.S. Hon. M.R.I. (the Author)—Bibliographie des Principaux Phénomènes subjectifs de la Vision. 2e Supplément. 4to. 1880.
Preussische Akademie der Wissenschaften—Monatsberichte: Nov. 1880. 8vo.
Royal Society of London—Proceedings, Nos. 209, 210. 8vo. 1881.
Russell, The Hon. Rollo, F.M.S. M.R.I. (the Author)—London Fogs. (K 104) 8vo. 1880.
Smithsonian Institution, Washington, U.S.—James Smithson and his Bequest. By W. J. Rhees. 8vo. 1880.
Stone, Dr. Wm. H. (the Author)—Elementary Lessons on Sound. 16to. 1879.
Symons, G. J.—Monthly Meteorological Magazine, March, 1881. 8vo.
Tidy, C. Meymott, Esq. M.B. M.R.I. (the Author)—River Water. (No. 2) A Reply to Dr. Frankland. (K 104) 8vo. 1881.
Verein zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, 1881: No. 3. 4to.
Wells, T. Spencer, Esq. M.R.I. (the Author)—Cremation or Burial. (K 104) 8vo. 1880.
Yorkshire Archæological and Topographical Association—Journal, Parts 23 and 24. 8vo. 1881.

WEEKLY EVENING MEETING,

Friday, April 8, 1881.

George Busk, Esq. F.R.S. Treasurer and Vice-President, in the Chair.

Professor Tyndall, D.C.L. F.R.S. *M.R.I.*

The Conversion of Radiant Heat into Sound.

(Abstract Deferred.)

WEEKLY EVENING MEETING,

Friday, April 29, 1881.

Warren De La Rue, Esq. D.C.L. F.R.S. Cor. Mem. Inst. France, &c. Secretary and Vice-President, in the Chair.

Professor J. Stuart Blackie, F.R.S.E.

The Language and Literature of the Scottish Highlands.

Some fifty years ago Colonel Vane Kennedy, in a book of no vulgar speculation and research, could make the assertion before the scholars of Great Britain that the Celtic languages constitute a special family, having no connection with any other known languages, specially altogether distinct from Sanscrit, Latin, Greek, Teutonic, and other members of the great Aryan class. At the present day there is not a fairly instructed schoolboy in an ordinary English classical school who is not familiar with the exact contrary of this proposition. That such an assertion should have been made at all, admits of explanation only from the general neglect of the Celtic languages by well-educated British scholars, together with the crude state of arbitrary divination, in the limbo of which even good philologers were in those days blindly tossed about. Against this system of would-be scientific conjecture as applied to the Celtic languages, Colonel Kennedy stoutly and wisely protested; but his own knowledge of Celtic, picked up mainly from the dictionary, without any living knowledge either of its habits or its anatomy, was altogether insufficient to enable him to make a diagnosis of the language, that might furnish reliable materials for a scientifically conducted induction. Such a diagnosis, thanks to the labours of those "intellectual moles" and intellectual eagles, the Germans, we are now in a condition, with the most perfect ease and with the most sure-footed safety, to conduct. My own acquaintance with the Celtic languages is confined to that member of the family spoken in the Highlands of Scotland, commonly called Gaelic; and as it was an acquaintance which I made accidentally from sympathy with the people among whom for a succession of summer seasons I had pitched my tent, and followed out as a pleasant recreation rather than a serious business, I cannot pretend, in addressing you, to speak with the full weight of authority that would belong to the words of a Zeuss, an Ebel, or a Windisch. But I know enough of the general principles of comparative philology, and enough also both of the grammar and the living genius of the language as now spoken in the Highlands, to keep me from falling into any serious blunder; and I appear here before you to-night, I presume, on the very practical and profitable assumption that in a domain where everybody knows nothing,

a man who knows something may pass for a pundit. I shall therefore proceed to tell you what I know of the matter, on John Locke's famous supposition that your metropolitan minds are, in reference to the subject of my lecture, as a sheet of blank paper, on which an unkempt uncovenanted Scot may for once be allowed to stamp any scripture he pleases.

Colonel Kennedy was perfectly aware that there existed not a few words in Welsh and Irish manifestly cognate with the same words in Latin; but he had a ready theory that all savage or semi-civilised tribes borrow largely and greedily from their civilised superiors, and he thought that this theory was sufficient to explain all the similarities which he had noted. Now, it is quite true, however some stiff Galicians may kick against it, that not only ecclesiastical words, but other words not a few, may be either certainly set down as borrowed from Latin, or labouring under a strong suspicion of such importation. But it is equally true that words for the most common objects and necessary relations of life, and where no suspicion of borrowing can intrude, appear in Gaelic with a distinctly Latin physiognomy; and it is truly surprising to me how the bad luck could have happened to any ransacker of dictionaries, to march out two long columns of Celtic roots of familiar objects without stumbling upon a single Latin or Teutonic equivalent. If the Celts borrowed *fion* from the Latin *vinum*, which is possible enough, though anything but certain, it certainly cannot be said that the words MATHAIR, *mother*, BRATHAIR, *brother*, EACH, *horse*, and CU, *dog*, fall under the same foreign category. And what shall we say to the numerals? It should have seemed to Colonel Kennedy that it was as irrational to suppose that the Celts borrowed the names of the simple numerals from the Romans, as with the scholars of last century to believe that Sanscrit is a language borrowed from Greek as a consequence of the conquests of Alexander the Great. The lowest savages count by fives and tens and scores; and the Celts in Julius Cæsar's time were confessedly far above that level. Let us commence therefore with the numerals as at once the most striking proof of the original identity of the language, and as presenting examples of some of the most characteristic mutations of consonants, which regulate the passage of an original Indo-European root from the Latin to the Celtic form.

GAELIC.	LATIN.
aon	*unus.*
da	*duo.*
treas	*tres.*
ceithir	*quatuor.*
coig	*quinque.*
se	*sex.*
seachd	*septem.*
ochd	*octo.*
naoidh	*novem.*
deich	*decem.*
ficheat	*viginti.*
ceud	*centum.*

Now the three first of these numerals require no observation. In the fourth we see an illustration of a law very common in Gaelic, as compared with Latin, and as one would expect also in French—viz. dropping a consonant in the middle of a word, when preceded and followed by a vowel. Thus the French from *pater* make *père*, and from *mater*, *mère;* and so the Latin *quatuor* is smoothed down to *ceithir* (pronounced *Cā-ur*), by the omission of the aspirated *t*. In *coig* another law is exemplified, which leads to the omission of the nasal *n* before a consonant, exactly as in Ionic Greek we have πύθοιντο vocalised into πυθοίατο. So in Gaelic we have *mios*, a *month*, for *mensis*. The number *sex* is softened down by the common practice of shaving off a final consonant. So in *septem*, *novem*, and *decem*, the final *m* falls, as we know neither was it pronounced by the Romans, and as the modern Greeks treat the final ν of the second declension of nouns, saying καλό for καλόν. In *seachd* and *ochd* we further see the preference given by the Celts to the aspirated guttural *ch*, while as an initial of roots *c* remains as in *cridhe* καρδία and *creadh creta;* and in *deach* compared with *decem* we have further to note that the hard *c* or *k* in Latin at the end of a word is softened into *ch*, as in *each* for *equus; naoidh* vocalises the medial *v* of the Latin. *Ficheat* exemplifies the change of *v* into *f*, as in *vinum*, and in *fios* for the German *wissen;* and again, the throwing out of the *n* before the final *t*, as when the Greeks changed the original Doric λέγοντι into λέγουσι. *Centum* becomes *ceud* on the same principle.

And now, summing up all these special differences between the Gaelic language and its nearest relative,* we may say at once that the Gaelic language bears on its face the impress of a curtailed, smoothed over, and somewhat emasculated Latin—a language which has dealt consistently with the original stock of Latin which it brought with it from the East, exactly in the same fashion that French has dealt with its imported Latin. This curtailment in both languages, French and Gaelic, has gone to such an extreme that it is not seldom difficult for an inexperienced eye to recognise the identity. Thus between *gour*, a goat (I write here as pronounced), and *caper*, *gawl*, and *capere*, *aar* and *pater*, on a superficial view there seems no connection ; but spell these words as they appear in the books, *gabhar*, *gabhail*, *athair*, and a philological eye discerns at a glance the original identity of the divergent terms. For the spelling of these words clearly indicates that the medial consonant before being dropped was aspirated, that is, softened down by a breathing which renders it more easy of pronunciation, and prepares the way for its final disappearance. Restore this medial consonant, with all the sharpness of its natural features, and there is not the slightest difficulty, even to an

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* Ebel says that the Gaelic roots which can be proved to be modified forms of the same roots in the Aryan family belong in pretty nearly equal groups to the Latin and Teutonic stock. I deal only with the Latin here, as being the more familiar to the general audience.

unscientific eye, in perceiving that *gabar* and *caper*, *gabail* and *capere* are identical, the change of the sharp into the blunt consonant in both cases, and the rejection of the final vowel, with the familiar change of *r* into *l* in *capere*, being all that is required to effect the passage from the Latin to the Celtic form of the word. In *athair* a further change takes place, the dropping of the initial consonant; but this is quite in order, as the Homeric forms αἶα for γαῖα, εἴβω for *libo*, and αἰνὸς for δεινὸς sufficiently prove. The Gaels seem to have had a peculiar antipathy to *p* at the commencement of a word; so that not only in *athair* from *pater*, but in *leac* from πλακ-, in *leana* from *planus* and in *lan* from *plenus*, and in *uchdt* from *pectus*, this unoffending letter has been rudely thrown out. The system of aspiration here noted as a preparatory step for the evasion of the medial consonant, and taking the BONES, so to speak, out of the word, extends in Gaelic and all the Celtic languages far beyond the case of the medial consonant. It is a regular habit of the language to modify by aspirates the initial consonant of any word, when it is preceded by certain words, most of which are distinguished by a long final vowel, a modification which in not a few cases amounts to a total deletion of the consonant, and in certain cases to a sweeping erasure of both consonant and aspirate from the field of hearing; a result which not only emasculates the word, but renders it difficult to be recognised by those whose ear has been trained to the primary and unmodified form. Thus the word TIGH, a house (in which as spelt the Latin *tego*, the Greek στέγος, the German *dach*, and the English *deck* are plainly recognised), when preceded by *mo* or *do*, *my* or *thy*, forthwith becomes *hīgh*. A similar modification takes place regularly in the flexion of nouns and verbs, and specially when an adjective is joined to a feminine noun. Thus, as *Ben*, a mountain, is feminine in Gaelic, instead of *Ben More*, or big mount, the natives say *Benvōre*, or, as they spell it, *Benmhor*, changing the *m* into *v* by the addition of the aspiration. I remember how much I was puzzled with the signification of *Ben Awt* (the name, as pronounced, of the north peak of Ben More in Mull), till I consulted a lady living at the bottom of the hill, who told me that *Awt* as pronounced was only a modified form of FAD, *long*, the modification being caused by the feminine gender of the noun, which necessitated the aspiration of the initial *f*; and this, again, necessitated the disappearance of both aspirate and consonant! The effect of all this, while it unquestionably gives a certain indistinctness and want of firmness to the expression of the language, is to make it admirably fitted for musical purposes; as we see also in Scotch, where *hall* becomes *ha; at all* becomes *ava; gold*, *gowd; will not*, *winna; do not*, *dinna; must not*, *mauna*, and so forth. This state of the case contrasts wonderfully with the common opinion entertained of Gaelic by the English people, who are accustomed to talk of it as harsh and guttural; but this opinion arises partly from the fact that tourists in the Highlands seldom hear the language spoken except by the most unrefined persons, and partly from the notion that

the final *ch*, in which Gaelic, like German, abounds, is a harsh sound. It is quite the reverse. The German *milch* is the soft form of the harsh and sharp English *milk*. It is nothing singular that men attempt to fasten a fault on an object perceived, when the real flaw lies in the defective organ of the percipient.

So much for the language. The literature in its main stream consists of popular ballads and songs—those *κλέα ἀνδρῶν* with which Achilles is represented as solacing his solitary grudge when Agamemnon sends the embassy to request him to rejoin the Greek army. Of these songs and ballads a collection was made by a certain Dean Macgrigor, of Lismore, in Argyll, about the time of the Reformation; for a long time preserved in the Advocates' Library in Edinburgh, and some years ago published and translated under the able editorship of Skene and MacLauchlan. Another most extensive and valuable collection has recently been made by John Campbell of Islay, taken down from the mouths of the people and preserving many of the old Fenian traditions in a form which, without his work, must very soon have disappeared. I myself have heard some of these ballads recited by an old man in Tobermory, the descendant no doubt of a race of ballad-singers and story-tellers, who formed a regular profession in the Highlands, but which now, like other good things in that quarter, is rapidly dying out. As in ancient Greece, the original musical form in which the popular traditions were embodied soon gave rise to a prose version of cognate matter in a kindred tone; so beside the ballads and songs of which we have spoken, there existed in the Highlands a rich collection of prose stories or tales, which were told by accomplished story-tellers to lighten the heaviness of the winter evenings at the smoky fireside. To the patriotic diligence of Mr. Campbell in this case also we are indebted for the preservation of a body of prose Highland tales of primary importance in the history of early Aryan and European civilisation. The contents of these stories, though often fanciful and childish, like our fairy tales, are seldom without a subtle moral significance; and their style is masterly, with a certain natural quaintness and grace, for which we shall find no parallel except in some of the most attractive pages of Herodotus. Some of these ample ballad materials, about the middle of the last century, as all the world knows, fell into the hands of a literary gentleman named MacPherson, belonging to the district of Badenoch, between Braemar and Kingussie; and manipulated by his hands and a few friends well skilled in Celtic lore, they were sent forth to the world under the name of the poems of Ossian. That these famous poems—whose originality was recognised with fervour by Goethe, Herder, and others of the most notable names in European literature—are a genuine Celtic production, both in respect of the materials from which they were composed, and the manipulators who put the materials together, there can be no doubt. The only doubt is how much or how little these gentlemen did to put the materials which they unquestionably possessed into

their published shape; and this is a doubt which, like many points connected with the Homeric poems of early Greece, must, I fear, remain for ever unremoved. The Greek Homer, that is, the great poet who usually passes for the author of the Iliad, and the Celtic Homer, that is, not Ossian, but MacPherson, equally founded their fame on the working up of the floating materials of popular ballads into a more elevated form; as they both equally, no doubt, left imprinted on the materials which they used the stamp of their own peculiar genius; only with this difference, that Homer lived in an age when the minstrel world to which he belonged was still in its vigour, while MacPherson appeared late in a literary age in the character rather of an antiquarian refurbisher than of an active contemporary bard. The consequence is, that between Homer and the times of which he sings, the most complete and pleasant harmony everywhere is felt; whereas MacPherson's work can never altogether be cleared from the suspicion of having quitted the healthy simplicity of the old traditions to indulge in the superfine sentiment and a certain tragic attitudinising, characteristic of the somewhat flat and feeble century to which he belonged.

Though the Highlanders were never a reading people, and are not even now so to any great extent, we must not suppose that they were in any sense a savage or a degraded or an uncultured race. Not in the least. Man liveth not by books alone, but by every word that floweth out of the living soul of a brother. Professional bards always existed amongst them, learned in all the traditions of their clan, and with senses well exercised to discern all the beauty and sublimity of the picturesque country which they inhabited. Of the intellectual fertility of this race a notion may be had from the study of the *Sar obair* or book of the classical Highland poets, a collection made by a certain John MacKenzie, of Gairloch, in Ross-shire, to whose memory a monument recently erected strikes the eye of the traveller as he proceeds from the old village to the New Inn outside the loch.

It would be impossible for me, in the bird's-eye view I am here presenting, to enumerate even the names of those who have merited an honourable place in this Pantheon of the Celtic bards; for not only within the book but outside of it, everywhere, even at the present hour, the intellectual atmosphere of the Highlands is intensely lyrical, and common people express their best thoughts in song as naturally as the moist banks shoot forth primroses in April.* But I may single out three as having more than common claims to the notice of the general British public; I mean ALASTAIR MACDONALD, of Ardnamurchan, DUGALD BUCHANAN, of Loch Rannoch, Perthshire, and

* The fertility of the living Celtic Muse will be best understood by the perusal of the *Oranaiche* and other lyrical collections published by Mr. Sinclair, Argyle Street, Glasgow, or to be had from MacLachlan and Stewart, publishers, opposite the College, Edinburgh.

Duncan MacIntyre, of Inveroran in Argyleshire, all belonging to the middle or the latter half of the last century. MacDonald, unlike his brethren of the Celtic lyre, had received a university education, and had more of the character of a modern literary man than of a genuine Highland minstrel. Possessed of a bold Byronic genius, he was the author of several poems of undeniable power, and a man altogether who, under more favourable circumstances, might have ripened into a great British poetic notability. He lived in the country of the Clan Ranald, and his *launch* of the *Biorlinn*, or *Barge of Clan Ranald*, is unquestionably one of the most spirited and powerful poems in the Gaelic language.

Dugald Buchanan, the Bunyan of the religious world in the Highlands, had a genuine poetic vein, as his poem on Hamlet's suggestive theme—a human skull—places beyond doubt; but that classical production, and his other poems, are marred to heterodox readers, by their want of sympathy with the peculiar theology of terrors and tortures with which the natural gay temperament of the Highland Celts, since the Evangelical revival of last century, in its most narrow and repulsive form, has been largely infected.

MacIntyre, or Duncan Ban, fair Duncan, as he is more familiarly called, like a genuine old Celtic bard, knew nothing of reading or writing, but spun his musical musings into shape as he wandered up and down the glens in the vicinity of *Tyndrum* and *Loch Tulloch*. His poems breathe the finest appreciation of Nature and the most genuine human kindness; health and joy and beauty are the atmosphere which he constantly carries about with him; he borrows his colour from the purple heather, and his music from the mountain brook; while the stag on the brae is his familiar friend, and the most distinctive living figure in his landscape. As a picture of mountain scenery, and a glorification of the characteristic Highland sport of deer-stalking, MacIntyre's "Ben Doran" is a work as unique and perfect in the region of poetical art as Landseer's pictures are in the sister art of painting. Of this poem it may be interesting to present a specimen from a translation made by me some years ago in Oban.*

"Right pleasant was the view
Of that fleet red-mantled crew,
As with sounding hoof they trod
O'er the green and turfy sod
        Up the brae,
As they sped with lithsome hurry
Through the rock-engirded corrie,
With no lack of food, I ween,
When they cropped the banquet green
        All the way.
O grandly did they gather,
In a jocund troop together,

* Published in 'Language and Literature of the Scottish Highlands.' Edinburgh: Edmonston and Douglas, 1876.

In the corrie of the Fern
With light-hearted unconcern;
Or by the smooth green loan
Of Achalader were shown,
Or by the ruined station
Of the old heroic nation
        Of the Fin,
Or by the willow rock
Or the witch-tree on the knock,
The branchy crested flock
        Might be seen.
Nor will they stint the measure
Of their frolic and their pleasure
        And their play,
When with airy-footed amble
At their freakish will they ramble
        O'er the brae,
With their prancing and their dancing,
And their ramping and their stamping,
And their splashing and their washing
        In the pools,
Like lovers newly wedded,
Light-hearted, giddy-headed
        Little fools.
No thirst have they beside
The mill-brook's flowing tide
And the pure well's lucid pride
        Honey-sweet;
A spring of lively cheer,
Sparkling cool and clear,
And filtered through the sand
        At their feet;
'Tis a life-restoring flood
To repair the wasted blood
The cheapest and the best in all the land;
And vainly gold will try
For the Queen's own lips to buy
        Such a treat.
From the rim it trickles down
Of the mountain's granite crown
        Clear and cool;
Keen and eager though it go
Through your veins with lively flow,
Yet it knoweth not to reign
In the chambers of the brain
        With misrule;
Where dark water-cresses grow
You will trace its quiet flow,
With mossy border yellow,
So mild, and soft, and mellow,
        In its pouring.
With no slimy dregs to trouble
The brightness of its bubble
As it threads its silver way
From the granite shoulders grey
        Of Ben Dorain.
Then down the sloping side
It will slip with glassy slide
        Gently welling.

Till it gather strength to leap,
With a light and foamy sweep,
To the corrie broad and deep
    Proudly swelling;
Then bends amid the boulders,
'Neath the shadow of the shoulders
    Of the Ben,
Through a country rough and shaggy,
So jaggy and so knaggy,
Full of hummocks and of hunches,
Full of stumps and tufts and bunches,
Full of bushes and of rushes,
    In the glen,
Through rich green solitudes,
And wildly hanging woods
With blossom and with bell,
In rich redundant swell,
    And the pride
Of the mountain daisy there,
And the forest everywhere,
With the dress and with the air
    Of a bride."

As a whole, Gaelic literature is a literature which is likely to die, as it has lived, without going largely into what we call more distinctively literature. The genuine Highlander still sings. He does not write. An admirable, and to a certain extent successful, attempt at creating a prose literature was made by Dr. Norman Macleod, father of his better-known son, the Queen's favourite clergyman, in the early part of the present century. He published a magazine full of graphic sketches of Highland life and character, set forth with a grace and seasoned with a humour, enough to give a classical position to any writer. But admirable as these tracts were, and forming, as they do at the present hour, the unequalled model of classical Gaelic prose, the reading element in Highland society was too weak to encourage any further adventure in this style. It is in vain to write for a people who either do not read at all, or are led by irresistible seduction to seek for what books can give in the full-flowing streams of English, rather than in the thin rivulets of Gaelic prose. Next to sketches of character, given in the lively style of popular dialogue, the staple of Macleod, one would expect from the Highlander, being as he is notably a very serious and religious person, a large display of sermon or pulpit literature; but here expectation finds itself hugely disappointed. The fervour of Celtic apostleship is well known; and the very numerous adherence of the Presbyterians north of the Grampians, to the Free Church, whatever other value it may have, is certainly a remarkable proof of the efficiency and the popularity of the clergy in those parts; but however fervid in pulpit demonstration, and zealous in points of traditional orthodoxy, the trans-Grampian Evangelists may be, they have wisely confined their ministrations to the electric effect of the living word, and not endeavoured to gain a position for Gaelic in the printed

eloquence of the pulpit which few could appreciate and everybody could spare. Among contemporary attempts to use Gaelic for the currency of the hour, the Gaelic articles in that sturdy organ of Radicalism the *Inverness Highlander*, are deserving of special praise; but the very small proportion of the columns of that journal in which the native language appears, affords the most satisfactory proof that the great mass of Highland readers prefer the English tongue, and are in fact for the most part unable to read the works of their best poets, by whose names they are yet proud to swear. The only other production of Gaelic prose that seems to call for special mention is their body of wise saws and popular apophthegms, originally collected by an Episcopal clergyman of the name of Macintosh, who lived in the early part of the present century, and now republished with large additions and valuable comments by that genial and accomplished Celt, Sheriff Nicolson, of Kirkcudbright.

Should I be expected to say, in conclusion, what is the present state and future prospects of the Celtic population in the Highlands, the answer may be short, but sad. Personally I am one of those who like to see Highlanders in the Highlands; but where Nature, and unnatural landlords, and partial land laws, and a one-eyed political economy divorced from all moral considerations and social ties, have now for more than a century conspired to drain away the native population of the glens, my wishes are a mere breath that will pass the weighted scales innocuously, and leave the balance where it was. Our noble Highlanders, the best-conditioned peasantry morally and physically in Europe, and the best constituent of our once famous armies, that knew no defeat, have been lost to us, I fear, for ever, by land laws which, while they strengthened by artificial enactments the natural strength of the lords of the soil, left the mass of the people at the mercy of pleasure-hunting lords—not seldom absentees—and omnipotent factors inflated by economical crotchets or spurred by commercial greed. Laws were made and maintained with jealous severity to preserve the game; but no one dreamt of preserving the people. The consequence has been that the people, receiving no encouragement from their natural protectors, who rather seemed anxious in not a few cases to get rid of people, poachers, and poor laws at a stroke, retreated year after year from their dear old homes, which were homes now only for gamekeepers and game, and Titanic dealers in Highland wool and hill-mutton, and sought for higher wages, more kindly treatment, and far less healthy moral and physical surroundings in the hot-beds and back slums of our great manufacturing towns. In these circumstances it is in vain to expect that the Gaelic language and the Gaelic literature should be at present in a very vigorous condition. It is no doubt wonderful to observe what flashes of the genuine old spirit occasionally shoot forth in fervid verse, and in sagacious prose; but they are only FLASHES. Genuine Celtic sentiment, and loving appreciation of Celtic culture, appear only in a few exceptional individuals; the best part of the people have left the country in despair; and those who remain behind,

feeble, dejected, and dispirited, slaves to the urgent necessities of the hour, are more anxious to catch greedily at any bait which the purse-proud Saxon may fling before them than to retain the honourable heritage of manhood and self-reliance which they received from their sires. With the great mass of Highlanders, I fear, patriotic sentiment does not go much beyond a sentiment; men in their depressed condition, in fact, cannot afford to feed on the savour of old traditions, however ennobling; they stand face to face with the hard facts of a world that knows nothing about Duncan Ban, and to whom the spirit-stirring strains of the national pipe can be looked on only as an ill-timed interruption to the whirling of their gigantic wheels, and the whirring of their multitudinous power-looms. A special blow of discouragement has recently been given to the maintenance of a genuine Celtic spirit in the Highlands by the recent Education Act. In the code of the Metropolitan Board, neither Gaelic poetry, nor Gaelic music, nor anything with a distinctively Highland hue and Celtic flavour, makes its appearance. The Socratic principle of educating by drawing out what is in people, rather than by injecting them with what is foreign, seems utterly unknown to those who in London are entrusted with the important function of teaching the young mind how to shoot in the world benorth of the Grampians. But red tape and centralization, however, naturally narrow and unsympathetic, are not in this case altogether to blame. It is the indifference of the people themselves that lies at the root of this neglect of the best popular culture for a Celtic people in a Celtic country, and the wholesale adoption of what is strange and artificial. Much of the best soul and the stoutest brawn of the country has, we have already said, been driven by partial laws, and commercial selfishness, and inconsiderate pleasure-hunting, into a voluntary expatriation; while the few that remain, often the feeblest and most spiritless, must be content to look up to their Saxon masters to feed them and to clothe them, rather than to their Celtic ancestors to inspire them; and, so far as this is the case, there is small hope for them. Where the Celtic soul, by an unfortunate conspiracy of external circumstances and selfish agencies, has been pumped out of them, it cannot be the business of the School Boards to pump it in again. Where sparks of the grand old fire still remain, their only resource seems to be that they should form voluntary districtual associations for the preservation of patriotic culture and sentiment and music, after the example of what has recently been done in Rogart, Sutherland, by that most intelligent and manly Celt, John Mackay, Swansea. No small people, under the daily influence of strong currents of denationalising electricity from a people on a higher social platform, can hope to rescue its individuality without a manly determination to do so. Here SELF-HELP is the only help; and UNION under courageous leaders the only form that efficient help can assume.

[J. S. B.]

ANNUAL MEETING,

Monday, May 2, 1881.

William Bowman, Esq. LL.D. F.R.S. Vice-President, in the Chair.

The Annual Report of the Committee of Visitors for the year 1880, testifying to the continued prosperity and efficient management of the Institution, was read and adopted. The Real and Funded Property now amounts to above 85,400*l.*, entirely derived from the Contributions and Donations of the Members.

Forty-nine new Members paid their Admission Fees in 1879.

Sixty-two Lectures and Nineteen Friday Evening Discourses were delivered in 1880.

The Books and Pamphlets presented in 1879 amounted to about 166 volumes, making, with 555 volumes (including Periodicals bound) purchased by the Managers, a total of 721 volumes added to the Library in the year.

Thanks were voted to the President, Treasurer, and Secretary, to the Committees of Managers and Visitors, and to the Professors, for their valuable services to the Institution during the past year.

The following Gentlemen were unanimously elected as Officers for the ensuing year:

President—The Duke of Northumberland, D.C.L. LL.D.
Treasurer—George Busk, Esq. F.R.C.S. F.R.S.
Secretary—Warren De La Rue, Esq. M.A. D.C.L. F.R.S. Cor. Mem. Inst. France, &c.

Managers.

George Berkley, Esq. M.I.C.E.
William Bowman, Esq. LL.D. F.R.S. F.R.C.S.
Thomas Boycott, M.D. F.L.S.
Frederick Joseph Bramwell, Esq. F.R.S.
Edward Frankland, Esq. D.C.L. F.R.S.
Cæsar Henry Hawkins, Esq. F.R.S. F.R.C.S.
Sir Joseph D. Hooker, K.C.S.I. C.B. D.C.L. F.R.S.
William Huggins, Esq. D.C.L. F.R.S.
William Watkiss Lloyd, Esq.
Sir John Lubbock, Bart. M.P. D.C.L. F.R.S.
Sir Frederick Pollock, Bart. M.A.
Henry Pollock, Esq.
The Lord Arthur Russell, M.P.
C. William Siemens, Esq. D.C.L. F.R.S.
William Spottiswoode, Esq. M.A. D.C.L. Pres. R.S.

Visitors.

The Lord Brabazon.
George B. Buckton, Esq. F.R.S. F.L.S.
Stephen Busk, Esq.
Henry Herbert Stephen Croft, Esq. M.A.
Thomas Andros De La Rue, Esq. B.A.
James N. Douglass, Esq.
Alexander John Ellis, Esq. B.A. F.S.A. F.R.S.
Right Hon. The Lord Claud Hamilton.
Robert James Mann, M.D. F.R.C.S.
William Henry Michael, Esq. Q.C.
Hugo W. Müller, Esq. Ph.D. F.R.S.
Sir Thomas Pycroft, M.A. K.C.S.I.
Lachlan Mackintosh Rate, Esq.
The Hon. Rollo Russell, F.M.S.
Edward Woods, Esq. M.I.C.E.

WEEKLY EVENING MEETING,

Friday, May 6, 1881.

William Watkiss Lloyd, Esq. Manager, in the Chair.

The Hon. George C. Brodrick, M.A. B.C.L.

WARDEN OF MERTON COLLEGE, OXFORD.

The Land Systems of England and of Ireland.

I have undertaken to address you to-night on the land-systems of England and of Ireland, that is, on the distinctive and typical features which characterise them among the land systems of the world. Such a study is especially interesting at the present moment, when radical changes in the Irish land system are actually under the consideration of Parliament, and the English land-system itself may be said to be on its trial. But the rules of this Institution do not permit me to discuss English or Irish land questions in the political or controversial sense. We are mainly concerned to-night with the past and present aspects of the English and Irish land systems; the future development of those land systems rests with the Legislature, and the members of this Institution have little reason to envy their responsibility.

I. The land systems of England and of Ireland have a common historical origin. Modern researches have shown that in both countries the earliest form of agrarian constitution was a tribal settlement, or village community, representing a clan or group of kindred families. It is needless here to dwell upon the peculiar and minute rules which governed the division and cultivation of land in this primitive society, which are still preserved in the so-called "Brehon Laws" of Ireland. What is important to note is that it left no room for that threefold division of burdens and profits between landlords, tenant-farmers, and farm-labourers, which is the special mark of the English rural economy. Every freeman was, in theory, his own landlord, his own farmer, and his own labourer, and, except serfs or slaves, there were very few persons who did not form members of the landed democracy, as it might be properly called. But the landowners of that day were not peasant proprietors, for though each was entitled to a lot of his own, he could not be sure of holding the same piece of ground two years together; and there were few, if any, separate enclosures for cattle. By slow degrees, however, the principle of individual ownership asserted itself. The chief, or strongest member, of a clan would obtain larger allotments than others, and at last get them severed from the common fields; at the same time he would claim the lion's share of the waste, and at last came to treat it as his own property, only subject to rights of

pasturage and turf-cutting. Meanwhile, other causes were at work to undermine the landed democracy, and transform it into a landed aristocracy, under which the village community became the manor, the greater freeholders became tenants, and the lesser freeholders sank into the class of villeins or mere labourers. We must not stop to investigate the steps by which this remarkable transition was effected. Suffice it to say, that it seems to have been completely effected in most parts of England before the Norman Conquest, and had been partially, if not completely, effected in Ireland, when it passed under the rule of Henry II. a century later.

During the Middle Ages, the land systems of both countries were profoundly modified by the introduction of feudal tenures. Not that feudal tenures, with all their well-known incidents, were substituted all at once for the old national customs by a single act of the sovereign power. Even in England more than a century elapsed before feudalism was fully established, and even then it was subject to important exceptions in Kent and elsewhere. As for Ireland, the greater part of the island remained outside the dominion of English law until the reign of Henry VIII. For some little time after the Conquest, an attempt was made to extend the new institution of judicial assizes over the whole country, and Magna Charta was proclaimed there as promptly as if Ireland had already formed part of an United Kingdom. But, in fact, both English law and English authority were confined within the boundaries of a few counties, thence called the English Pale. These counties at last dwindled down to four, and even here the old Irish customs of land tenure, as well as the old Irish manners, had encroached more and more upon English customs and land tenures. The King of England was not king, but only "Lord," of Ireland; but one English army (under Richard II.) crossed the Irish Channel in the course of three or four centuries; and we know, from the works of Edmund Spenser and Sir John Davies, that all the strange anomalies of tribal ownership survived in vast tracts of Ireland up to the end of Elizabeth's reign, and the beginning of James I.'s reign.

Still, the feudal system is the real basis of the English and Irish land laws, as they exist at this moment. I must assume that my audience is sufficiently acquainted with the broad outlines of that system, which ceased to govern the whole structure of society after the Reformation, but which continued to regulate the land tenures of most European countries until after the French Revolution. In England, it is true, it was otherwise. "Feudal tenures," in the strict legal sense, were abolished here in the reign of Charles II., but, perhaps for that very reason, the principles and rules of feudal law escaped revision here, when they were swept away elsewhere, and have left an indelible stamp on the distinctive features of the Anglo-Irish land-system.

II. These features are five in number:—(1) The law and custom of Primogeniture, governing the descent and ownership of land.

(2) The peculiar nature of family settlements, which convert the nominal owner of land into a tenant for life, with very limited powers over the estate. (3) The consequent distribution of landed property among a comparatively small and constantly decreasing number of families. (4) The direction of cultivation by a class of tenant-farmers, usually holding from year to year without the security of a lease. And (5) the dependent condition of the agricultural labourers, who are mostly hired by the day or the week, and have seldom any interest in the soil. It is the combination of these features which makes the rural economy of England so entirely unique, unlike that of any other European country, and still more unlike that of the United States or our own colonies. They are often represented as the spontaneous growth of our national character and history, coupled with the peculiarities of our soil and climate. I think I shall be able to show that such is not the fact—that, in reality, they are mainly the result of artificial causes, and that it is quite within the province and the power of law to remodel—of course gradually—the land systems of England and of Ireland.

1. Let us first glance at the institution of Primogeniture. The right of the eldest son to inherit all the land, in case of intestacy, was not recognised by Roman law, or by any of the primitive codes known to us, such as those of the ancient Hindoos, the ancient Germans, the Irish, or the Anglo-Saxon. The Saxon rule of descent, as is well known, was that of gavelkind, or equal division; nor was it superseded by the Norman rule of Primogeniture until about the year 1200. It has often been observed that under a charter of Henry I., which seems to have continued in force only five years, the eldest son did not succeed to all his father's land, but only to his "principal fee," or the chief of several estates. A very similar rule still prevails in the Channel Islands, which are virtually a fragment of that Normandy from which England was conquered. This was, in fact, the old Norman law, and it was only for military reasons that William the Conqueror and his successors adopted the strict and absolute law of Primogeniture which has now been firmly established in England for nearly seven centuries. After a careful study of the subject, I am convinced that it is this law of Primogeniture which has produced and kept alive the custom, and that it is not the custom which has perpetuated the law. Before the law was introduced in England, there is no reason to believe that any general custom of Primogeniture existed in English families. After the law was swept away in America, an equal partition of land became the almost universal custom, although American testators enjoy almost the same liberty of making wills that is allowed in England. Moreover, in the case of personal property, where the law is different in England, the custom is also different, and hardly any one thinks of accumulating all his personalty on one son. Nor must we suppose that because the law seldom operates directly, it has not a very wide and powerful operation indirectly. When a man makes a will, or settlement, he

knows very well, or if he does not his solicitor tells him, that all his land would naturally go by law to his eldest son, and this knowledge, transmitted from one generation to another for seven hundred years, creates a sentiment or prejudice in favour of Primogeniture which nothing but a reversal of the law will effectually counteract. No doubt there is much to be said for, as well as against, Primogeniture; but for our present purpose the important fact is that Primogeniture, founded on law and consecrated by custom, is the chief corner-stone of the English land system.

2. But the custom of Primogeniture is far more stringent than the law. When land descends to an eldest son, on intestacy, it belongs to him absolutely, and he is free to deal with it as he pleases. On the other hand, when it comes to him under a will or a settlement, it usually comes to him for life only, and must afterwards go to his eldest son, whether he pleases or not. This is the consequence of certain legal refinements devised in the seventeenth century, whereby it is possible for a grandfather to ordain beforehand that his eldest grandson, as yet unborn, and who may turn out the most worthless or the most exemplary of mankind, shall inherit a particular estate, making his son only a life tenant or "limited owner." Under the older entails of the Middle Ages this was impossible, and though similar powers of tying up land were acquired by the landed aristocracy in the fourteenth and fifteenth centuries, means were found to defeat them, so that in the sixteenth and first half of the seventeenth centuries the ownership of land was far more free than it now is. At present, the great mass of land in this country is under settlement, and land under settlement is land which has not, and perhaps never may have, a real owner. The apparent owner of a great family estate is nothing but a trustee, and though of late something has been done to give him more liberty of action, he is hampered at every turn by the necessity of obtaining consents from a number of different parties, or perhaps from the Court of Chancery. Suppose all these consents to be obtained, he may doubtless improve or even sell the property; but what motive has he to do so, when he cannot reap the fruit of the improvements or become master of the purchase-money? Indeed, the evils of limited ownership are so obvious, especially from an economical point of view, that no one would venture to defend it, but that it is supposed to keep old family properties from being broken up. But then the question arises whether this is altogether an advantage. The character of the English gentry and aristocracy was formed before limited ownership was known, and when estates descended from father to son either in fee simple, or under the old rule of entail, which allowed of their being instantly converted into fee simple estates. In those days, family properties were placed under the guardianship, not of conveyancers, but of the families themselves, and the nation was content that if they came into the possession of degenerate heirs they should be sold and purchased by worthier competitors. Even now such cases occur,

where a family property is ruined by one or two spendthrift limited owners in succession. Experience amply shows that, in such cases, it generally changes hands for the better, notwithstanding the loss of ancestral connection. The new purchaser may be comparatively ignorant of country life, but he is not encumbered by rent-charges of indefinite duration, by mortgages contracted to pay off his father's debts, by dynastic traditions of estate-management, by the silly family pride which must needs emulate the state of some richer predecessor, by the passion for political dictation to which the refusal of leases is so frequently due, or by the supposed necessity of satisfying the supposed expectations of the neighbourhood. He can provide for his widow and younger children by selling off portions of the property, if he pleases, instead of charging the estate, and in the meantime he can develope the resources of the property, without feeling that he is either compromising or unjustly enriching an eldest son. These advantages make themselves felt even when the new purchaser is surrounded with great settled estates and influenced by the example of their possessors. But they might be expected to make themselves far more conspicuously felt if all landowners enjoyed the same freedom of disposition.

3. The inevitable tendency of a land system thus founded on Primogeniture, and guarded by family settlements, is to prevent the dispersion of land, and to promote its concentration in a few hands. Settled estates seldom come into the market, and, when they do, the money has generally to be reinvested in land; but there is nothing to prevent a rich life-tenant from increasing the size of his property, and this is constantly happening. A very large number of farm-houses in England are really ancient manor houses, formerly the residence of squires and yeomen, whose little freeholds have been gradually absorbed into the princely territories of the landed aristocracy, and whose descendants are settled in the neighbouring towns. Of course, we must not forget the opposite movement, or counter-migration of retired tradespeople into the country; but they seldom take root there; they do not look upon their villas as homes, they count for nothing in a county, and their children are usually re-absorbed into the town population.

Upon the whole, it may be stated with certainty that the number of agricultural landowners in England was never so small, as the population was never so large, as it now is. It would appear from Domesday Book that in the reign of William the Conqueror the soil of England was divided among about 170,000 landowners, including more than 100,000 villeins, as well as above 50,000 freeholders. There is no direct mode of estimating the number of landowners between that age and our own, but there is a vast body of indirect evidence pointing to the conclusion that in the reign of Elizabeth, for instance, petty squires, yeomen, and small freeholders occupied a much larger space in the community than they do at present. Even since the compilation of the 'New Domesday Book,' in 1876, there is

great difficulty in ascertaining the exact actual number of English landowners; but, after devoting much attention to the subject, with the able assistance of Mr. John Bateman, I have arrived at an approximate result. I believe that, excluding the holders of less than one acre, there are now about 150,000 landowners in England and Wales, while about 2250 persons own together nearly half the enclosed land in England and Wales. Considering that England and Wales now contain a population of more than 20,000,000, and did not contain above 2,000,000 in the reign of William the Conqueror, the proportion of landowners to population is now less than one-tenth of what it then was, and, what is still more striking, nearly half of all the land belongs to a mere fraction—about $1\frac{1}{2}$ per cent.—of all the existing landowners, even excluding those below one acre.

It would be superfluous to point out the political danger involved in this distribution of landed property, which contrasts most strongly with that which exists in foreign countries. For instance, in France, before the loss of Alsace and Lorraine, there were about 5,000,000 proprietors owning about $7\frac{1}{2}$ acres each, on the average; about 500,000 proprietors owning 75 acres each, on the average; and about 50,000 proprietors owning 750 acres each, on the average. In Wurtemberg, there are some 280,000 peasant owners, with less than five acres each, and about 160,000 proprietors of estates above five acres. No doubt, this extreme subdivision is, to a great extent, the result of the Code Napoléon, under which at the death of a proprietor all his land is divided equally among his children, except one child's portion, which is left at his own disposal. On the other hand, the extreme aggregation of land in England is no less the result, and the foreseen result, of Primogeniture and settlement. It is not merely that, under the law of Primogeniture, a great estate which may have been formed out of many small estates goes to one child, instead of being subdivided among several; nor is it only that settlements prevent family estates from being diminished, while they do not prevent them being increased. It is also that Primogeniture and family settlements have created a landed aristocracy under the cold shadow of which a true yeomanry, like the old English, cannot flourish. It is too much to say that the old yeomen have been *crushed out* by powerful neighbours. Many have sold their patrimonies because they were in debt, or because they found that by getting a fancy price from some great nobleman or millionaire they could improve their incomes and the expectations of their families. But it is still more delusive to regard the disappearance of the old English yeomanry as the result of natural causes beyond the control of law. When it is said that land in this country has now become the luxury of the rich, and that a poor man would be very foolish to retain a few hundred acres when he could make a profit by selling them, it is forgotten that in Northern France, Belgium, Holland, and elsewhere, land fetches a higher price than in England, but that small proprietors do *not* die out; on the contrary, that they are the highest

bidders in the land-market. We must, therefore, look beyond the fancy price of land for the explanation of the fact that in England the body of landowners is getting smaller and smaller. The explanation is not far to seek. The vast preponderance of great landowners has left the yeoman class no place in county government or county society. As one yeoman vanishes after another, those who survive, feeling themselves more and more isolated, and missing the neighbourly fellowship of past generations, are drawn insensibly into country towns, until at last the rural population of English counties may be said to consist of three elements, and three only, landlords, tenant-farmers, and labourers.

4. This leads us to consider the fourth distinctive feature of the English land system—the direction of cultivation by a class of tenant-farmers usually holding from year to year, without the security of a lease. For the great bulk of the land in these islands, as is well known, is cultivated, not by the owners, but by this intermediate class, numbering between 500,000 and 600,000 farmers in Great Britain, who hold on the average 56 acres each. It is not thus in other countries, especially in the most civilised. There, on the contrary, the great bulk of the land is cultivated by the owners themselves, most of whom may be classed with our agricultural labourers rather than with our tenant-farmers, but form a real peasantry of a class well nigh extinct in England. For it was not always thus in England itself. Lord Macaulay believes the small freeholders, whom he estimates at 160,000, to have greatly outnumbered the tenant-farmers in the reign of Charles II., and there is good reason to believe that English farms were commonly held under lease until the period of the French war at the end of the last century. The history of yearly tenancy is difficult to trace, but it is certain that it was very much encouraged by the long continuance of "war prices" which made landlords very unwilling to part with the immediate control of their properties, and by their desire to maintain political influence over their tenants. The late agricultural depression has operated in the same direction, inclining landlords to keep farms at their disposal until rents improve, and inclining tenants to rely on the forbearance of landlords under yearly tenancy, rather than "hang a lease round their necks," as they say. On the other hand, the want of security incident to a mere yearly tenancy, and especially the want of security for a farmer's improvements, have been very much felt and discussed of late. Unhappily, it has not led to a revival of leases, but to attempts to bolster up the unstable system of yearly tenancy. One of these attempts was embodied in the Agricultural Holdings Act of 1875, to extend which is the object of two Bills introduced this year. Such measures may be described as tending to establish a national system of tenant-right, and this would certainly be a great advance on mere yearly tenancy, but it would be a very poor substitute for leases, and no substitute at all for ownership.

5. We now come to the fifth distinctive feature of the English land

system—the dependent condition of the agricultural labourer. During the Middle Ages, English labourers, whether freemen or serfs, had always been essentially *peasants*, that is, occupiers of land which they cultivated in spare hours for their own benefit, and from which they could not be displaced, so long as they rendered certain customary services or paid their rent. With the growth of the commercial spirit, the suppression of monasteries, the general rise of prices, and the progress of enclosure, a new era set in, and the poor-law of Elizabeth finally transformed the old English peasant into the modern English agricultural labourer, who lives on weekly wages, never owns land, and seldom holds any beyond a small garden or allotment, looking upon the workhouse as his natural refuge in old age. Probably he is better housed and clothed than his mediæval ancestor, though it is doubtful whether he is better fed, if we take into account the exorbitant price of meat in these days. But he is certainly less independent, and, notwithstanding the spread of education, he must still be ranked below a great part of the continental peasantry—not to speak of American farmers—in the scale of civilisation.

III. Let us now consider how far these distinctive features of the English land system apply to Ireland.

1, 2. Of course, the law of succession to land and the practice of family settlements are the same in both countries, though Primogeniture was not established in the Celtic parts of Ireland until after the great confiscations of the sixteenth and seventeenth centuries. Even now, it is not so deeply rooted in Irish as in English popular sentiment. The yeomen and small proprietors who still survive in some English counties generally "make eldest sons," but Irish tenant-farmers, who have long been wont to deal with their farms as if they were their own, often leave them by will to their widows, and usually make a liberal provision out of them for younger sons and daughters.

3. But, however this may be, the landowning class, under the operation of Primogeniture and entail, has become even smaller in Ireland than in England—smaller, not only absolutely, but relatively. Speaking broadly, we may say that all Ireland is divided among about 20,000 proprietors, and that by far the greater part is owned by about 10,000 proprietors, of whom most are Protestant and of English descent, while many of the largest are absentees. This contrast between 20,000 or 10,000 owners and more than half a million occupiers, must never be forgotten in a survey of Irish rural economy. It is of course partly the result of conquest and confiscation—great tracts of land having been allotted to any soldier or adventurer willing to settle in the country. It partly arises also from the want of trade and manufactures in Ireland, which reduces the number of people able and willing to purchase land, for the purpose of improving their social position. But there can be no doubt that it mainly arises from the operation of Primogeniture and entail, keeping the ownership of Irish land in the hands of men unconnected with Ireland, many of whom, if free trade in land had been established, would have

sold their estates long ago to the occupying tenants. This has actually been done under the Irish Church Act, and to a less extent under the Irish Land Act of 1870, which have added about 5000 to the number of small Irish proprietors. Probably more occupiers would have availed themselves of the facilities granted by these Acts, if they had not been taught by agitators that, by waiting a little while, they would get the land for nothing.

4. But the greatest distinction between the Irish and English land system is in the relation between landlord and tenant,—partly in the laws which regulate it, but mainly in the customs and ideas which influence it. Let us briefly notice some of the circumstances which have brought about this great difference in customs and ideas.

The greater part of Ireland never adopted feudal institutions as a whole, and, where they were adopted, the feudal lord was not the friend and protector of his tenant, as in England, but was constantly regarded as an alien intruder. Again, though the feudal law of landlord and tenant was ultimately established in Ireland, it was more favourable to the landlord and less favourable to the tenant, than in England. Nevertheless, for two centuries and a half after the English poor-law was established, there was no poor-law in Ireland, so that small tenants naturally held on to the land for bare life, having no other means of subsistence.

Meanwhile the respect for property, that is, for the landlord's rights, as distinct from the tenant's, was very much weakened by the differences of religion, and by the demoralising effect of the penal laws. It was further weakened by the fact that so many Irish landlords entirely neglected their duties, and left all improvements, including the erection of farm-buildings, to be executed by their tenants, while they contented themselves with receiving their rents. Of course, the case was aggravated where the landlord, as often happened, was an absentee. No liberality on the part of an agent can supply the want of that kindly intercourse between the hall and the cottage, which binds classes together in an English village, but of which Irish farmers and labourers have little experience. No wonder that Irish tenants should thus grow up in the belief that the soil was theirs, and the rent only the landlord's.

We cannot do justice to the agrarian movement in Ireland, or appreciate the deeper causes to which it owes its origin, without placing ourselves in the position of a representative Irish tenant before the Act of 1870, and striving to interpret the feelings which underlay his fierce hatred of landlordism, a hatred which even the remedial legislation of that year has failed to appease. We shall afterwards be far better able to appreciate the still more sweeping reforms which are now in contemplation.

The representative Irish tenant is not a capitalist farmer at all, in the English sense, but rather a cottager holding some fifteen or twenty acres of land, including several acres of rough pasturage for the cows, of which the poorest Irish family generally manages to keep

one or two, with very humble pretension to breed, yet frequently yielding a large supply of milk. He was born upon the land which he cultivates, if not in the cabin which he inhabits. Sometimes the little farm lies compactly round its steading; more often it is scattered about in irregular patches, or stretches in a long narrow strip from a hillside down towards a stream or marshy bottom. It is tilled by the farmer himself, with the aid of his son or nephews, and occasionally of an obliging neighbour, but in most cases, without recourse to hired labour. Perhaps his ancestors, in far-off times, were entered on the sept-roll as possessors of this very plot, which has been tenanted ever since by his family, though repeated confiscations may have effaced the memory of its superior lords before the last century, and its last purchaser may have acquired it under a sale in the Encumbered Estates Court. Perhaps it was painfully won from the adjoining waste by his father or himself, either in the capacity of a mere squatter, or under a verbal arrangement with the agent that no rent should be exacted for a certain number of years. However this may be, and whether its present occupant inherited it or reclaimed it by his own industry, all that has made it a *home* for him was created by himself or his kindred, nor is it possible for him to regard it as the sole property of a stranger. Every piece of stonework upon it, from the rude homestead to the meanest shed or byre, was erected by himself or his forefathers, every fence or enclosure was made by them, every field cleared and roughly drained by them, nor is there any visible sign of proprietorship other than his own, unless it be the occasional presence of an agent who is chiefly known to him as a collector of rent. His rent is not high, it is true, being little above the Government valuation, and far less than some insolvent and reckless neighbour would undertake to pay if the farm were put up for competition. His landlord, too, is a kind-hearted man, in his way, never raising a tenant's rent twice in one lifetime, and willing to make abatements in hard seasons, but seldom resident, and cut off from his sympathy by the iron barriers of race and religion. The genial influence of a good English squire, who devotes himself to county business, takes an interest in the parish school, directs his own improvements, and visits his labourers' cottages, is something of which he cannot even conceive. No one ever threatened him with eviction, or informed him directly that in such a case he must not look for compensation. The idea of eviction and its consequences, however, is always present to his mind. He remembers that, after the great famine, scores of little cabins disappeared from the mountain-side opposite, and that nothing was ever heard again of their former inmates. It has been reported to him that in the next county vast grazing-farms have been formed out of holdings like his own, and that the experiment has been financially successful. He read only the other day a paragraph in the newspapers advertising for sale just such an estate as his landlord's, and describing it as greatly under-rented and suitable for pasture. He is aware, indeed,

that here and there a good landlord compelled to part with a portion of his property, has granted leases beforehand to old tenants, and thereby protected their equitable rights against the purchaser. But he believes such magnanimity to be very rare, and dares not count upon it himself, especially as he is told that since tenant-right has come to mean downright confiscation, proprietors must get their estates so far as possible into their own hands. He lives, therefore, from hand to mouth, as his fathers lived before him, tilling no more land than he can till with one horse and without machinery, never laying out a penny that he can help, studiously keeping up the appearance of poverty, and hoarding the little profits of his scanty crops and butter in an old stocking, till he can lodge them clandestinely in a bank, not too near, for the marriage portions of his daughters. It is vain to assure him that he may safely rely on the honour of an individual who may die to-morrow, or sell to a Dublin speculator, or be driven into a system of rack-renting by the pressure of his creditors. Why, he asks, should not the law secure to me an indefeasible right of possession, so long as I pay a fair rent, if this is what I may fairly expect from my landlord's sense of justice? Why should I be left absolutely at his mercy, and my children at the mercy of those who may succeed him, if it be admitted that it would be an abuse of power to confiscate my improvments, or even to disturb my occupancy? *

Such was the actual position of a representative Irish tenant before the Act of 1870, and such, in spite of the Act, is still the *sentiment* of a representative Irish tenant. For while the Act placed a heavy penalty on "disturbance," it laid no effectual restriction on the increase of rent. Moreover, as we are reminded, in the report of Lord Bessborough's Commission, "what the aggrieved tenant wants, in nearly all cases, is not to be compensated for the loss of his farm, but to be continued in its occupancy at a fair rent."

5. The case of the Irish labourer has received less attention than it deserves. He is somewhat roughly defined, in the Report of Lord Bessborough's Commission, as "a farmer who is without a farm." In other words, there is hardly such a thing in Ireland as an independent class of agricultural labourers. Most of those so described in the Census are small farmers working in spare hours, or sons of tenant-farmers, or perhaps men who have been turned out of farms for non-payment of rent. In the west of Ireland, however, and especially in Connaught, there are thousands of cottier tenants living on patches of land incapable of supporting a family, even if they were held rent-free, who eke out a livelihood by going over to England or Scotland for harvest, and returning with their wages, on which they mainly subsist during the winter. The misery of these poor creatures who returned empty-handed after the bad harvests of 1878 and 1879,

* This description of the Irish farmer has been extracted, with little variation, from an Essay on the Irish Land question in 1870, published in Brodrick's 'Political Studies,' 1879.

only to find their own potato crops destroyed by the blight, was among the main causes of the late agrarian agitation in Ireland. The leaders of the Land League seized eagerly upon it, and the crusade against rent, first preached in the wilds of Connemara, rapidly spread over all Ireland.

In picturing to ourselves the lot of these cottiers it is material to observe that, as the poor-law is more strictly administered in Ireland, they do not enjoy the same privilege of receiving outdoor relief as in most English counties. Now, although a "liberal" system of outdoor relief is a very doubtful boon to labourers, since it tends to pauperise them and reduce the rate of wages, the refusal of outdoor relief is specially hard to bear where regular wages are not always to be procured. The old English poor-law was virtually a compensation for those changes which had depressed the English peasant of the Middle Ages into a mere day-labourer. But for the poor-law, socialistic ideas would have propagated themselves long ago in the rural districts of England; and in Ireland, where poor-law relief is only given to able-bodied men in the workhouse, such ideas have actually propagated themselves with fearful rapidity. For the evicted cottier in Ireland can seldom find work in towns; his only resource, except the workhouse, is emigration—which he considers banishment—to England or the United States.

IV. We have now passed in review, however briefly, the distinctive and typical features of the English and Irish land systems. I have said that we are not specially concerned with their future development, but I cannot forbear to add a few words on the conditions which must govern it, and the general course which it may be expected to follow.

Speaking first of England, I desire to express my earnest conviction that no reforms of the English land system are likely to be permanent or beneficial which are not in harmony with the organic and apparently indestructible elements of our national character. The new rural economy of England must, above all, be essentially and thoroughly English. It cannot be modelled upon that of France, or Germany, or Russia, or Switzerland, or Italy, or Belgium, or the United States. On the other hand, we must beware, once more, of imagining that all the distinctive features of the English land system must needs be the spontaneous growth of the national character and history. We know, on the contrary, that in Saxon times the agrarian constitution of England was essentially democratic; that in Norman times ecclesiastics rather than barons were the pioneers of agricultural improvement, and the models of territorial benevolence; that in the England of Elizabeth, and for two centuries after the Reformation, the lesser gentry and yeomanry were the bone and sinew of the landed interest; that the dependent condition of English labourers dates from the poor-law, and that of English farmers from a far more recent period; that, in fact, the English land system is not an indigenous product of the soil, but an artificial creation of feudal

lawyers, matured by their successors in the evil days after the Restoration, largely modified by such temporary causes as the high prices current during the Great War, and afterwards strengthened by a constant flow of population towards great towns, partly consequent on the operation of the land system itself.

I will not conceal my belief that, before another generation has elapsed, the law of Primogeniture will have been abolished, that the power of entail will have been largely restricted, that by these means and by simpler methods of land transfer land will come to be divided among a larger number of owners, that, by degrees, more landlords will farm their own land, and more farmers will own the land which they cultivate, that leases will more and more be substituted for yearly tenancy, and that labourers, no longer divorced from the soil, but enabled to rise by industry into the class of farmers, will regain the self-respect and providence which are the special virtues of a true peasantry.

I will venture to read you a passage in which I endeavoured, a few years ago, to group together some of the effects likely to result from such a movement, and especially from the extension of the territorial aristocracy.*

"We may rest assured that no sudden or startling change would be wrought by so moderate a reform of the land system in the characteristic features of English country life. There would still be a squire occupying the great house in most of our villages, and this squire would generally be the eldest son of the last squire, though he would sometimes be a younger son of superior merit or capacity, and sometimes a wealthy and enterprising purchaser from the manufacturing districts. Only here and there would a noble park be deserted or neglected for want of means to keep it up and want of resolution to part with it; but it is not impossible that deer might often be replaced by equally picturesque herds of cattle; that landscape gardening and ornamental building might be carried on with less contempt for expense; that hunting and shooting might be reduced within the limits which satisfied our sporting forefathers; that some country gentlemen would be compelled to contract their speculations on the turf, and that others would have less to spare for yachting or for amusement at Continental watering-places. Indeed, it would not be surprising if greater simplicity of manners, and less exclusive notions of their own dignity, should come to prevail even among the higher landed gentry, leading to a revival of that free and kindly social intercourse which made rural neighbourhoods what they were in the olden times. The peculiar agricultural system of England might remain, with its threefold division of labour, between the landlord charged with the public duties attaching to property, the farmer contributing most of the capital and all the skill, and the labourer relieved by the assurance of continuous wages from all risks except

* See Brodrick's 'English Land and English Landlords,' 1881, pages 362–4.

that of illness. But the landlords would be a larger body, containing fewer grandees and more practical agriculturists, living at their country homes all the year round, and putting their savings into land, instead of wasting them in the social competition of the metropolis. The majority of them would still be eldest sons, many of whom, however, would have learned to work hard till middle life for the support of their families; and besides these there would be not a few younger sons who had retired to pass the evening of their days on little properties near the place of their birth, either left them by will or bought out of their own acquisitions. With these would be numbered other elements in far larger measure and greater variety than at present—wealthy capitalists eager to enter the ranks of the landed gentry; merchants, traders, and professional men content with a country villa and a hundred freehold acres around it; yeomen farmers, who had purchased the fee simple of their holdings from embarrassed landlords, and even labourers who had worked their way upwards and seized favourable chances of investing in land. Under such conditions, it is not too much to expect that some links, now missing, between rich and poor, gentle and simple, might be supplied in country districts; that "plain living and high thinking" might again find a home in some of our ancient manor houses, once the abode of landowners, but now tenanted by mere occupiers; that with less of dependence and subordination to a dominant will, there would be more of true neighbourly feeling, and even of clanship; and that posterity, reaping the happy fruits of greater social equality, would marvel, and not without cause, how the main obstacle to greater social equality—the law and custom of Primogeniture—escaped revision for more than two centuries after the final abolition of feudal tenures."

V. Let us lastly turn our eyes, once more, to the Irish land system, if that can be called a land system, which is a patchwork of antique customs and modern enactments, plastered one upon another, with little regard to consistency or symmetry. It is not my purpose to criticise the policy of the new Irish Land Bill; it may or may not be a necessary sequel to the Act of 1870; it may be framed in a spirit of justice, or it may have been dictated by political expediency. With all this we have nothing to do. What mainly concerns us is its self-evident tendency to establish a new form of double ownership, or agrarian partnership between landlord and tenant. We sometimes hear the great reforms carried out by Stein and Hardenberg in Prussia cited as a precedent for such legislation. Exactly the reverse is the fact. The reforms of Stein and Hardenberg were directed, and successfully directed, to substitute unity of ownership for double ownership—to give the peasant the greater part of his former holding as his own absolute property, relieved of all vexatious services; and to compensate the landlord for the loss of those services by a fixed allotment of land or by a fixed rent. In other words, these reforms substituted proprietorship for landlordism and tenancy, but left freedom of contract untouched. The reforms now proposed for Ireland assume the maintenance of the

relation between landlord and tenant, but place the regulation of it in the hands of a Court, and virtually abolish freedom of contract. There is no rashness in predicting that, under such circumstances, the relation will be found intolerable, and that in the end the same result at which Stein and Hardenberg deliberately aimed will be produced by the very opposite process. Either by the aid of facilities provided in the Bill itself, or by private agreements, Irish landlords will part with their estates in large numbers, and Irish tenants will be the nominal purchasers. Whether, having purchased, they will cultivate the land themselves, or convert themselves into squireens, whether they will keep out of the hands of money-lenders, and whether money-lenders may not become the worst of landlords, and whether those who chance to be without land just now will tamely acquiesce in their exclusion from the privileged caste of irremovable tenants—these are questions into which I must not wander. What is certain is that, come what may, the experiment of peasant ownership or farmer ownership will be tried in Ireland as it has never been tried before.

It may be said, with too much reason, that Irish tenants as a class have never yet exhibited the far-sighted industry which has become traditional and hereditary among French or Belgian peasants, and upon which peasant ownership in France or Belgium depends for its success. It may be said, on the other hand, that Irish tenants are already their own masters for most purposes, and that the civilising influence of country gentlemen with their families is already little felt in the great majority of Irish parishes. The transition from landlordism to farmer proprietorship would, therefore, be far gentler, and attended by much less of social change, in Ireland than in England. Nor must it be forgotten that French peasants, as described by Arthur Young a hundred years ago, did not differ widely from small Irish farmers in the present day. The "magic of property" has assuredly worked miracles in making them what they now are, and the magic of property is likely to be more potent in Ireland than in France, because it would place the new proprietor entirely above the influence of those agitators who now trade upon his wrongs, real or imaginary, and would give him a direct interest in the maintenance of law and order.

And thus it may come to pass that under the operation of different causes—some of them natural and some artificial, some in themselves pernicious, and some beneficent—the Irish land system may gravitate in the same direction as the English land system, and assume a more democratic aspect. Considering the history and national character of the Irish people, we are not warranted in forecasting with confidence the result of such a development. The utmost that we can affirm is that it affords a better prospect of a stable equilibrium in Ireland than the modern English form of rural economy. There is a fine passage in Edmund Spenser's 'View of Ireland,' written in the reign of Queen Elizabeth, where he supposes one of two friends to suggest various

remedies for the improvement of Ireland, and puts into the mouth of the other the following reply:—

"Marry, so there have been divers good plots devised, and wise counsels cast already about reformation of that realm; but they say it is the fatal destiny of that land, that no purposes, whatsoever are meant for her good, will prosper or take good effect; which whether it proceed from the very genius of the soil, or influence of the stars, or that Almighty God hath not yet appointed the time of her reformation, or that He reserveth her in this unquiet state still for some secret scourge which shall by her come into England, it is hard to be known but yet much to be feared."

It would be difficult to express in language more pathetic or appropriate the anxieties and misgivings which still oppress the most hopeful minds in legislating for Ireland, after the lapse of three hundred years. But despair can have no place in the counsels of statesmen. It cannot be that Ireland is eternally doomed either by the genius of her soil, or by the influence of the stars, or by the decrees of an inexorable Providence, to brood helplessly over her ancient wrongs, an unprofitable and irreconcilable member of the European family. There must surely be a happier and better future reserved for her too in the fulness of time, and this future may be expected to date from the day on which Parliament shall accomplish,—not a provisional and one-sided adjustment, but a comprehensive, just, and permanent settlement,—of the Irish land system.

GENERAL MONTHLY MEETING,

Monday, May 9, 1881.

George Busk, Esq. F.R.S. Treasurer and Vice-President, in the Chair.

The following Vice-Presidents for the ensuing year were announced:

William Bowman, Esq. F.R.S.
Frederick Joseph Bramwell, Esq. F.R.S.
Sir W. Frederick Pollock, Bart. M.A.
William Spottiswoode, Esq. D.C.L. Pres. R.S.
George Busk, Esq. F.R.S. Treasurer,
Warren De La Rue, Esq. M.A. D.C.L. F.R.S. Secretary.

Frederick Arthur Crisp, Esq.
Ernest Gye, Esq.
Samuel Armstrong Lane, Esq. F.R.C.S.

were elected Members of the Royal Institution.

JOHN TYNDALL, Esq. D.C.L. LL.D. F.R.S.

was re-elected Professor of Natural Philosophy.

The PRESENTS received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

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Antiquaries, Society of—Proceedings, Vol. VIII. No. 4. 8vo. 1880.
Asiatic Society of Bengal—1881, Proceedings, No. 2. 8vo.
Asiatic Society, Royal—Journal, Vol. XIII. Part 2. 8vo. 1881.
Astronomical Society, Royal—Monthly Notices, Vol. XLI. No. 5. 8vo. 1881.
Bankers' Institute—Journal, Vol. II. Parts 4, 5. 8vo. 1881.
Belgique Académie des Sciences, &c.:—
Mémoires Couronnés, Tome XXXIX. Partie 2. Tomes XLII. XLIII. 4to. 1879–80.
Tomes XXIX. XXX. XXXII. 8vo. 1880–1.
Tables, 1816–78. 2 Parts. 1858 and 1879.
Bulletins, 2e Serie, Tomes XLVI.–L. 8vo. 1879–80.
Annuaires. 1879–81. 16to.
Blackie, Professor J. Stuart, F.R.S.E. (the Author)—Gaelic Societies, Highland Depopulation, and Land Law Reform. (K 104) 8vo. 1880.
The Jewish Sabbath and the Christian Lord's Day. (K 104) 8vo. 1880.
British Architects, Royal Institute of—Proceedings, 1880–1. No. 16. 4to.
Cambridge Philosophical Society—Transactions, Vol. XIII. Part 1. 4to. 1881.
Proceedings, Vol. III. Parts 7, 8; Vol. IV. Part I. 8vo. 1881.
Chemical Society—Journal for April, 1881. 8vo.
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Cobden Club Committee:—
A. Mongredien—Free Trade and English Commerce. 16to. 1881.
Crisp, Frank, Esq. LL.B. F.L.S. &c. M.R.I. (the Editor)—Journal of the Royal Microscopical Society, Series II. Vol. I. Part 2. 8vo. 1881.
Crookes, W. Odling, W. and C. Meymott Tidy (the Authors)—Reports on London Water Supply, 1880–1. 4to.
Editors—American Journal of Science for April, 1881. 8vo.
Analyst for April, 1881. 8vo.
Athenæum for April, 1881. 4to.
Chemical News for April, 1881. 4to.
Engineer for April, 1881. fol.
Horological Journal for April, 1881. 8vo.
Iron for April, 1881. 4to.
Nature for April, 1881. 4to.
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Telegraphic Journal for April, 1881. 8vo.
Franklin Institute—Journal, No. 664. 8vo. 1881.
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WEEKLY EVENING MEETING,

Friday, May 13, 1881.

Frederick Joseph Bramwell, Esq. F.R.S. Vice-President, in the Chair.

Francis Galton, Esq. M.A. F.R.S. *M.R.I.*

Mental Images and Vision.

[Abstract deferred.]

WEEKLY EVENING MEETING,

Friday, May 20, 1881.

William Spottiswoode, Esq. M.A. D.C.L. Pres. R.S. Vice-President, in the Chair.

Walter H. Pollock, Esq. M.A.

Shakspeare Criticism.

The speaker began by noticing some of the absurd theories respecting Shakspeare's life and works.

Hume the historian said: "If Shakspeare be considered as a man born in a rude age and educated in the lowest manner, without any instruction from books or from the world, he may be regarded as a prodigy. If represented as a poet capable of furnishing a proper entertainment to a refined or an intelligent audience, we must abate much of this eulogy. A striking peculiarity of sentiment adapted to a single character he frequently hits, as it were, by inspiration; but a reasonable propriety of thought he cannot for any time uphold. Nervous and picturesque expressions, as well as descriptions, abound in him; but it is in vain we look for purity or simplicity of diction." Goldsmith makes "The Vicar of Wakefield" say, "Can the present age be pleased with that antiquated dialect, that obsolete humour, those over-charged characters?" Voltaire said 'Hamlet' was "the dream of a drunken savage with some flashes of beautiful thoughts."

* * * * * *

Gervinus truly says that, after two hundred and fifty years of commentators' digging, as in a mine, Shakspeare has remained an enigma to the literary world. This is due to a great error. Shakspeare did not write for studious reading in the closet, but for representation on the stage for ordinary understanding. He often wrote carelessly; not at all as if every word and line were to be critically discussed. In fact, his plays were at first surreptitiously printed, which was considered injurious to his reputation.

* * * * * *

After reading some of Mr. Pepys's amusing comments on the renewed performance of Shakspeare at the Restoration in 1660, Mr. Pollock gave specimens of the sacrilegious manner in which the plays had been dealt with by Davenant, Dryden, and others in the seventeenth century. He referred especially to 'Romeo and Juliet,' as dealt with by Otway, in which are striking examples of the "art of sinking," and also to the Duke of Buckingham's alterations of 'Julius Cæsar.'

* * * * * *

Mr. Pollock then read the story on which 'Hamlet' was founded, from Saxo-Grammaticus, the Danish historian, to show the way

in which Shakspeare's genius ennobled his crude materials, after which he discussed some of the criticisms and comments on 'Hamlet,' as an example of the way in which the poet had been dealt with.

"In Warburton," writes Gervinus, "in Johnson, and in Steevens (the most intelligent of all) there are excellent explanations of certain passages, traits, and characters which burst forth amid prejudices and false judgment, as proofs of how the greatness of the poet prevailed more and more even over the narrow minds of their criticisers. In accordance with this partial investigation and with these passing flashes of perception, alternating with greater darkness, was the treatment of Shakspeare on the stage both in Germany and England. The jubilee, two hundred years after Shakspeare's birth, celebrated in Stratford-on-Avon in 1764, denotes about the time when the poet's works were revived by Garrick on the English stage." It may be well to remark, what Gervinus merely alludes to at this point, that Garrick's representations were very far from being,—apart from his own acting,—what would satisfy any critical audience nowadays. The alterations to which Gervinus presently alludes were often of a most disastrous kind, notably in the case of the complete excision of the gravediggers in 'Hamlet.' It is a minor point that Garrick played Shakspeare's characters in the costume of his own (Garrick's) time, according to the then universal custom. The German critic goes on to assert, and it might be difficult to quarrel with the assertion, that "the man who first valued Shakspeare according to his full desert was indisputably Lessing. With all the force of a true taste, he pointed to Wieland's translation of the English dramatist when scarcely any one in Germany knew him." Lessing, it will be remembered, was born in 1729 and died in 1781, and it can hardly be doubted that, as Gervinus says, his influence in Germany caused a reaction in England, until "when Nathan Drake in 1817 published his ample work upon 'Shakspeare and his Times' the idolatry of the poet had passed already to his native land." It is a pity that Gervinus, who goes on to dwell with pleasure on the wide-spread interest in Shakspeare again excited of late years in England, could not see the artistic homage now paid to him on the stage of what has become our leading theatre.

* * * * * *

The only modern version of 'Hamlet' which has obtained any real success on the stage in France was the one written by the great Dumas, who had a keen appreciation of the genius of Shakspeare, and who, indeed, was first fired to write for the stage by seeing Shakspeare's plays represented in Paris by an English company in English. Upon the arrival of this company, "I had never read," he writes in his memoirs, "one play of the foreign drama. They put up 'Hamlet.' I only knew the 'Hamlet' of Ducis. I went to see the 'Hamlet' of Shakspeare. This it was that I had longed for; this it was that I had ever felt the want of. It was the players forgetting the traditions of the stage, it was the imaginary life that art made

real; it was this presentment by actors of human beings—not of stilted heroes with the unfeeling and conventional declamation of the stage. I saw Romeo, Virginius, William Tell, Shylock, Othello; I saw Macready, Kean, and Young. Then I read, I devoured the library of the foreign stage, and I saw that in the world of the drama all springs from Shakspeare, as in the greater world all springs from the sun. I saw that no writer could be compared with him. He was as dramatic as Corneille, as comic as Molière, as daring as Calderon, as thoughtful as Goethe, as passionate as Schiller."

* * * * * *

After commenting on various foreign translations of Shakspeare, Mr. Pollock cited the following as a specimen of some of the very strange annotations and translations. Moratin, the Spanish translator, in a note says: "They paint now the Omnipotent in the act of hurling thunderbolts at man; indeed it is quite common. But to imagine the Almighty discharging a park of artillery at him, is certainly something very new; whilst it should be noted that in the time of Hamlet there were neither cannons nor gunpowder." This amazing note is explained by the fact that he translates:

> "Or that the Everlasting had not fixed
> His canon 'gainst self-slaughter,"

as follows:

> "O! el Todopoderoso no asestára
> El cañon contra el homicida de si mismo."

Later translators substitute *fusil* (gun) for cañon.

In conclusion, Mr. Pollock read the beautiful version of the Willow Song by the elder Dumas.

WEEKLY EVENING MEETING,

Friday, May 27, 1881.

William Bowman, Esq. LL.D. F.R.S. Vice-President, in the Chair.

H. E. Roscoe, Esq. LL.D. F.R.S. &c.
PRESIDENT OF THE CHEMICAL SOCIETY.

Indigo, and its Artificial Production.

More than eleven years ago the speaker had the pleasure of bringing before this audience a discovery in synthetic chemistry of great interest and importance, viz. that of the artificial production of alizarin, the colouring substance of madder. To-day it is his privilege to point out the attainment of another equally striking case of synthesis, viz. the artificial formation of indigo. In this last instance, as in the former case, the world is indebted to German science, although to different individuals, for these interesting results, the synthesis of indigo having been achieved by Professor Adolf Baeyer, the worthy successor of the illustrious Liebig in the University of Munich. Here then we have another proof of the fact that the study of the most intricate problems of organic chemistry, and those which appear to many to be furthest removed from any practical application, are in reality capable of yielding results having an absolute value measured by hundreds of thousands of pounds.

In proof of this assertion, it is only necessary to mention that the value of the indigo imported into this country in the year 1879 reached the enormous sum of close on two millions sterling, whilst the total production of the world is assessed at twice that amount; so that if, as is certainly not impossible, artificial indigo can be prepared at a price which will compete with the native product, a wide field is indeed open to its manufacturers.

Indigo, as is well known, is a colouring matter which has attracted attention from very early times. Cloth dyed with indigo has been found in the old Egyptian tombs. The method of preparing and using this colour is accurately described by both Pliny and Dioscorides, and the early inhabitants of these islands were well acquainted with indigo, which they obtained from the European indigo plant, *Isatis tinctoria*, the woad plant, or pastel. With this they dyed their garments and painted their skins. After the discovery of the passage to India by the Cape of Good Hope, the eastern indigo, derived from various species of *Indigofera*, gradually displaced woad, as containing more of the colouring matter. But this was not accomplished without great opposition from the European growers of

woad; and severe enactments were promulgated against the introduction of the foreign colouring matter, an edict condemning to death persons "who used that pernicious drug called devil's food" being issued by Henry the Fourth of France. The chief source of Indian indigo is the *Indigofera tinctoria*, an herbaceous plant raised from seed which is sown in either spring or autumn. The plant grows with a single stalk to a height of about three feet six inches, and about the thickness of a finger. It is usually cut for the first time in June or July, and a second or even a third cutting obtained later in the year. The value of the crop depends on the number of leaves which the plant puts forth, as it is in the leaves that the colouring principle is chiefly contained. Both the preparation of the colouring matter from the plant, and its employment as a dye or as a paint, are carried on at the present day exactly as they have been for ages past. The description of the processes given by Dioscorides and Pliny tally exactly with the crude mode of manufacture carried on in Bengal at the present day as follows:—

"The Bengal indigo factories usually contain two rows of vats, the bottom of one row being level with the top of the other. Each series numbers from fifteen to twenty, and each vat is about 7 yards square and 3 feet deep; they are built of brickwork lined with stone or cement. About a hundred bundles of the cut indigo plants are placed in each vat in rows, and pressed down with heavy pieces of wood; this is essential to the success of the operation. Water is then run in so as to completely submerge the plants, when a fermentation quickly ensues, which lasts from nine to fourteen hours, according to the temperature of the atmosphere. From time to time a small quantity of the liquor is taken from the bottom of the vat to see how the operation is proceeding. If the liquor has a pale-yellow hue the product obtained from it will be far richer in quality, but not so abundant as if it had a golden-yellow appearance. The liquor is then run off into the lower vats, into which men enter and agitate it by means of bats or oars, or else mechanically by means of a dash-wheel, each vat requiring seventeen or eighteen workpeople, who are kept employed for three or four hours. During the operation, the yellow liquor assumes a greenish hue, and the indigo separates in flakes. The liquor is then allowed to stand for an hour, and the blue pulpy indigo is run into a separate vessel, after which it is pumped up into a pan and boiled, in order to prevent a second fermentation, which would spoil the product by giving rise to a brown matter. The whole is then left to stand for twenty hours, when it is again boiled for three or four hours, after which it is run on to large filters, which are placed over vats of stonework about 7 yards long, 2 yards wide, and 1 yard deep. The filters are made by placing bamboo canes across the vats, covering these with bass mats, and over all stretching strong canvas. The greater part of the indigo remains under the form of a dark blue or nearly black paste, which is introduced into small wooden frames having holes at the bottom and

lined with strong canvas. A piece of canvas is then placed on the top of the frame, a perforated wooden cover, which fits into the box, put over it, and the whole submitted to a gradual pressure. When as much of the water as possible has been squeezed out the covers are removed, and the indigo allowed to dry slowly in large drying sheds, from which light is carefully excluded. When dry, it is ready for the market. Each vat yields from 36 to 50 lbs. of indigo." *

The same process carried out in the times of the Greeks is thus described by Dioscorides: "Indigo used in dyeing is a purple coloured froth formed at the top of the boiler; this is collected and dried by the manufacturer; that possessing a blue tint and being brittle is esteemed the most."

The identity of the blue colouring matter of woad and that of the Bengal plant was proved by Hellot, and by Planer and Trommsdorff at the end of the last century. These latter chemists showed that the blue colour of the woad can be sublimed, and thus obtained in the pure state, a fact which was first mentioned in the case of indigo by O'Brien, in 1789, in his treatise on calico printing. Indigo thus purified is termed indigotin. It has been analysed by various chemists, who ascertained that its composition may be most simply expressed by the formula C_8H_5NO.

Indigo is a blue powder, insoluble in water, alkalis, alcohol, and most common liquids. In order to employ it as a dyeing agent it must be obtained in a form in which it can be fixed or firmly held on to the fibres of the cloth. This is always effected by virtue of a property possessed by indigo-blue of combining with hydrogen to form a colourless body, soluble in alkalis, known as indigo-white, or reduced indigo, of which the simplest formula is C_8H_6NO. This substance rapidly absorbs oxygen from the air and passes into the blue insoluble indigo, which, being held in the fibre of the cloth, imparts to it a permanent blue dye. This reduction to white indigo may be effected in various ways. The old cold vat, or blue-dip vats as they are termed, consist of a mixture of indigo, slacked lime, and green vitriol. The latter salt reduces the indigo, and the white indigo dissolves in the lime water. This process of indigo dyeing is both expensive and troublesome, owing to loss of indigo and formation of gypsum, so that many plans have been proposed to remedy these evils.

Concerning the origin of indigo in the leaves of the *Indigofera*, various and contradictory views have been held. Some have supposed that blue indigo exists ready formed in the plant; others, that white indigo is present, which on exposure to air is converted into indigo-blue. Schunck has, however, proved beyond doubt that the woad plant (*Isatis tinctoria*), the *Indigofera tinctoria* of India, and the Chinese and Japanese indigo plant (*Polygonum tinctorium*) contain neither indigo-blue or white indigo ready formed. It is now known

* Crace-Calvert.

that by careful treatment the leaves of all these indigo-yielding plants can be shown to contain a colourless principle termed indican, and that this easily decomposes, yielding a sugar-like body and indigo-blue. That white indigo is not present in the leaves is proved by the fact that this compound requires an alkali to be present in order to bring it into solution, whereas the sap of plants is always acid. The decomposition is represented by Schunck as follows:—

$$\underset{\text{Indican.}}{C_{26}H_{31}NO_{17}} + 2H_2O = \underset{\text{Indigotin.}}{C_8H_5NO} + \underset{\text{Indiglucin.}}{3C_6H_{10}O_6}.$$

So readily does this change from indican to indigo take place, that bruising the leaf or exposing it to great cold is sufficient to produce a blue stain. Even after mere immersion in cold alcohol or ether, when the chlorophyll has been removed, the leaves appear blue, and this has been taken to show the pre-existence of indigo in the plant. But these appearances are deceptive, for Schunck has proved that if boiling alcohol or ether be used, the whole of the colour-producing body as well as the chlorophyll is removed, the leaves retaining only a faint yellow tinge, whilst the alcoholic extract contains no indigo-blue, but on adding an acid to this liquid the indican is decomposed and indigo-blue is formed.

Passing now to the more immediate subject of his discourse, the speaker again reminded his hearers that indigo was the second natural colouring matter which has been artificialy prepared; alizarin the colouring matter of the madder-root being the first. As a rule, the simpler problems of synthetic chemistry are those to which solutions are the soonest found, and these instances form no exception to the rule. The synthetic production of indigo is a more difficult matter than the artificial formation of alizarin, and hence the speaker did not apologise for leading up to the complex through the more simple phenomenon.

When the ingenious Japanese workman who had never seen a watch had one given to him with an order to make a duplicate, he took the only sensible course open to him, and carefully pulled the watch to pieces, to see how the various parts were connected together. Having once ascertained this, his task was a comparatively easy one, for he then had only to make the separate parts, and fit them together, and he thus succeeded so well in imitating the real article that no one could tell the difference. So it is with the chemist, until he knows how the compound is built up, that is, until he has ascertained its constitution, any attempt at synthesis is more like groping in the dark than like shaping the course by well-known landmarks into harbour.

In the case of alizarin it was comparatively easy to reduce it to its simplest terms, and to show that the backbone of this colouring matter is anthracene $C_{14}H_{10}$, a hydro-carbon found in coal-tar. This

fact being ascertained, the next step was the further process of clothing the hydro-carbon by adding four atoms of oxygen and subtracting the two atoms of hydrogen present in excess, and this was soon successfully accomplished, so that now, as we know, artificial alizarin has excluded the natural colouring matter altogether.

$C_{14}H_{10}$	$C_{14}H_6O_2(OH)_2$.
Anthracene.	Alizarin.

What now was the first step gained in our knowledge concerning the constitution of indigo, of which the simplest formula is C_8H_5NO?

STEP No. 1.—This was made so long ago as 1840, when Fritsche proved that aniline, $C_6H_5NH_2$, can be obtained from indigo. The name for this now well-known substance is indeed derived from the Portuguese "anil," a word used to designate the blue colour from indigo. This result of Fritsche's is of great importance, as showing that indigo is built up from the well-known benzene ring C_6H_6, the skeleton of all the aromatic compounds, and moreover that it contains an amido-group.

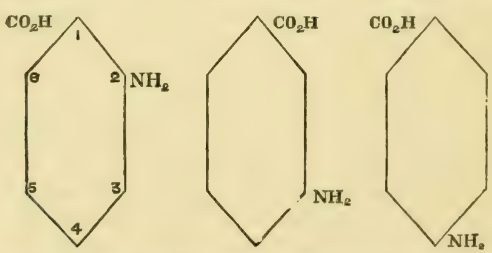
STEP No. 2 was also made by Fritsche in the following year, when, by boiling indigo with soda and manganese dioxide, he obtained ortho-amido-benzoic acid, or, as he then termed it, anthranilic acid. The following is the reaction which here occurs:—

$$C_8H_5NO + O + 2H_2O = C_7H_5NH_2O_2 + CH_2O_2.*$$

Indigo. Ortho-amido-benzoic acid.

What light does this fact shed upon the constitution of indigo? It shows (1) that one of the eight atoms of carbon in indigo can be readily separated from the rest; (2) that the carboxyl and the amido-

Ortho-position. Meta-position. Para-position.



group are in neighbouring positions in the benzene ring, viz. 1 and 2. For we have three isomeric acids of the above composition.

* Bottinger, Deut. Chem. Ges. 1877, i. 269.

STEP No. 3.—The next advance of importance in this somewhat complicated matter is the discovery by Erdmann and Laurent independently, that indigo on oxidation yields a crystalline body, which, however, possesses no colouring power, to which they gave the name of isatin.

$$\underset{\text{Indigo.}}{C_8H_5NO} + O = \underset{\text{Isatin.}}{C_8H_5NO_2}.$$

STEP No. 4.—The reverse of this action, viz. the reduction of isatin to indigo, was accomplished by Baeyer and Emmerling in 1870 and 1878, by acting with phosphorus pentachloride on isatin, and by the reducing action of ammonium sulphide on the chloride thus formed.

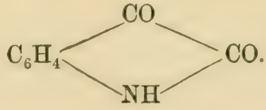
Understanding now something of the structure and of the relationships of the body which we wish to build up, let us see how this edifice has, in fact, been reared. Three processes have been successfully employed for carrying out this object. But of these three only one is of practical importance. A synthetic process may yield the wished-for result, but the labour incurred may be too great and the losses during the campaign may be too severe to render it possible to repeat the operation with advantage on a large scale; just as it costs, at the usual rate of wages, more than twenty shillings to wash a sovereign's worth of gold out of the Rhine sands, so that this employment is only carried on when all other trades fail.

For the sake of completeness, let us, however, consider all three processes, although Nos. 1 and 2 are at present beyond the pale of practical schemes.

These three processes have certain points in common. (1) They all proceed from some compound containing the benzene nucleus. (2) They all start from compounds containing a nitrogen atom. (3) They all commence with an ortho-compound.

They differ from one another; inasmuch as process No. 1 starts from a compound containing seven atoms of carbon (instead of eight), and to this, therefore, one more atom must be added; process No. 2, on the other hand, starts from a body which contains exactly the right number (eight) of carbon atoms; whilst No. 3 commences with a compound in which nine atoms of carbon are contained, and from which, therefore, one atom has to be abstracted before indigo can be reached.

Process No. 1 (Kekulé—Claissen and Shadwell).—So long ago as 1869 Kekulé predicted the constitution of isatin, and gave to it the formula which we now know that it possesses, viz.



Following up this view, Claissen and Shadwell, two of Kekulé's pupils, succeeded in preparing isatin, and, therefore, now indigo, from ortho-nitro-benzoic acid.

The following are the steps in the ascent:—

1. Ortho-nitro-benzoic acid acted on by phosphorus pentachloride yields the chloride $C_6H_4(NO_2)COCl$.
2. This latter heated with silver cyanide yields the nitril $C_6H_4(NO_2)CO.CN$.
3. On heating this with caustic potash it yields ortho-nitro-phenyl-glyoxylic acid, $C_6H_4(NO_2)CO.CO_2H$.
4. This is converted by nascent hydrogen into the amido-compound $C_6H_4(NH_2)CO.CO_2H$.
5. And this loses water and yields isatin, $C_6H_4NH.CO.CO$.

(Q. E. D.)

The reasons why this process will not work on a large scale are patent to all those who have had even bowing acquaintance with such unpleasant and costly bodies as phosphorus pentachloride or cyanogen.

Process No. 2.—Baeyer's (1878) synthesis from ortho-nitro-phenyl acetic acid.

This acid can be obtained synthetically from toluol, and it is first converted into the amido-acid, and which, like several ortho-compounds, loses water, and is converted into a body called oxindol, from which isatin, and therefore indigo, can be obtained. The precise steps to be followed are:—

1. Ortho-amido-phenylacetic yields oxindol:

$$C_6H_4{<}\begin{matrix}CH_2CO_2H\\NH_2\end{matrix} \quad = \quad C_6H_4{<}\begin{matrix}CH_2\\NH\end{matrix}{>}CO \quad + \quad H_2O.$$

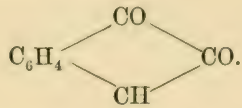
2. This on treatment with nitrous acid yields nitrosoxindol:

$$C_6H_4{<}\begin{matrix}C(NO)\\NH\end{matrix}{>}CO.$$

3. This again with nascent hydrogen gives amidoxindol:

$$C_6H_4{<}\begin{matrix}CH(NH_2)\\NH\end{matrix}{>}CO.$$

4. Which on oxidation gives isatin,



(Q. E. D.)

This process, the feasibility of which had also been foreseen by Kekulé, is however not available as a practical scheme for various reasons.

Process No. 3.—This may be called the manufacturing process, and was also proposed by Baeyer. It starts from cinnamic acid, a substance contained in gum benzoin, balsam of Peru, and some few other aromatic bodies. These sources are, however, far too expensive to render this acid thus obtained available for manufacturing purposes. But Bertagnini, in 1856, had obtained cinnamic acid artificially from oil of bitter almonds, and other processes for the same purpose have since been carried out. Of these, that most likely to be widely adopted is the following practical modification by Dr. Caro of Mr. Perkin's beautiful synthesis of cinnamic acid:—

1. $\underset{\text{Toluene.}}{C_6H_5CH_3} + 4Cl = \underset{\text{Benzylene dichloride.}}{C_6H_5CHCl_2} + 2HCl.$

2. $\underset{\text{Benzylene dichloride.}}{C_6H_5CHCl_2} + \underset{\text{Sodium acetate.}}{2CH_3.CO.O.Na} = \underset{\text{Cinnamic acid.}}{C_6H_5CH{=}CH.CO.OH.} + 2Nacl.$

But why did Baeyer select this nine-carbon acid from which to prepare indigo? For this he had several reasons. In the first place, it had long been known that all indigo compounds when heated with zinc-dust yield indol, C_8H_7N, a body which stands therefore to indigo in the same relation as anthracene to alizarin, and Baeyer and Emmerling had so long ago as 1869 prepared this indol from ortho-nitro-cinnamic acid thus:—

$$C_8H_6(NO_2)CO_2H = C_8H_7N + O_2 + CO_2.$$

Secondly, the ortho-nitro-cinnamic acid required (for we must remember that indigo is an ortho-compound and also contains nitrogen) can be readily prepared from cinnamic acid, and this itself again can be obtained on a large scale. Thirdly, this acid readily parts with one atom of carbon, and thus renders possible its conversion into eight-carbon indigo.

The next steps in the process are (3) the formation of ortho-nitro-cinnamic acid, (4) the conversion of this into its dibromide, (5) the separation from this of the two molecules of hydrobromic

acid, giving rise to ortho-nitro-phenyl-propiolic acid, and (6), and lastly, the conversion of this latter into indigo by heating its alkaline solution with grape sugar, xanthate of soda, or other reducing agent. These reactions are thus represented :—

3. $C_6H_5CH{=}CHCOOH$ yields $C_6H_4(NO_2)CH{=}CH.COOH$.
Cinnamic acid — Ortho-nitro-cinnamic acid.

In this process the para-acid is also obtained, and as this is useless for the manufacture of indigo, it has to be removed. This is effected by converting the acids into their ethyl ethers, which, possessing different degrees of solubility, can be readily separated from one another.

4. This is next converted into the dibromide

$$C_6H_4(NO_2)CHBr.CHBrCOOH.$$

5. And by careful treatment with caustic soda this yields ortho-nitro-phenyl-propiolic acid, thus :—

$$C_6H_4(NO_2)CHBrCHBrCOOH + 2NaOH = C_6H_4(NO_2)C_2.COOH + 2NaBr + 2H_2O.$$

6. $n[C_6H_4(NO_2)C_2.COOH + H_2 = C_8H_5NO + CO_2 + H_2O]$.
Ortho-nitro-phenyl-propiolic acid. Indigotin.

(Q. E. D.)

The last of these reactions is in reality not so simple as the equation indicates. For only about 40 per cent. of indigo is obtained, whereas according to theory 68 per cent. should result. Indeed, although, as we have seen, indigo can be prepared by these three methods, chemists are as yet in doubt as to its molecular weight, the probability being that the molecule of indigo contains twice 16 atoms of carbon, or has the formula $4(C_8H_5NO)$ or $C_{32}H_{20}N_4O_4$. Still it must be remembered that according to Sommaruga the vapour density of indigo is 9·45, a number corresponding to the simpler formula $C_{16}H_{10}N_2O_2$.

The artificial production of indigo may even now be said to be within measurable distance of commercial success, for the ortho-nitro-phenyl-propiolic acid, the colourless substance which on treatment with a reducing agent yields indigo-blue, is already in the hands of the Manchester calico printers, and is furnished by the Baden Company for alkali and aniline colours at the price of 6*s*. per lb. for a paste containing 25 per cent. of the dry acid.

With regard to the nature of the competition between the artificial and the natural colouring matters it is necessary to say a few words. In the first place, the present price at which the manufacturers are able to sell their propiolic acid is 50*s*. per kilo. But 100 parts of this can only yield, according to theory, 68·58 parts of indigo-blue,

so that the price of the artificial (being 73*s.* per kilo.) is more than twice that of the pure natural colour. Hence competition with the natural dye-stuff is not to be thought of until the makers can reduce the price of dry propiolic acid to 20*s.* per kilo., and also obtain a theoretical yield from their acid. This may, or it may not, be some day accomplished, but at present it will not pay to produce indigo from nitro-phenyl-propiolic acid. Nevertheless a large field lies open in the immediate future for turning Baeyer's discovery to practical account. It is well known that a great loss of colouring matter occurs in all the processes now in use for either dyeing or printing with indigo. It has already been stated that a large percentage of indigo is lost in the "cold vats" in the sediment. Another portion is washed off and wasted after the numerous dippings, whilst in order to produce a pattern much indigo must be destroyed before it has entered into the fibre of the cloth. Moreover, the back of the piece is uselessly loaded with colour. In the processes of printing with indigo the losses are as great, or even greater, and, in addition, such considerable difficulties are met with that only a few firms (Potter, Grafton in Manchester, and Schlieper in Elberfeld) have been successful in this process. But a still more important fact remains, that no printing process exists in which indigo can be used in combination with other colours in the ordinary way, or without requiring some special mode of fixing after printing. Hence it is clear that the weak points of natural indigo lie in the absence of any good process for utilising the whole of its colouring matter, and in the impossibility, or at any rate, great difficulty of employing it in the ordinary madder styles of calico printing. Such were the reasons which induced the patentees to believe that although the artificial dye cannot be made at a price to compete with natural indigo for use in the ordinary dye-beck, it can even now be very largely used for styles to which the ordinary dye-stuff is inapplicable.

To begin with, Baeyer employed (Patent 1177) grape sugar as a reducing agent. The reduction in this case does not take place in the cold, and even on long standing only small traces of indigo are formed, but if heated to 70° or upwards the change takes place. Unfortunately this production of indigo-blue is rapidly followed by its reduction to indigo-white, and it is somewhat difficult in practice to stop the reaction at the right moment. But "necessity is the mother of invention," and Dr. Caro of Mannheim, to whom the speaker is greatly indebted for much of the above information, found that sodium xanthate is free from many of the objections inherent to the glucose reduction process, inasmuch as the reaction then goes on in the cold. Moreover, he finds that the red isomeride of indigo-blue, Indirubin, which possesses a splendid red colour, also occurring in natural indigo, but whose tinctorial power is less than that of the blue, is produced in less quantity in this case than when glucose is employed. On this cloth, alumina and iron mordants may be printed, and this afterwards dyed in alizarin, &c., or this

colouring matter may also be printed on the cloth and the colour fixed by moderate steaming without damage to the indigo-blue. This process is now in actual use by printers both in England and on the Continent, so that, thanks especially to the talent and energy of Dr. Caro, Baeyer's discovery has been practically applied within the short space of twelve months of its conception. Operations on a manufacturing scale have been successfully carried on in the Baden Soda and Aniline Works at Ludwigshafen for the last two months, and the directors see no reason why they should not be able to supply any demand, however great, which may be made for ortho-nitro-phenyl-propiolic acid.

The proper way of looking at this question at present is, therefore, to consider ortho-nitro-phenyl-propiolic acid and indigo as two distinct products not comparable with each other, inasmuch as the one can be put to uses for which the other is unfitted, and there is surely scope enough for both. Still, looking at the improvements which will every day be made in the manufacturing details, he must be a bold man who would assert the impossibility of competition with indigo in all its applications. For we must remember that we are only at the beginning of these researches in the indigo field. Baeyer and other workers will not stay their hands, and possibly other colouring matters of equal intensity and of equal stability to indigo may be obtained from other as yet unknown or unrecognised sources, and it is not improbable that these may turn out to be more formidable competitors in the race with natural indigo than ortho-nitro-phenyl-propiolic acid.

Looking at this question of the possible competition of artificial with the natural indigo from another point of view, it must, on the other hand, be borne in mind that the present mode of manufacturing indigo from the plant is extremely rude and imperfect, and that by an improved and more careful carrying out of the process, great saving in colouring matter may be effected, so that it may prove possible to produce a purer article at a lower price, and thus to counterbalance the production of the artificial material.

The following are the directions issued by the patentees to calico printers for using the new colour :—

PRINTING WITH ARTIFICIAL INDIGO.

No. I.—On Unprepared Cloth.

Standard.

Take 4 lb. propiolic acid paste (equal to 1 lb. dry acid), and 1 lb. borax finely powdered ; mix well. The mixture first becomes fluid and at last turns stiff. Then add 3 quarts white starch thickening (wheat starch), mix well, and strain.

Printing Colour.

Take the above standard and dissolve in it immediately before printing $1\frac{1}{2}$ lb. xanthate of soda, stir well, and ready for use.

For lighter shades reduce the above printing colour with the following: In 1 gallon white starch paste dissolve 1 lb. xanthate of soda.

Directions for use.—Print and dry as usual. The pieces ought not to be placed in immediate contact with drying cylinders, or otherwise be subjected to heat above 100° C. The indigo-blue is best developed by allowing the printed goods to remain in a dry atmosphere and at an ordinary temperature for about 48 hours. Damp air ought to be excluded as much as possible until the colour is fully developed. Then the pieces may be passed through the ageing machine, or steamed at low pressure if such treatment should be required for fixing any other colour or mordant printed along with the indigo-blue.

After the blue is ready formed, the pieces are first thoroughly washed in the washing machine and *then boiled* in the clean water, or better, in a weak solution of hyposulphite of soda (1 lb. to 10 gallons), *and at a full boil* for half an hour in order to volatilise the smell which would otherwise adhere to the goods.

Clean in a soap-bath, at a temperature not above 40° C.; wash, dry, and finish.

Observations.—Wheat starch gives the best results in the colour, then follows gum tragacanth. The colour is considerably reduced by using gum senegal, dark British gum, or calcined farina as thickening materials.

So far borax has answered best as an alkaline solvent of propiolic acid, it may however be replaced in the above standard by acetate of soda (from 1 to $1\frac{1}{2}$ lb.) or by 6 oz. pearlash or soda. Any excess of caustic-potash, or soda, destroys propiolic acid.

The above standard keeps unchanged for any length of time, it is likewise not sensibly altered by a small amount of xanthate of soda, but when mixed with its full proportion of xanthate, as in the above printing colour, it gradually loses strength after several hours.

The xanthate ought therefore to be mixed with the standard immediately before printing, and any colour remaining unused may then be saved by mixing with the same a large proportion of starch paste.

Propiolic acid may be printed along with aniline black, catechu brown and drabs, and with alumina and iron mordants for madder colours.

After the indigo-blue is fully developed, the mordants are fixed in the ordinary manner, dyed with alizarin, padded with Turkey-red oil, steamed, and otherwise treated as usual.

Indigo-blue, whether natural or artificial, suffers by prolonged

steaming at high pressure. For this reason, only such steam colours can be associated with propiolic acid as may be fixed by short steaming at low pressure.

No. II.—On Prepared Cloth (for Full Shades).

Dissolve 2 lb. of xanthate of soda in 1 gallon of cold water. Pad the goods with the above; dry, print with standard, and after printing follow the above treatment. The pieces may also be first printed with xanthate and then covered with standard. Alumina and iron mordants for madder colours may be likewise printed on cloth thus prepared, or printed with xanthate of soda.

The potential importance, from a purely commercial point of view, of the manufacture, may be judged of by reference to the following statistics, showing that the annual value of the world's growth of indigo is no less than four millions sterling.

Estimated Yearly Average of the Production of Indigo in the World, taken from the Total Crop for a Period of Ten Years.

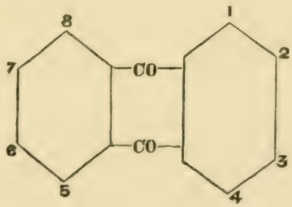
	Pounds Weight.	Pounds Sterling.
Bengal, Tirhoot, Benares, and N.-W. India ..	8,000,000	2,000,000
Madras and Kurpah	2,200,000	400,000
Manilla, Java, Bombay, &c.	..	500,000
Central America	2,250,000	600,000
China and elsewhere, consumed in the country	..	Say 500,000
		4,000,000

How far the artificial will drive out the natural colouring matter from the market cannot, as has been said, be foreseen. It is interesting, as the only instance of the kind on record, to cast a glance at the history of the production of the first of the artificial vegetable colouring matters, alizarin. In this case the increase in the quantity produced since its discovery in 1869 has been enormous, such indeed that the artificial colour has now entirely superseded the natural one, to the almost complete annihilation of the growth of madder-root. It appears that whilst for the ten years immediately preceding 1869 the average value of the annual imports of madder-root was over one million sterling, the imports of the same material during last year (1880) amounted only to 24,000*l.* The whole difference being made up by the introduction of artificial alizarin. In 1868, no less a quantity than 60,000 tons of madder-root were sent into the market, this containing 600,000 kilos. of pure natural alizarin. But in ten years later a quantity of artificial alizarin more than equal to the above

amount was sent out from the various chemical factories. So that in ten years the artificial production had overtaken the natural growth, and the 300,000 or 400,000 acres of land which had hitherto been used for the growth of madder, can henceforward be better employed in growing corn or other articles of food. According to returns, for which the speaker had to thank Mr. Perkin, the estimated growth of madder in the world previous to 1869 was 90,000 tons, of the average value of 45*l.* per ton, representing a total of 4,050,000*l.*

Last year (1880) the estimated production of the artificial colouring matter was 14,000 tons, but this contains only 10 per cent. of pure alizarin. Reckoning 1 ton of the artificial colouring matter as equal to 9 tons of madder, the whole artificial product is equivalent to 126,000 tons of madder. The present value of these 14,000 tons of alizarin paste, at 122*l.* per ton, is 1,568,000*l.* That of 126,000 tons of madder at 45*l.* is 5,670,000*l.*, or a saving is effected by the use of alizarin of considerably over four millions sterling. In other words, we get our alizarin dyeing done now for less than one-third of the price which we had to pay to have it done with madder.

Our knowledge concerning the chemistry of alizarin has also proportionately increased since the above date. For whilst at that time only one distinct body having the above composition was known, we are now acquainted with no less than nine out of the ten di-oxyanthraquinones whose existence is theoretically possible, according as the positions of the two semi-molecules of hydroxyl are changed.



Of the nine known di-oxyanthraquinones, only one, viz. alizarin, or that in which the hydroxyls are contained in the position 1, 2, is actually used as a colouring agent. Then again, three tri-oxyanthraquinones, $C_4H_5O_2(OH)_3$, are known. One of these is contained in madder-root, and has long been known as purpurin. The other tri-oxyanthroquinones can be artificially prepared. One termed anthrapurpurin is an important colouring matter, especially valuable to Turkey-red dyers, as giving a full or fiery red. The other, called flavo-purpurin, gives an orange dye with alumina mordants. All these various colouring matters can now be artificially produced, and by mixing these in varying proportions a far greater variety of tints can be obtained than was possible with madder alone, and thus the power of diversifying the colour at will is placed in the hands of the dyer and calico printer.

It is quite possible that in an analogous way a variety of shades of blue may be ultimately obtained from substituted indigos, and thus our catalogue of coal-tar colours may be still further increased.

To Englishmen it is a somewhat mortifying reflection, that whilst the raw materials from which all these coal-tar colours are made are produced in our country, the finished and valuable colours are nearly all manufactured in Germany. The crude and inexpensive materials are, therefore, exported by us abroad, to be converted into colours having many hundred times the value, and these expensive colours have again to be bought by English dyers and calico printers for use in our staple industries. The total annual value of manufactured coal-tar colours amounts to about three and a half millions; and as England herself, though furnishing all the raw material, makes only a small fraction of this quantity, but uses a large fraction, it is clear that she loses the profit on the manufacture. The causes of this fact, which we must acknowledge, viz. that Germany has driven England out of the field in this important branch of chemical manufacture, are probably various. In the first place, there is no doubt that much of the German success is due to the long-continued attention which their numerous Universities have paid to the cultivation of Organic Chemistry as a pure science. For this is carried out with a degree of completeness, and to an extent to which we in England are as yet strangers. Secondly, much again is to be attributed to the far more general recognition amongst German than amongst English men of business of the value, from a merely mercantile point of view, of high scientific training. In proof of this it may be mentioned, that each of two of the largest German colour-works employs no less a number than from twenty-five to thirty highly educated scientific chemists, at salaries varying from 250*l.* to 500*l.* or 600*l.* per annum. A third cause which doubtless exerts a great influence in this matter is the English law of patents. This, in the special case of colouring matters at least, offers no protection to English patentees against foreign infringement, for when these colours are once on the goods they cannot be identified. Foreign infringers can thus lower the price so that only the patentee, if skilful, can compete against them, and no English licencees of the patent can exist. This may to some extent account for the reluctance which English capitalists feel in embarking in the manufacture of artificial colouring matters. That England possesses both in the scientific and in the practical direction ability equal to the occasion none can doubt. But be that as it may, the whole honour of the discovery of artificial indigo belongs to Germany and to the distinguished chemist Professor Adolf Baeyer, whilst towards the solution of the difficult problem of its economic manufacture the first successful steps have been taken by Dr. Caro and the Baden Aniline and Soda Works of Mannheim.

[H. E. R.]

WEEKLY EVENING MEETING,

Friday, Feb. 18, 1881.

Thomas Boycott, Esq. M.D. F.L.S. Vice-President, in the Chair.

Sir John Lubbock, Bart. M.P. D.C.L. LL.D. F.R.S. *M.R.I.*
Pres. Linn. Soc.

Fruits and Seeds.

Our eloquent countryman, Mr. Ruskin, commences his work on Flowers by a somewhat severe criticism of his predecessors. He reproduces a page from a valuable but somewhat antiquated work, 'Curtis' Magazine,' which he alleges to be "characteristic of botanical books and botanical science, not to say all science," and complains bitterly that it is a string of names and technical terms. No doubt that unfortunate page does contain a list of synonyms, and long words. But in order to identify a plant you must have synonyms and technical terms, just as to learn a language you must have a dictionary. To complain of this would be to resemble the man who said that Johnson's Dictionary was dry and disjointed reading. But no one would attempt to judge the literature of a country by reading a dictionary. Neither can we estimate the interest of a science by reading technical descriptions. On the other hand, it is impossible to give a satisfactory description of an animal or plant except in strict technical language.

Let me reproduce a description which Mr. Ruskin has given of the Swallow, and which, indeed, he says in his lecture on that bird, is the only true description that could be given. His lecture was delivered before the University of Oxford, and is, I need hardly say, most interesting. Now, how does he describe a swallow? You can, he says, "only rightly describe the bird by the resemblances and images of what it seems to have changed from, then adding the fantastic and beautiful contrast of the unimaginable change. It is an owl that has been trained by the Graces. It is a bat that loves the morning light. It is the aërial reflection of a dolphin. It is the tender domestication of a trout." That is, no doubt, very poetical, but it would be absolutely useless as a scientific description, and, I must confess, would never have suggested, to me at least, the idea of a swallow.

But though technical terms are very necessary in science, I shall endeavour, as far as I can, to avoid them here. As, however, it will be impossible for me to do so altogether, I will do my best at the commencement to make them as clear as possible, and I must therefore ask those who have already looked into the subject, to

pardon me if, for a few moments, I go into very elementary facts. In order to understand the structure of the seed, we must commence with the flower, to which the seed owes its origin. Now if you take such a flower as, say, a Geranium, you will find that it consists of the following parts: Firstly, there is a whorl of green leaves, known as the sepals, and together forming the calyx; secondly, a whorl of colored leaves, or petals, generally forming the most conspicuous part of the flower, and called the corolla; thirdly, a whorl of organs more or less like pins, which are called stamens; and in the heads, or anthers, of which the pollen is produced. These anthers are in reality, as Goethe showed, modified leaves; in the so-called double flowers, as, for instance, in our garden roses, they are developed into colored leaves like those of the corolla, and monstrous flowers are not unfrequently met with, in which the stamens are green leaves, more or less resembling the ordinary leaves of the plant. Lastly, in the centre of the flower is the pistil, which also is theoretically to be considered as constituted of one or more leaves, each of which is folded on itself, and called a carpel. Sometimes there is only one carpel. Generally the carpels have so completely lost the appearance of leaves, that this explanation of their true nature requires a certain amount of faith. The base of the pistil is the ovary, composed, as I have just mentioned, of one or more carpels, in which the seeds are developed. I need hardly say that many so-called seeds are really fruit; that is to say, they are seeds with more or less complex envelopes.

We all know that seeds and fruits differ greatly in different species. Some are large, some small; some are sweet, some bitter; some are brightly colored, some are good to eat, some poisonous, some spherical, some winged, some covered with bristles, some with hairs, some are smooth, some very sticky; and we may be sure that there are good reasons for these differences.

In the case of flowers, much light has been thrown on their various interesting peculiarities by the researches of Sprengel, Darwin, Müller, and other naturalists. As regards seeds also, besides Gærtner's great work, Hildebrand, Krause, Steinbrinck, Kerner, Grant Allen, Wallace, Darwin, and others, have published valuable researches, especially with reference to the hairs and hooks with which so many seeds are provided, and the other means of dispersion they possess. Nobbe also has contributed an important work on seeds, principally from an agricultural point of view, but the subject as a whole offers a most promising field for investigation. It is rather with a view of suggesting this branch of science to you, than of attempting to supply the want myself, that I now propose to call your attention to it. In doing so I must, in the first place, express my acknowledgments to Mr. Baker, Mr. Carruthers, Mr. Hemsley, and specially to Mr. Thiselton Dyer and Sir Joseph Hooker, for their kind and most valuable assistance.

It is said that one of our best botanists once observed to another

that he never could understand what was the use of the teeth on the capsules of mosses. "Oh," replied his friend, "I see no difficulty in that, because if it were not for the teeth, how could we distinguish the species?" We may, however, no doubt, safely consider that the peculiarities of seeds have reference to the plant itself, and not to the convenience of botanists.

In the first place, then, during growth, seeds in many cases require protection. This is especially the case with those of an albuminous character. It is curious that so many of those which are luscious when ripe, as the Peach, Strawberry, Cherry, Apple, &c., are stringy, and almost inedible, till ripe. Moreover, in these cases, the fleshy portion is not the seed itself, but only the envelope, so that even if the sweet part is eaten the seed itself remains uninjured.

On the other hand, such seeds as the Hazel, Beech, Spanish Chestnut, and innumerable others, are protected by a thick, impervious shell, which is especially developed in many Proteaceæ, the Brazil-nut, the so-called Monkey-pot, the Cocoa-nut, and other palms.

In other cases the envelopes protect the seeds, not only by their thickness and toughness, but also by their bitter taste, as, for instance, in the Walnut. The genus *Mucuna*, one of the Leguminosæ, is remarkable in having the pods covered with stinging hairs.

In many cases ripening of the seed is accompanied by important movements of the neighbouring organs. In some, for instance, the calyx, which is closed when the flower is in bud, opens when the flower expands, and then, after the petals have fallen, closes again until the seeds are ripe, when it opens for the second time. This is the case with the common Herb Robert (*Geranium robertianum*). In *Atractylis cancellata*, a South European plant, allied to the thistles, the outer envelopes form an exquisite little cage. Another case, perhaps, is that of *Nigella*, the "Devil-in-a-bush," or, as it is sometimes more prettily called, "Love-in-a-mist," of old English gardens.

Again, the protection of the seed is in many cases attained by curious movements of the plant itself. In fact, plants move much more than is generally supposed. So far from being motionless, they may almost be said to be in perpetual movement, though the changes of position are generally so slow that they do not attract attention. This is not, however, always the case. We are all familiar with the Sensitive Plant, which droops its leaves when touched. Another species (*Averrhoa bilimbi*) has leaves like those of an Acacia, and all day the leaflets go slowly up and down. *Desmodium gyrans*, a sort of pea living in India, has trifoliate leaves, the lateral leaflets being small and narrow; and these leaflets, as was first observed by Lady Monson, are perpetually moving round and round, whence the specific name *gyrans*. In these two cases the object of the movement is quite unknown to us. In *Dionæa*, on the other hand, the leaves form a regular fly-trap. Directly an insect alights on them they shut up with a snap.

In a great many cases leaves are said to sleep; that is to say, at the approach of night they change their position, and sometimes fold themselves up, thus presenting a smaller surface for radiation, and being in consequence less exposed to cold. Mr. Darwin has proved experimentally that leaves which were prevented from moving suffered more from cold than those which were allowed to assume their natural position. He has also observed with reference to one plant, *Maranta arundinacea*, the Arrowroot, a West Indian species allied to Canna, that if the plant has had a severe shock it cannot get to sleep for the next two or three nights.

The sleep of flowers is also probably a case of the same kind, though, as I have elsewhere attempted to show, it has now, I believe, special reference to the visits of insects; those flowers which are fertilised by bees, butterflies, and other day insects, sleep by night, if at all; while those which are dependent on moths rouse themselves towards evening, as already mentioned, and sleep by day. These motions, indeed, have but an indirect reference to our present subject. On the other hand, in the Dandelion (*Leontodon*), the flower-stalk is upright while the flower is expanded, a period which lasts for three or four days; it then lowers itself and lies close to the ground for about twelve days, while the fruits are ripening, and then rises again when they are mature. In the Cyclamen the stalk curls itself up into a beautiful spiral after the flower has faded.

The flower of the little Linaria of our walls (*L. cymbalaria*) pushes out into the light and sunshine, but as soon as it is fertilised it turns round and endeavours to find some hole or cranny in which it may remain safely ensconced until the seed is ripe.

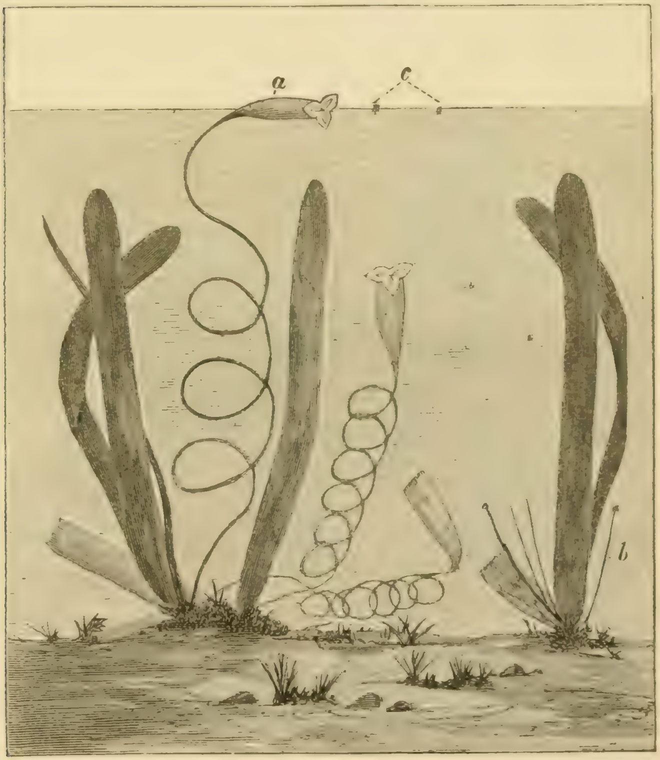
In some water plants the flower expands at the surface, but after it is faded retreats again to the bottom. This is the case, for instance, with the Water Lilies, some species of Potamogeton, *Trapa natans*, &c. In *Valisneria*, again, the female flowers (Fig. 1 *a*) are borne on long stalks, which reach to the surface of the water, on which the flowers float. The male flowers (Fig. 1 *b*), on the contrary, have short, straight stalks, from which, when mature, the pollen (Fig. 1 *c*) detaches itself, rises to the surface, and, floating freely on it, is wafted about, so that it comes in contact with the female flowers. After fertilisation, however, the long stalk coils up spirally, and thus carries the ovary down to the bottom, where the seeds can ripen in greater safety.

The next points to which I will direct your attention are the means of dispersion possessed by many seeds. Farmers have found by experience that it is not desirable to grow the same crop in the same field year after year, because the soil becomes more or less exhausted. In this respect, therefore, the powers of dispersion possessed by many seeds are a great advantage to the species. Moreover, they are also advantageous in giving the seed a chance of germinating in new localities suitable to the requirements of the species. Thus a common European species, *Xanthium spinosum*, has rapidly spread

over the whole of South Africa, the seeds being carried in the wool of sheep. From various considerations, however, it seems probable that in most cases the provision does not contemplate a dispersion for more than a short distance.

I have already referred to the case of the Common Dandelion. Here the flower-stalk stands more or less upright while the flower is expanded, a period which generally lasts for three or four days. It

Fig. 1.



Valisneria spiralis.

a, female flower; *b*, male flower; *c*, floating pollen.

then lowers itself, and lies more or less horizontally and concealed during the time the seeds are maturing, which in our summers occupies about twelve days. It then again rises, and, becoming almost erect, facilitates the dispersion of the seeds, or, speaking botanically, the fruits, by the wind. Some plants, as we shall see, even sow their seeds in the ground, but these cases will be referred to later on.

In other cases the plant throws its own seeds to some little distance. This is the case with the common *Cardamine hirsuta*, a little plant—I do not like to call it a weed—six or eight inches high, which comes up of itself abundantly on any vacant spot in our kitchen-gardens or shrubberies, and which much resembles that represented in Fig. 17, but without the subterranean pods *b*. The seeds are contained in a pod which consists of three parts, a central membrane, and two lateral walls. When the pod is ripe the walls are in a state of tension. The seeds are loosely attached to the central piece by short stalks. Now, when the proper moment has arrived, the outer walls are kept in place

FIG. 2.

Viola hirta.

a, young bud; *b*, ripe seed capsule.

by a delicate membrane, only just strong enough to resist the tension. The least touch, for instance a puff of wind blowing the plant against a neighbour, detaches the outer wall, which suddenly rolls itself up, generally with such force as to fly from the plant, thus jerking the seeds to a distance of several feet.

In the common violets, beside the colored flowers, there are others

in which the corolla is either absent or imperfectly developed. The stamens also are small, but contain pollen, though less than in the colored flowers. In the autumn large numbers of these curious flowers are produced. When very young they resemble an ordinary flower-bud (Figs. 2 and 3 *a*), the central part of the flower being entirely covered by the sepals, and the whole having a triangular form. When older (Figs. 2 and 3 *b*) they look at first sight like an ordinary seed-capsule, so that the bud seems to pass into the capsule without the flower-stage. The pansy violets do not possess these interesting flowers. In the Sweet Violet (*V. odorata* and *V. hirta*, Fig. 2) they may easily be found, by searching among the leaves, nestling close to the ground. It is often said, for instance by Vaucher, that the plants

Fig. 3.

Viola canina.

a, bud; *b*, bud more advanced; *c*, capsule open, some of the seeds are already thrown.

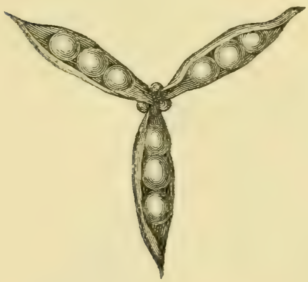
actually force these capsules into the ground, and thus sow their own seeds. I have not, however, found this to be the case, though as the stalk elongates, and the point of the capsule turns downwards, if the earth be loose and uneven, it will no doubt sometimes so happen. When the seeds are fully ripe, the capsule opens by three valves and allows them to escape.

In the Dog Violet (*V. canina*, Fig. 3) the case is very different. The capsules are less fleshy, and though pendent when young, at

maturity they erect themselves (Fig. 3 *c*), stand up boldly above the rest of the plant, and open by the three equal valves (Fig. 4) resembling an inverted tripod. Each valve contains a row of three, four, or five brown, smooth, pear-shaped seeds, slightly flattened at the upper, wider end. Now the two walls of each valve, as they become drier, contract, and thus approach one another, thus tending to squeeze out the seeds. These resist some time, but at length the attachment of the seed to its base gives way, and it is ejected several feet, this being no doubt much facilitated by its form and smoothness. I have known even a gathered specimen throw a seed nearly 10 feet. Fig. 5 represents a capsule after the seeds have been ejected.

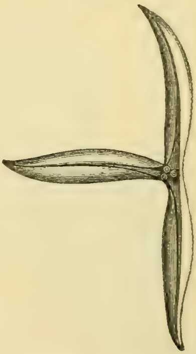
Now we naturally ask ourselves what is the reason for this difference between the species of violets; why do *V. odorata* and *V. hirta* conceal their capsules among the moss and leaves on the ground, while *V. canina* and others raise theirs boldly above their heads, and throw

Fig. 4.



Viola canina—seed-vessel with seed.

Fig. 5.



Viola canina—seed-vessel after ejecting the seed.

the seeds to seek their fortune in the world? If this arrangement be best for *V. canina*, why has not *V. odorata* also adopted it? The reason is, I believe, to be found in the different mode of growth of these two species.

V. canina is a plant with an elongated stalk, and it is easy therefore for the capsule to raise itself above the grass and other low herbage among which violets grow. *V. odorata* and *V. hirta*, on the contrary, have, in ordinary parlance, no stalk, and the leaves are radical, i. e. rising from the root. This is at least the case in appearance, for, botanically speaking, they rise at the end of a short stalk. Now, under these circumstances, if the Sweet Violet attempted to shoot its seeds, the capsules not being sufficiently elevated, the seeds would merely strike against some neighbouring leaf, and immediately fall to the ground. Hence, I think, we see that the arrangement of the capsule in each species is that most suitable to the general habit of the plant.

In the true geraniums again, as for instance in the Herb Robert (Fig. 6), after the flower has faded, the central axis gradually elongates (Fig. 6 *c d*). The seeds, five in number, are situated at the base of the column, each being enclosed in a capsule, which terminates upwards in a rod-like portion, which at first forms part of the central axis, but gradually detaches itself. When the seeds are ripe the ovary

Fig. 6.

The Herb Robert (*Geranium robertianum*).

a, bud; *b*, flower; *c*, flower after the petals have fallen; *d*, flower with seeds nearly ripe; *e*, flower with ripe seeds; *f*, flower after throwing seeds.

raises itself into an upright position (Fig. 6 *e*); the outer layers of the rod-like termination of the seed-capsule come to be in a state of great tension, and eventually detach the rod with a jerk, and thus throw the seed some little distance. Fig. 6 *f* represents the central rod after the seeds have been thrown. In some species, as for instance

in *Geranium dissectum*, Fig. 7, the capsule-rod remains attached to the central column and the seed only is ejected.

It will, however, be remembered that the capsule is, as already observed, a leaf folded on itself, with the edges inwards, and in fact in the Geranium the seed-chamber opens on its inner side. You will, therefore, naturally observe to me that when the carpel bursts outwards, the only effect would be that the seed would be forced against the outer wall of the carpel, and that it would not be ejected, because the opening is not on the outer but on the inner side. Your remark is perfectly just, but the difficulty has been foreseen by our Geraniums,

FIG. 7.

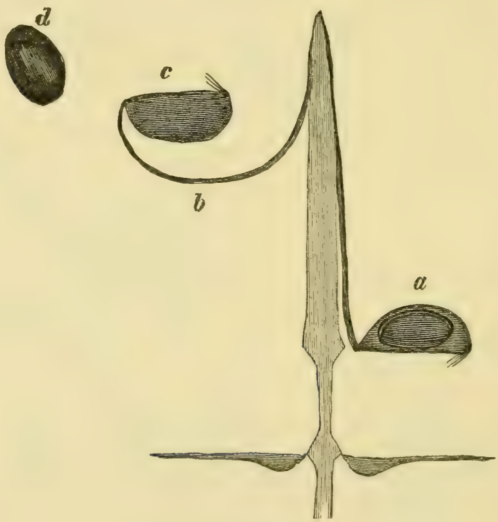


Diagram.

Geranium dissectum.

a, just before throwing seed; *b*, just after throwing seed; *c*, the capsule still attached to the rod; *d*, the seed.

and is overcome by them in different ways. In some species, as for instance in *Geranium dissectum*, a short time before the dehiscence, the seed-chamber places itself at right angles to the pillar (Fig. 7 *a*). The edges then separate, but they are provided with a fringe of hairs, just strong enough to retain the seed in its position, yet sufficiently elastic to allow it to escape when the carpels burst away, remaining attached, however, to the central pillar by their upper ends (Fig. 7 *c*).

In the common Herb Robert (Fig. 8), and some other species, the arrangement is somewhat different. In the first place the whole

carpel springs away (Fig. 8 *b* and *c*). The seed-chamber (Fig. 8 *c*) detaches itself from the rod of the carpel (Fig. 8 *b*), and when the seed is flung away remains attached to it. Under these circumstances it is unnecessary for the chamber to raise itself from the central pillar, to which accordingly it remains close until the moment of disruption (Fig. 6 *e*). The seed-chamber is moreover held in place by a short tongue which projects a little way over its base; while, on the other hand, the lower end of the rod passes for a short distance between the seed-capsule and the central pillar. The seed-capsule has also near its apex a curious tuft of silky hair (Fig. 8 *c*), the use of which I will not here stop to discuss. As the result of all this complex mechanism

Fig. 8.

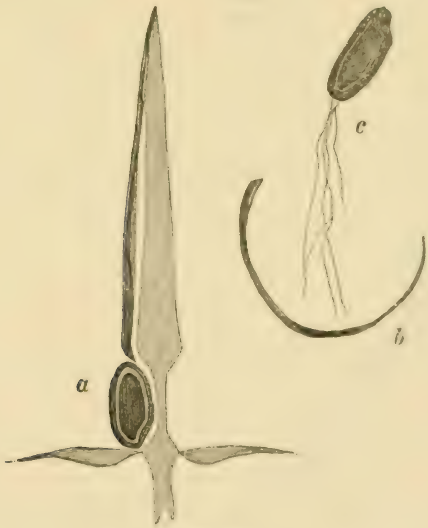


Diagram.
Geranium robertianum.
a, just before throwing the seed; *b*, the rod; *c*, the seed enclosed in the capsule.

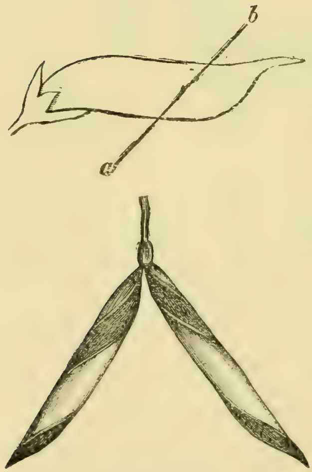
the seeds, when ripe, are flung to a distance which is surprising when we consider how small the spring is. In their natural habitat it is almost impossible to find the seeds when once thrown. I therefore brought some into the house and placed them on my billiard-table. They were thrown from one end completely over the other, in some cases more than twenty feet.

Some species of vetch, again, and the common broom, throw their seeds, owing to the elasticity of the pods, which, when ripe, open suddenly with a jerk. Each valve of the pod contains a layer of woody cells, which, however, do not pass straight up the pod, but are

more or less inclined to its axis (Fig. 9). Consequently, when the pod bursts it does not, as in the case of Cardamine, roll up like a watch-spring, but twists itself more or less like a corkscrew.

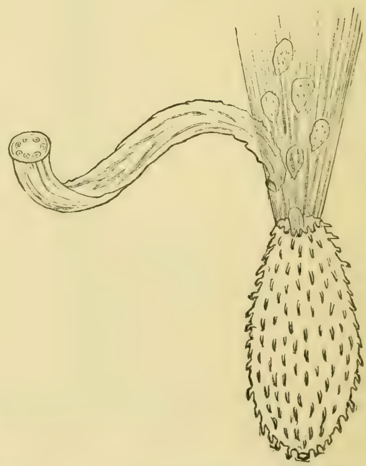
I have mentioned these species because they are some of our commonest wild flowers, so that during the summer and autumn we may, in almost any walk, observe for ourselves this innocent artillery. There are, however, many other more or less similar cases. Thus the Squirting Cucumber (*Momordica elaterium*), a common plant in the south of Europe, and one grown in some places for medicinal purposes, effects the same object by a totally different mechanism. The fruit is a small cucumber (Fig. 10), and when ripe it becomes so gorged with fluid that it is in a state of great tension. In this condition a very

Fig. 9.



Vicia Sepium.
The line *a b* shows the direction of the woody fibres.

Fig. 10.



The Squirting Cucumber (*Momordica elaterium*).

slight touch is sufficient to detach it from the stalk, when the pressure of the walls ejects the contents, throwing the seed some distance. In this case of course the contents are ejected at the end by which the cucumber is attached to the stalk. If any one touches one of these ripe fruits, they are often thrown with such force as to strike him in the face. In this the action is said to be due to endosmosis.

In *Cyclanthera*, a plant allied to the cucumber, the fruit is unsymmetrical, one side being round and hairy, the other nearly flat and smooth. The true apex of the fruit, which bears the remains of the flower, is also somewhat eccentric, and, when the seeds are ripe, if it is touched even lightly, the fruit explodes and the seeds are thrown to some distance. The mechanism by which this is effected has been

described by Hildebrand. The interior of the fruit is occupied by loose cellular structure. The central column, or placenta, to which the seeds are attached, lies loosely in this tissue. Through the solution of its earlier attachments, when the fruit is ripe, the column adheres only at the apical end, under the withered remains of the flower, and at the swollen side. When the fruit bursts the placenta unrolls, and thus hurls the seeds to some distance, being even itself sometimes also torn away from its attachment.

Other cases of projected seeds are afforded by *Hura* one of the *Euphorbiæ*, *Collomia*, *Oxalis*, some species allied to Acanthus, and by *Arceuthobium*, a plant allied to the mistletoe, and parasitic on Junipers, which ejects its seeds to a distance of several feet, throwing them thus from one tree to another.

Even those species which do not eject their seeds often have them so placed with reference to the capsule that they only leave it if swung or jerked by a high wind. In the case of trees, even seeds with no special adaptation for dispersion must in this manner be often carried to no little distance; and to a certain, though less extent, this must hold good even with herbaceous plants. It throws light on the, at first sight, curious fact that in so many plants with small, heavy seeds, the capsules open not at the bottom, as one might perhaps have been disposed to expect, but at the top. A good illustration is afforded by the well-known case of the Common Poppy (Fig. 11), in which the upper part of the capsule presents a series of little doors (Fig. 11 *a*), through which, when the plant is swung by the wind, the seeds come out one by one. The little doors are protected from rain by overhanging eaves, and are even said to shut of themselves in wet weather. The genus *Campanula* is also interesting from this point of view, because some species have the capsules pendent, some upright, and those which are upright open at the top, while those which are pendent do so at the base.

FIG. 11.

Seed-head of Poppy (*Papaver*).

In other cases the dispersion is mainly the work of the seed itself. In some of the lower plants, as, for instance, in many seaweeds, and in some allied fresh-water plants, such as *Vaucheria*, the spores * are covered by vibratile cilia, and actually swim about in the water, like infusoria, till they have found a suitable spot on which to grow. Nay, so much do the spores of some seaweeds resemble animals, that they are

* I need hardly observe that, botanically, these are not true seeds, but rather motile buds.

provided with a red "eye-spot" as it has been called, which, at any rate, seems so far to deserve the name that it appears to be sensitive to light. This mode of progression is, however, only suitable to water-plants. One group of small, low-organised plants, *Marchantia*, develop among the spores a number of cells with spirally thickened walls, which, by their contractility, are supposed to disseminate the spores. In the common Horse Tails (*Equisetum*), again, the spores are provided with curious filaments, terminating in expansions, and known as "elaters." They move with great vigour, and probably serve the same purpose.

In much more numerous cases, seeds are carried by the wind. For this of course it is desirable that they should be light. Sometimes this object is attained by the character of the tissues themselves, sometimes by the presence of empty spaces. Thus, in *Valerianella auricula*, the fruit contains three cells, each of which would naturally be expected to contain a seed. One seed only, however, is developed, but, as may be seen from the figure given in Mr. Bentham's excellent 'Handbook of the British Flora,' the two cells which contain no seed actually become larger than the one which alone might, at first sight, seem to be normally developed. We may be sure from this that they must be of some use, and, from their lightness, they probably enable the wind to carry the seed to a greater distance than would otherwise be the case.

In other instances the plants themselves, or parts of them, are rolled along the ground by the wind. An example of this is afforded, for instance, by a kind of grass (*Spinifex squarrosus*), in which the mass of inflorescence, forming a large round head, is thus driven for miles over the dry sands of Australia until it comes to a damp place, when it expands and soon strikes root.

In *Pumilio argyrolepis*, an Australian Composite, the pappus, or portion corresponding to the feathered crown of the Dandelion seed, consists, as described by Mr. Darwin,* of nine scales (or sepals), expanded like a flower; the lower part of the fruit, which encloses the true seed, is bent nearly at a right angle, and in form closely resembles a human foot. The upper side or instep is smooth, but the toe and sole, which are about $\frac{1}{25}$ inch in length, are covered with from 30 to 40 little bladders, each formed of a thin skin and containing a small lump of gum. When the fruits are moistened these bladders burst and the gum exudes. As long then as the fruits remain dry, they are easily blown about by the wind; but as soon as they alight on a damp spot, the gum exudes and glues them to the ground. If a pinch of these seeds be dropped on a piece of paper, the greater number fall upright like shuttlecocks, but even if they alight on one side the tendency of the gum is to pull them upright, so that they look as if each had been placed upright and carefully gummed. It is not clear whether this position is of importance to the germination of the seed.

* 'Gardeners' Chronicle,' 5th Jan. 1881, p. 4.

So, again, the *Anastatica hierochuntica*, or "Rose of Jericho," a small annual with rounded pods, which frequents sandy places in Egypt, Syria, and Arabia, when dry, curls itself up into a ball or round cushion, and is thus driven about by the wind until it finds a damp place, when it uncurls, the pods open, and sow the seeds.

Fig. 12.

a, maple; *b*, sycamore; *c*, lime; *d*, hornbeam; *e*, elm; *f*, birch; *g*, pine; *h*, fir; *i*, ash.

These cases, however, in which seeds are rolled by the wind along the ground are comparatively rare. There are many more in which seeds are wafted through the air. If you examine the fruit of a Sycamore you will find that it is provided with a wing-like expansion,

in consequence of which, if there is any wind when it falls, it is, though rather heavy, blown to some distance from the parent tree. Several cases are shown in Fig. 12; for instance, the Maple *a*, Sycamore *b*, Hornbeam *d*, Elm *e*, Birch *f*, Pine *g*, Fir *h*, and Ash *i*, while in the Lime, *c*, the whole bunch of fruits drops together, and the "bract," as it is called, or leaf of the flower-stalk, serves the same purpose.

In a great many other plants the same result is obtained by flattened and expanded edges. A beautiful example is afforded by the genus *Thysanocarpus*, a North American crucifer; *T. laciniatus* has a distinctly winged pod; in *T. curvipes* the wings are considerably larger; lastly, in *T. elegans* and *T. radians* the pods are still further developed in the same direction, *T. radians* having the wing very broad, while in *T. elegans* it has become thinner and thinner in places, until at length it shows a series of perforations. Among our common wild plants we find winged fruits in the Dock (*Rumex*) and in the Common Parsnip (*Pastinaca*). But though in these cases the object to be obtained—namely, the dispersion of the seed—is effected in a similar manner, there are differences which might not at first be suspected. Thus in some cases, as, for instance, the Pine, it is the seed itself which is winged; in *Thlaspi arvense* it is the pod; in *Entada*, a leguminous plant, the pod breaks up into segments, each of which is winged; in *Nissolia* the extremity of the pod is expanded into a flattened wing; lastly, in the Lime, as already mentioned, the fruits drop off in a bunch, and the leaf at the base of the common flower-stalk, or "bract," as it is called, forms the wing.

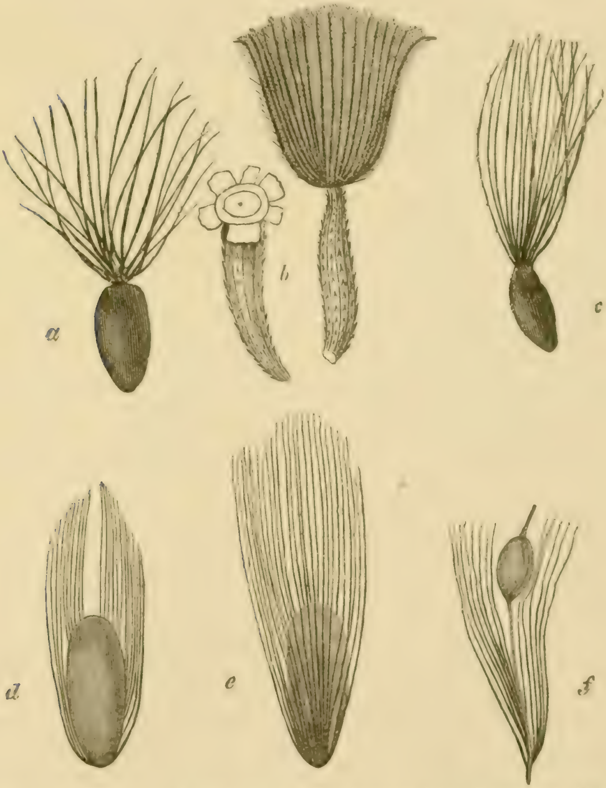
In *Gouania retinaria* of Rodriguez the same object is effected in another manner; the cellular tissue of the fruit crumbles and breaks away, leaving only the vascular tissue, which thus forms a net enclosing the seed.

Another mode, which is frequently adopted, is the development of long hairs. Sometimes, as in Clematis, Anemone, Dryas, these hairs take the form of a long feathery awn. In others the hairs form a tuft or crown, which botanists term a pappus. Of this the Dandelion and John Go-to-bed-at-noon, so called from its habit of shutting its flowers about mid-day, are well-known examples. Tufts of hairs, which are themselves sometimes feathered, are developed in a great many Composites, though some, as, for instance, the Daisy and Lapsana, are without them; in some very interesting species, of which the common *Thrincia hirta* of our lawns and meadows is one, there are two kinds of fruits, as shown in Fig. 13 *b*, one with a pappus and one without. The former are adapted to seek "fresh woods and pastures new," while the latter stay and perpetuate the race at home.

A more or less similar pappus is found among various English plants—in the Epilobium (Fig. 13 *a*), Thrincia (Fig. 13 *b*), Tamarix (Fig. 13 *c*), Willow (Fig. 13 *d*), Cotton Grass (Fig. 13 *e*), and Bulrush (Fig. 13 *f*); while in exotic species there are many other cases—as, for instance, the beautiful Oleander. As in the wings, so

also in that of the pappus, it is by no means always the same part of the plant which develops into the crown of hairs. Thus in the Valerians and Composites it is the calyx; in the Bulrush, the perianth; in Epilobium, the crown of the seed; in the Cotton-Grass it is supposed to represent the perianth; while in some, as, for instance, in the Cotton plant, the whole outer surface of the seed is

FIG. 13.



a, willow herb (*Epilobium*); *b*, two forms of seed of *Thrincia hirta*; *c*, *Tamarix*; *d*, willow (*Salix*); *e*, cotton-grass (*Eriophorum*); *f*, bulrush (*Typha*).

clothed with long hairs. Sometimes, on the contrary, the hairs are very much reduced in number, as, for instance, in some species of *Æschynanthus*, where there are only three, one on one side and two on the other. In this case, moreover, the hairs are very flexible, and wrap round the wool of any animal with which they may come in contact, so that they form a double means of dispersion.

In other cases seeds are wafted by water. Of this the Cocoa-nut is one of the most striking examples. The seeds retain their vitality for a considerable time, and the loose texture of the husk protects them and makes them float. Every one knows that the Cocoa-nut is one of the first plants to make its appearance on coral islands, and it is, I believe, the only palm which is common to both hemispheres.

The seeds of the Common Duckweeds (*Lemna*) sink to the bottom of the water in autumn, and remain there throughout the winter; but in the spring they rise up to the surface again and begin to grow.

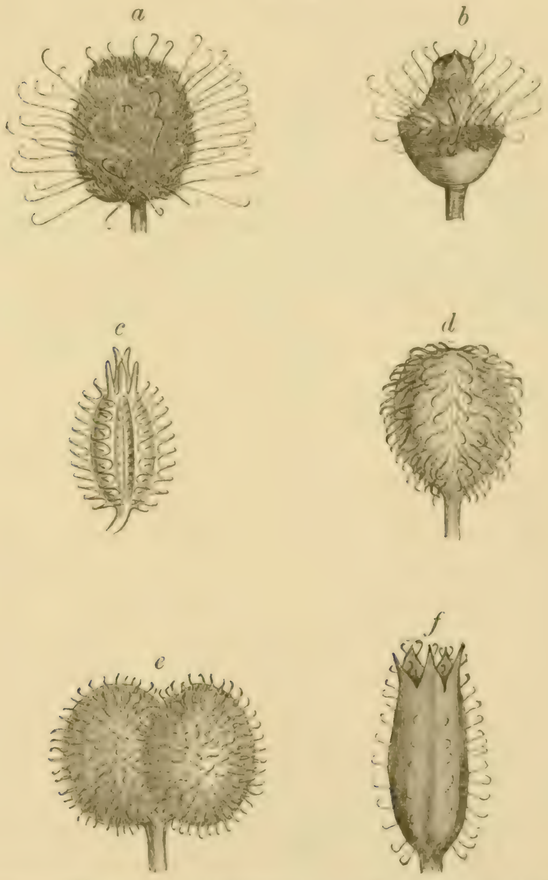
In a very large number of cases the diffusion of seeds is effected by animals. To this class belong the fruits and berries. In them an outer fleshy portion becomes pulpy, and generally sweet, enclosing the seeds. It is remarkable that such fruits, in order, doubtless, to attract animals, are, like flowers, brightly colored—as, for instance, the Cherry, Currant, Apple, Peach, Plum, Strawberry, Raspberry, and many others. This color, moreover, is not present in the unripe fruit, but is rapidly developed at maturity. In such cases the actual seed is generally protected by a dense, sometimes almost stony, covering, so that it escapes digestion, while its germination is perhaps hastened by the heat of the animal's body. It may be said that the skin of apple and pear pips is comparatively soft; but then they are embedded in a stringy core, which is seldom eaten.

These colored fruits form a considerable part of the food of monkeys in the tropical regions of the earth, and we can, I think, hardly doubt that these animals are guided by the colors, just as we are, in selecting the ripe fruit. This has a curious bearing on an interesting question as to the power of distinguishing color possessed by our ancestors in bygone times. Geiger, relying on the well-known fact that the ancient languages are poor in words for color, and that in the oldest books—as, for instance, in the Vedas, the Zendavesta, the Old Testament, and the writings of Homer and Hesiod—though, of course, the heavens are referred to over and over again, its blue color is never dwelt on, has argued that the ancients were very deficient in the power of distinguishing colors, and especially blue. In our own country Mr. Gladstone has lent the weight of his great authority to the same conclusion. For my part I cannot accept this view. There are, it seems to me, very strong reasons against it, into which I cannot, of course, now enter; and though I should rely mainly on other considerations, the colors of fruits are not, I think, without significance. If monkeys and apes could distinguish them, surely we may infer that even the most savage of men could do so too. Zeuxis would never have deceived the birds if he had not had a fair perception of color.

In these instances of colored fruits, the fleshy edible part more or less surrounds the true seeds; in others the actual seeds themselves become edible. In the former the edible part serves as a temptation to animals; in the latter it is stored up for the use of the plant itself. When, therefore, the seeds themselves are edible they are generally

protected by more or less hard or bitter envelopes, for instance the Horse Chestnut, Beech, Spanish Chestnut, Walnut, &c. That these seeds are used as food by squirrels and other animals is, however, by no means necessarily an evil to the plant, for the result is that they are often carried some distance and then dropped, or stored up and

Fig. 14.



a, burdock (*Lappa*); *b*, agrimony (*Agrimonia*); *c*, bur parsley (*Caucalis*); *d*, enchanter's nightshade (*Circaa*); *e*, cleavers (*Galium*); *f*, forget-me-not (*Myosotis*).

forgotten, so that in this way they get carried away from the parent tree.

In another class of instances animals, unconsciously or unwillingly, serve in the dispersion of seeds. These cases may be

divided into two classes, those in which the fruits are provided with hooks, and those in which they are sticky. To the first class belong, among our common English plants, the Burdock (*Lappa*, Fig. 14 *a*), *Agrimony* (*Agrimonia*, Fig. 14 *b*); the Bur Parsley (*Caucalis*, Fig. 14 *c*); Enchanter's Nightshade (*Circæa*, Fig. 14 *d*); Goose Grass or Cleavers (*Galium*, Fig. 14 *e*), and some of the Forget-me-Nots (*Myosotis*, Fig. 14 *f*). The hooks, moreover, are so arranged as to promote the removal of the fruits. In all these species the hooks, though beautifully formed, are small; but in some foreign species they become truly formidable. Two of the most remarkable are represented below,—*Martynia proboscidea* (Fig. 15 *b*) and *Harpagophyton procumbens* (Fig. 15 *a*). Martynia is a plant of Louisiana, and if its fruits once get hold of an animal it is most difficult to remove them. Harpagophyton is a South African genus. The fruits are most formidable, and are said sometimes even to kill lions. They roll about over the dry plains, and if they attach themselves to the skin, the wretched animal tries to tear them out, and sometimes getting them into its mouth perishes miserably.

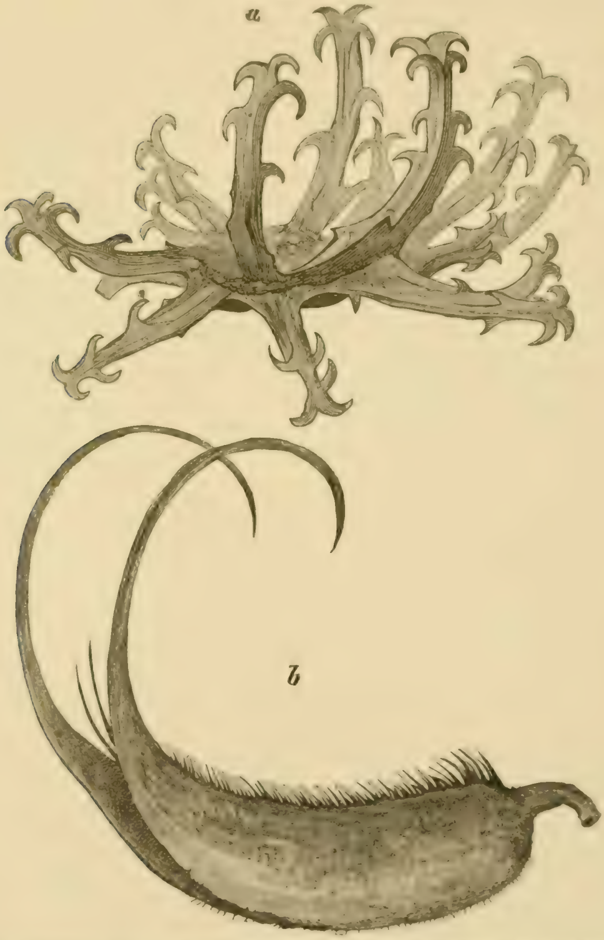
The cases in which the diffusion of fruits and seeds is effected by their being sticky are less numerous, and we have no well-marked instance among our native plants. The common Plumbago of South Europe is a case which many of you no doubt have observed. Other genera with the same mode of dispersion are *Pittosporum*, *Pisonia*, *Boerhavia*, *Siegesbeckia*, *Grindelia*, *Drymaria*, &c. There are comparatively few cases in which the same plant uses more than one of these modes of promoting the dispersion of its seeds, still there are some such instances. Thus in the Common Burdock the seeds have a pappus, while the whole flower-head is provided with hooks which readily attach themselves to any passing animal. *Asterothrix*, as Hildebrand has pointed out, has three provisions for dispersion: a hollow appendage, a pappus, and a rough surface.

But perhaps it will be said that I have picked out special cases; that others could have been selected, which would not bear out, or perhaps would even negative, the inferences which have been indicated; that I have put the cart before the horse; that the Ash fruit has not a wing in order that it may be carried by the wind, or the Burdock hooks that the heads may be transported by animals, but that happening to have wings and hooks these seeds are thus transported. Now doubtless there are many points connected with seeds which are still unexplained; in fact it is because this is so that I was anxious to direct attention to the subject. Still I believe the general explanations which have been given by botanists will stand any test.

Let us take for instance seeds formed on the same type as that of the Ash—heavy fruits, with a long wing, known to botanists as a Samara. Now such a fruit would be of little use to low herbs, which, however, are so numerous. If the wing was accidental, if it were not developed to serve as a means of dispersion, it would be as likely to occur on low plants and shrubs as on trees. Let us then consider

on what kind of plants these fruits are found. They occur on the Ash, Maple, Sycamore, Hornbeam, Pines, Firs, and Elm; while the Lime, as we have seen, has also a leaf attached to the fruits, which

Fig. 15.



a, Harpagophyton procumbens (natural size); *b, Martynia proboscidea* (natural size).

answers the same purpose. Seeds of this character therefore occur on a large proportion of our forest trees, and on them alone. But more than this: I have taken one or two of the most accessible works

in which seeds are figured, for instance Gærtner's 'De Fructibus et Seminibus,' Le Maout and Decaisne (Hooker's translation) 'Descriptive and Analytical Botany,' and Baillon's 'Histoire des Plantes.' I find thirty genera, belonging to twenty-one different natural orders, figured as having seeds or fruits of this form. They are all trees or climbing shrubs, not one being a low herb.

Let us take another case, that of the plants in which the dispersion of the seed is effected by means of hooks. Now, if the presence of these hooks were, so to say, accidental, and the dispersion merely a result, we should naturally expect to find some species with hooks in all classes of plants. They would occur, for instance, among trees and on water-plants. On the other hand, if they are developed that they might adhere to the skin of quadrupeds, then, having reference to the habits and size of our British mammals, it would be no advantage for a tree or for a water-plant to bear hooked seeds. Now, what are the facts? There are about thirty English species in which the dispersion of the seeds is effected by means of hooks, but not one of these is aquatic, nor is one of them more than four feet high. Nay, I might carry the thing farther. We have a number of minute plants, which lie below the level at which seeds would be likely to be entangled in fur. Now none of these, again, have hooked seeds or fruits. It would also seem, as Hildebrand has suggested, that in point of time, also, the appearance of the families of plants in which the fruits or seeds are provided with hooks coincided with that of the land mammalia.

Again, let us look at it from another point of view. Let us take our common forest trees, shrubs, and tall climbing plants; not, of course, a natural or botanical group, for they belong to a number of different orders, but a group characterised by attaining to a height of say over eight feet. We will in some cases only count genera; that is to say, we will count all the willows, for instance, as one. These trees and shrubs are plants with which you are all familiar, and are about thirty-three in number. Now, of these thirty-three no less than eighteen have edible fruits or seeds, such as the Plum, Apple, Arbutus, Holly, Hazel, Beech, and Rose. Three have seeds which are provided with feathery hairs; and all the rest, namely, the Lime, Maple, Ash, Sycamore, Elm, Hop, Birch, Hornbeam, Pine, and Fir are provided with a wing. Moreover, as will be seen by the table on the following page, the lower trees and shrubs, such as the Cornel, Guelder Rose, Rose, Thorn, Privet, Elder, Yew, and Holly have generally edible berries, much eaten by birds. The winged seeds or fruits characterise the great forest trees.

Or let us take one natural order. That of the Roses is particularly interesting. In the genus *Geum* the fruit is provided with hooks; in *Dryas* it terminates in a long feathered awn, like that of Clematis. On the other hand, several genera have edible fruits; but it is curious that the part of a plant which becomes fleshy, and thus tempting to animals, differs considerably in the different genera. In the Black-

Trees, Shrubs, and Climbing Shrubs Native or Naturalised in Britain.

	Seed or Fruit.			
	Edible.	Hairy.	Winged.	Hooked.
Clematis vitalba	..	×		
Berberis vulgaris	×			
Lime (*Tilia Europæa*)	..	..	×	
Maple (*Acer*)	..	..	×	
Spindle Tree (*Euonymus*)	×			
Buckthorn (*Rhamnus*)	×			
Sloe (*Prunus*)	×			
Rose (*Rosa*)	×			
Apple (*Pyrus*)	×			
Hawthorn (*Cratægus*)	×			
Medlar (*Mespilus*)	×			
Ivy (*Hedera*)	×			
Cornel (*Cornus*)	×			
Elder (*Sambucus*)	×			
Guelder Rose (*Viburnum*)	×			
Honeysuckle (*Lonicera*)	×			
Arbutus (*Arbutus*)	×			
Holly (*Ilex*)	×			
Ash (*Fraxinus*)	..	..	×	
Privet (*Ligustrum*)	×			
Elm (*Ulmus*)	..	..	×	
Hop (*Humulus*)	..	..	×	
Alder (*Alnus*)				
Birch (*Betula*)	..	..	×	
Hornbeam (*Carpinus*)	..	..	×	
Nut (*Corylus*)	×			
Beech (*Fagus*)	×			
Oak (*Quercus*)	×			
Willow (*Salix*)	..	×		
Poplar (*Populus*)	..	×		
Pine (*Pinus*)	..	..	×	
Fir (*Abies*)	..	..	×	
Yew (*Taxus*)	×			

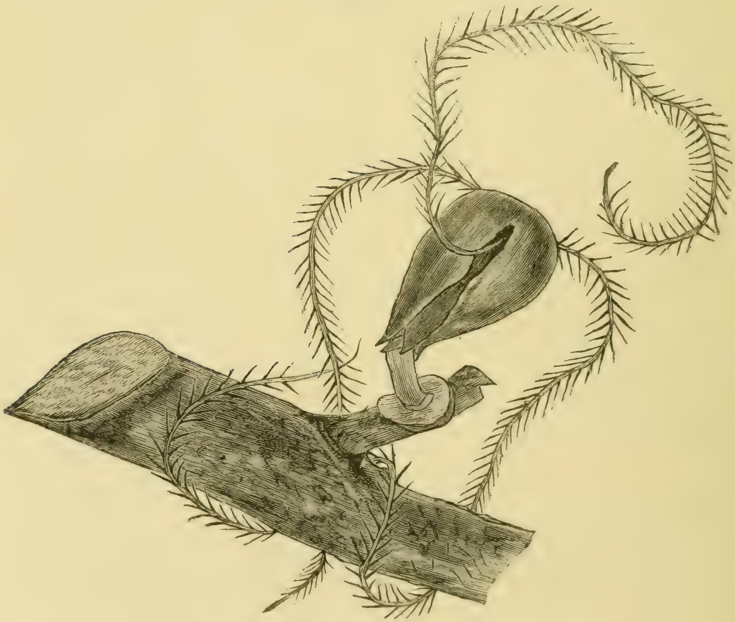
berry, for instance, and in the Raspberry, the carpels constitute the edible portion. When we eat a Raspberry we strip them off and leave the receptacle behind; while in the Strawberry the receptacle constitutes the edible portion; the carpels are small, hard, and closely surround the seeds. In these genera the sepals are situated below the fruit. In the Rose, on the contrary, it is the peduncle that is swollen and inverted, so as to form a hollow cup, in the interior of which the carpels are situated. Here you will remember that the sepals are situated above, not below, the fruit. Again, in the Pear and Apple, it is the ovary which constitutes the edible part of the fruit, and in which the pips are embedded. At first sight, the fruit of the Mulberry—which, however, belongs to a different family—

closely resembles that of the Blackberry. In the Mulberry, however, it is the sepals which become fleshy and sweet.

The next point is that seeds should be in a spot suitable for their growth. In most cases, the seed lies on the ground, into which it then pushes its little rootlet. In plants, however, which live on trees, the case is not so simple, and we meet some curious contrivances. Thus, the Mistletoe, as we all know, is parasitic on trees. The fruits are eaten by birds, and the droppings often therefore fall on the boughs; but if the seed was like that of most other plants it would soon fall to the ground, and consequently perish. Almost alone among English plants it is extremely sticky, and thus adheres to the bark.

I have already alluded to an allied genus, *Arceuthobium*, parasitic on Junipers, which throws its seeds to a distance of several feet. These also are very viscid, or, to speak more correctly, are embedded

Fig. 16.



Myzodendron. (After Hooker.)

in a very viscid mucilage, so that if they come in contact with the bark of a neighbouring tree they stick to it.

Another very interesting genus, again of the same family, is

Myzodendron (Fig. 16), a Fuegian species, described by Sir Joseph Hooker, and parasitic on the Beech. Here the seed is not sticky, but is provided with four flattened flexible appendages. These catch the wind, and thus carry the seed from one tree to another. As soon, however, as they touch any little bough the arms twist round it and there anchor the seed.

Dr. Watt has discovered a still more curious fact in an Indian species of Loranthus, which he considers to be *L. globosus*. The fruit, as is so common in this order, consists of a mass of viscid pulp. Under ordinary circumstances the seeds would be most likely in the first instance to drop upon a leaf; but if they remained there, when the leaves fell from the trees the seeds would drop also. They have, however, a curious power of movement, by means of which they quit the leaves and fasten themselves to the stem. The radicle, when it has elongated itself to about an inch, develops at its extremity a flattened disc. It then curves about until the disc touches any object that is near at hand. To this it then attaches itself, and tears the berry away from its previous position. The radicle then again curves, the berry is again carried to another spot, where it adheres again. This curious process is repeated until the seed finds itself on a spot suitable for its growth.

In many epiphytes the seeds are extremely numerous and minute. Their great numbers increase the chance that the wind may waft some of them to the trees on which they grow; and as they are then fully supplied with nourishment they do not require to carry any store with them. Moreover, their minute size is an advantage, as they are carried into any little chink or cranny in the bark; while a larger or heavier seed, even if borne against a suitable tree, would be more likely to drop off. In the genus *Neumannia*, the small seed is produced at each end into a long filament which must materially increase its chance of adhering to a suitable tree.

Even among terrestrial species there are not a few cases in which plants are not contented simply to leave their seeds on the surface of the soil, but actually sow them in the ground. Thus in *Trifolium subterraneum*, one of our rarer English Clovers, only a few of the florets become perfect flowers, the others form a rigid pointed head which at first is turned upwards, and as their ends are close together, constitute a sort of spike. At first, I say, the flower-heads point upwards like those of other Clovers, but as soon as the florets are fertilised, the flower-stalks bend over and grow downwards, forcing the flower-head into the ground, an operation much facilitated by the peculiar construction and arrangement of the imperfect florets. The florets are, as Darwin has shown, no mere passive instruments. So soon as the flower-head is in the ground they begin, commencing from the outside, to bend themselves towards the peduncle, the result of which of course is to drag the flower-head farther and farther into the ground. In most Clovers each floret

produces a little pod. This would in the present species be useless, or even injurious; many young plants growing in one place would jostle and starve one another. Hence we see another obvious advantage in the fact that only a few florets perfect their seeds.

I have already alluded to our Cardamines, the pods of which open elastically and throw their seeds some distance. A Brazilian species, *C. chenopodifolia*, Fig. 17, besides the usual long pods, Fig. 17 *a a*,

Fig. 17.

Cardamine chenopodifolia.
a a, ordinary pods; *b*, subterranean pods.

produces also short pointed ones, Fig. 17 *b b*, which it buries in the ground.

Arachis hypogæa is the ground-nut of the West Indies. The flower is yellow and resembles that of a pea, but has an elongated

calyx, at the base of which, close to the stem, is the ovary. After the flower has faded, the young pod, which is oval, pointed, and very minute, is carried forward by the growth of the stalk, which becomes several inches long, and curves downwards so as generally to force the pod into the ground. If it fails in this, the pod does not develop, but soon perishes; on the other hand, as soon as it is underground the pod begins to grow and develops two large seeds.

Again, in *Vicia amphicarpa*, Fig. 18, a South European species of

Fig. 18.

Vicia amphicarpa.

a a, ordinary pods; *b b*, subterranean pods.

Vetch, there are two kinds of pods: one of the ordinary form and habit (*a*), the other (*b*) oval, pale, containing only two seeds borne on underground stems, and produced by flowers which have no corolla.

Again, a species of the allied genus *Lathyrus*, Fig. 19, *L. amphicarpos*, affords us another case of the same phenomenon.

Other species possessing the same faculty of burying their seeds are *Okenia hypogæa*, several species of *Commelyna*, and of *Amphicarpæa, Voandzeia subterranea, Scrophularia arguta*, &c.; and it is very remarkable that these species are by no means nearly related, but

FIG. 19.

Lathyrus amphicarpos. (After Sowerby.)

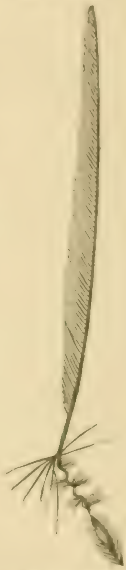
a, ordinary pods; *b*, subterranean pods.

belong to distinct families, namely the *Cruciferæ, Leguminosæ, Commelynaceæ, Violaceæ*, and *Scrophulariaceæ*.

Moreover, it is interesting that in *L. amphicarpos*, as in *Vicia amphicarpa* and *Cardamine chenopodifolium*, the subterranean pods differ from the usual and aërial form in being shorter and containing fewer seeds. The reason of this is, I think, obvious. In the ordinary pods the number of seeds of course increases the chance that some

will find a suitable place. On the other hand, the subterranean ones are carefully sown, as it were, by the plant itself. Several seeds together would only jostle one another, and it is therefore better that one or two only should be produced.

Fig. 20.



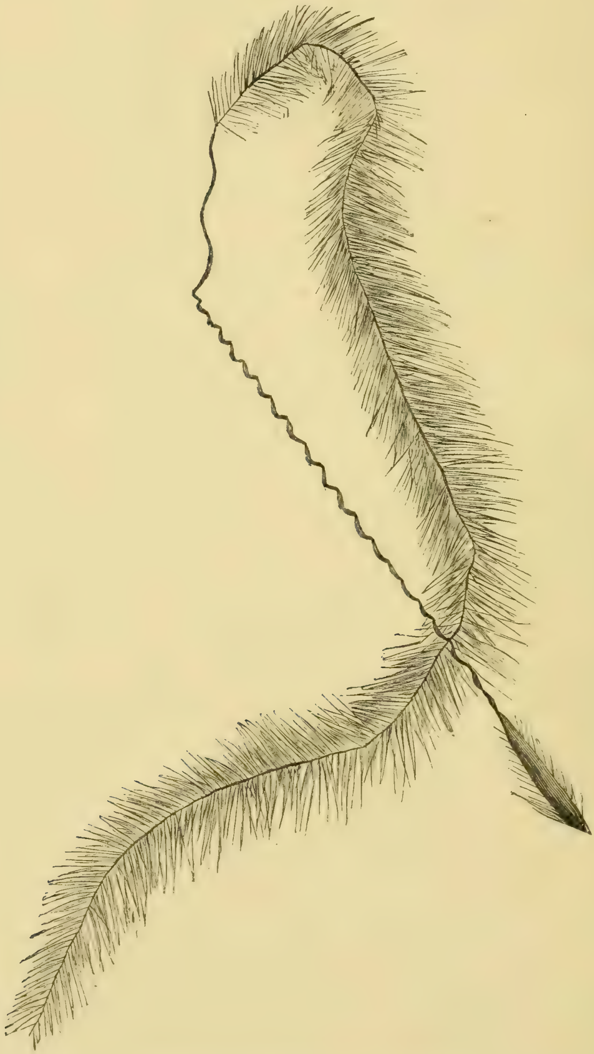
Erodium glaucophyllum. (After Sweet.)

In the *Erodiums*, or Crane's Bills, the fruit is a capsule which opens elastically, in some species throwing the seeds to some little distance. The seeds themselves are more or less spindle-shaped, hairy, and produced into a twisted hairy awn as shown in Fig. 20, representing a seed of *E. glaucophyllum.* The number of spiral turns in the awn depends upon the amount of moisture; and the seed may thus be made into a very delicate hygrometer, for if it be fixed in an upright position, the awn twists or untwists according to the degree of moisture, and its extremity thus may be so arranged as to move up and down like a needle on a register. It is also affected by heat. Now if the awn were fixed instead of the seed, it is obvious that during the process of untwisting, the seed itself would be pressed downwards, and, as M. Roux has shown, this mechanism thus serves actually to bury the seed. His observations were made on an allied species, *Erodium ciconium*, which he chose on account of its size. He found that if a seed of this plant is laid on the ground, it remains quiet as long as it is dry; but as soon as it is moistened—i. e. as soon as the earth becomes in a condition to permit growth—the outer side of the awn contracts, and the hairs surrounding the seed commence to move outwards, the result of which is gradually to raise the seed into an upright position with its point on the soil. The awn then commences to unroll, and consequently to elongate itself upwards, and it is obvious that as it is covered with reversed hairs, it will probably press against some blade of grass or other obstacle, which will prevent its moving up, and will therefore tend to drive the seed into the ground. If then the air becomes dryer, the awn will again roll up, in which action M. Roux thought it would tend to draw up the seed, but from the position of the hairs the feathery awn can easily slip downwards, and would therefore not affect the seed. When moistened once more, it would again force the seed further downwards, and so on until the proper depth was obtained. A species of Anemone (*A. montana*) again has essentially the same arrangement, though belonging to a widely separated order.

A still more remarkable instance is afforded by a beautiful South European grass, *Stipa pennata* (Fig. 21), the structure of which has been described by Vaucher, and more recently, as well as more completely, by Frank Darwin. The actual seed is small, with a sharp

point, and stiff, short hairs pointing backwards. The posterior end of the seed is produced into a fine twisted corkscrew-like rod, which is followed by a plain cylindrical portion, attached at an angle to the

Fig. 21.

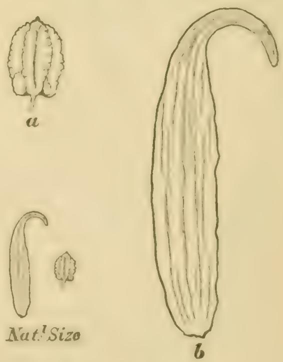


Seed of *Stipa pennata* (natural size).

corkscrew, and ending in a long and beautiful feather, the whole being more than a foot in length. The long feather, no doubt, facilitates the dispersion of the seeds by wind; eventually, however, they sink to the ground, which they tend to reach, the seed being the heaviest portion, point downwards. According to Darwin, the seed remains in the same position as long as it is dry, but if a shower comes on, or when the dew falls, the spiral unwinds, and if, as is most probable, the surrounding herbage or any other obstacle prevents the feather from rising, the seed itself is forced down and so driven by degrees into the ground.

I do not doubt that this seed may bury itself in the manner thus described, but I do doubt whether it always, or indeed generally, does so. One fine day not long ago, I chanced to be looking at a plant of this species in my garden, and round it were several seeds more or less firmly buried in the ground. There was a little wind blowing at the time, and it struck me that the long feathery awn was admirably adapted to catch the wind, while on the other hand it seemed almost too delicate to drive the seed into the ground in the manner described by Darwin. I therefore took a seed and placed it upright on the turf. The day was perfectly dry and fine, so that there could be no question of hygroscopic action. Nevertheless, when I returned after a few hours, I found that the seed had buried itself some little distance in the ground. I repeated the observation several times, always with the same result, and thus convinced myself that one method, at any rate, by which these seeds bury themselves is by taking advantage of the action of the wind, and the twisted portion of the awn by its corkscrew-like movement probably facilitates the entry of the seed into the ground.

FIG. 22.



Seeds of *Corydalis heterocarpa*.

I have already mentioned several cases in which plants produce two kinds of seeds, or at least of pods, the one being adapted to burying itself in the ground. Heterocarpism, if I may term it so, or the power of producing two kinds of reproductive bodies, is not confined to these species. There is, for instance, a North African species of *Corydalis* (*C. heterocarpa* of Durieu) which produces two kinds of seed (Fig. 22), one somewhat flattened, short and broad, with rounded angles; the other elongated, hooked, and shaped like a shepherd's crook with a thickened staff. In this case the hook in the latter form perhaps serves for dispersion.

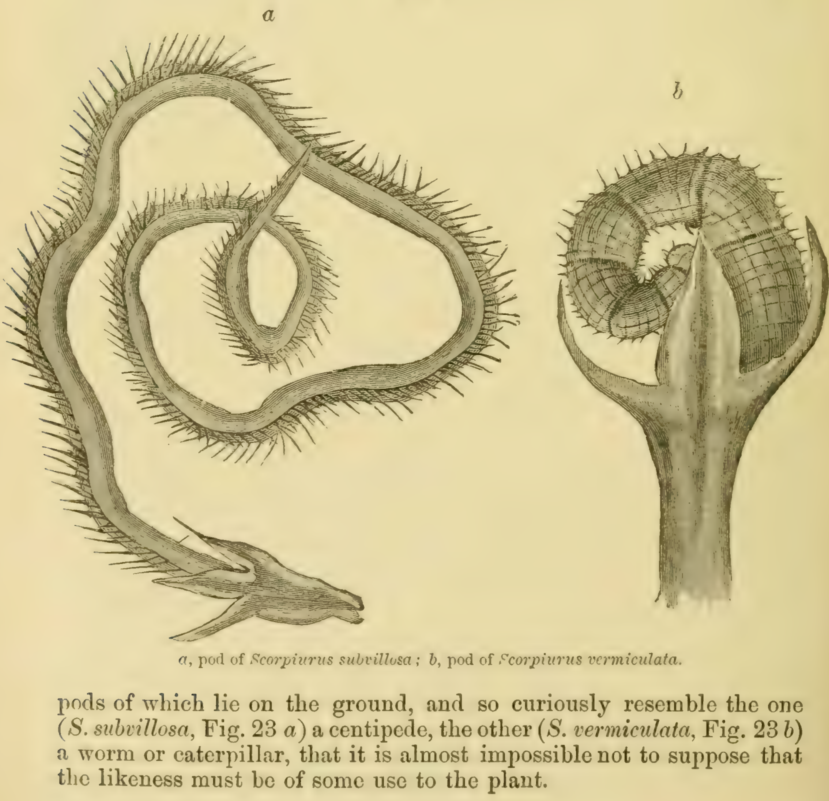
Our common *Thrincia hirta* (Fig. 13 *b*) also possesses, besides the fruits with the well-known feathery crown, others which are destitute

of such a provision, and which probably therefore are intended to take root at home.

Mr. Drummond, in the volume of 'Hooker's Journal of Botany' for 1842, has described a species of *Alismaceæ* which has two sorts of seed-vessels; the one produced from large floating flowers, the other at the end of short submerged stalks. He does not, however, describe either the seeds or seed-vessels in detail.

Before concluding, I will say a few words as to the very curious forms presented by certain seeds and fruits. The pods of Lotus, for instance, quaintly resemble a bird's foot, even to the toes; whence the specific name of one species, *ornithopodioides;* those of *Hippocrepis* remind one of a horseshoe; those of *Trapa bicornis* have an absurd resemblance to the skeleton of a bull's head. These likenesses appear to be accidental, but there are some which probably are of use to the plant. For instance there are two species of *Scorpiurus*, Fig. 23, the

Fig. 23.



a, pod of *Scorpiurus subvillosa*; *b*, pod of *Scorpiurus vermiculata.*

pods of which lie on the ground, and so curiously resemble the one (*S. subvillosa*, Fig. 23 *a*) a centipede, the other (*S. vermiculata*, Fig. 23 *b*) a worm or caterpillar, that it is almost impossible not to suppose that the likeness must be of some use to the plant.

The pod of *Biserrula Pelecinus* (Fig. 24 *a*) also has a striking resemblance to a crushed centipede; while the seeds of *Abrus precatorius*, both in size and in their very striking color, mimic a small beetle, *Artemis circumusta.*

Mr. Moore has recently called attention to other cases of this kind. Thus the seed of *Martynia diandra* much resembles a beetle with long antennæ: several species of Lupins have seeds much like spiders, and those of *Dimorphochlamys*, a gourdlike plant, mimic a piece of dry twig. In the Common Castor Oil plants (Fig. 24 *b*), though the resemblance is not so close, still at first glance the seeds might readily be taken for beetles or ticks. In many Euphorbiaceous plants, as for instance in *Jatropha* (Fig. 24 *c*), the resemblance is even more striking. The seeds have a central line resembling the space between the elytra, dividing and slightly diverging at the end, while between them the end of the abdomen seems to peep; at the anterior end the seeds possess a small lobe, or caruncle, which mimics the head or

Fig. 24.

a. *b.* *c.*

Pod of *Biserrula.* Seed of Castor Oil (*Ricinus*). Seed of *Jatropha.*

thorax of the insect, and which even seems specially arranged for this purpose; at least it would seem from experiments made at Kew that the carunculus exercises no appreciable effect during germination.

These resemblances might benefit the plant in one of two ways. If it be an advantage to the plant that the seeds should be swallowed by birds, their resemblance to insects might lead to this result. On the other hand, if it be desirable to escape from graminivorous birds, then the resemblance to insects would serve as a protection. We do not, however, yet know enough about the habits of these plants to solve this question.

Indeed, as we have gone on, many other questions will, I doubt not, have occurred to you, which we are not yet in a position to answer. Seeds, for instance, differ almost infinitely in the sculpturing of their surface. But I shall woefully have failed in my object to-night if you go away with the impression that we know all about seeds. On the contrary, there is not a fruit or a seed, even of one of our commonest

plants, which would not amply justify and richly reward the most careful study.

In this, as in other branches of science, we have but made a beginning. We have learnt just enough to perceive how little we know. Our great masters in natural history have immortalised themselves by their discoveries, but they have not exhausted the field; and if seeds and fruits cannot vie with flowers in the brilliance and color with which they decorate our gardens and our fields, still they surely rival, it would be impossible to excel them, in the almost infinite variety of the problems they present to us, in the ingenuity, the interest, and the charm of the beautiful contrivances which they offer for our study and our admiration.

[J. L.]

WEEKLY EVENING MEETING,

Friday, March 25, 1881.

WARREN DE LA RUE, Esq. D.C.L. F.R.S. Secretary and Vice-President,
Cor. Mem. Acad. des Sciences, France,
in the Chair.

ALEXANDER BUCHAN, Esq. M.A. F.R.S.E.
Sec. Scot. Met. Soc.

The Weather and Health of London.

To the statistician London affords materials for the prosecution of many inquiries such as could not be obtained from the statistics of any other city either in ancient or modern times. Among the more important of these inquiries are those which relate to questions suggested by the enormous aggregation of human beings over a limited area which London presents on a scale absolutely unparalleled in the world's history. It is one of these questions we bring before you this evening, viz. the influence of the climate on the health of the people of London.

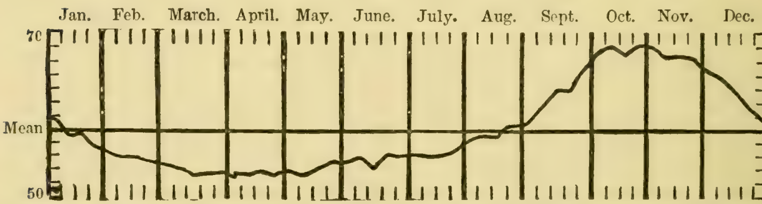
The relation of weather to health is a question which has engaged the attention of Dr. Arthur Mitchell and myself for many years. In an early stage of the inquiry our attention was mainly directed to Scotland, and more particularly to the data supplied by its eight large towns; but it was soon found that, owing to the sparseness and other conditions of the population, and to the division of time into months only, adopted by the Registrar-General for Scotland, the available data were not sufficiently exact to show the true relations of weather to the fluctuations of the death-rate through the year. In truth it was only after not a little unsuccessful labour, and what could at best be characterised as no more than partially successful work, that we resolved eight years ago to open the discussion of the whole subject by an exhaustive examination of the meteorological and vital statistics of London and London alone. More specifically our reasons for the selection of London were that it afforded data from (1) an enormous population spread over an area so limited that it might be regarded as having one uniform climate during each of the seasons of the year; (2) satisfactorily full weekly reports of weather and the deaths from the different diseases; and (3) returns extending over a sufficiently long time.

In the case of diseases such as diarrhœa and bronchitis, which seem to be directly and immediately under the influence of temperature, and such epidemics as scarlet fever and whooping-cough, the rate of

mortality from which is largely determined by season and weather, a comparatively small number of years is required to give a satisfactory approximation to their true weekly curve of mortality. But as regards the great majority of diseases, it quickly became apparent that a thirty years' average was required in the construction of curves which could be offered as true "constants" for the diseases to which they refer. The thirty years beginning with 1845 were therefore adopted. An examination of the curves shows that some of their striking features, particularly those indicating the complications of special diseases and their connections with each other, which the weekly averages disclose, would entirely disappear if monthly averages only were employed.

The curves of the more prominent and interesting of the diseases are shown on the accompanying woodcuts, the straight black line in each figure being drawn to represent the mean weekly death-rate on an average of the fifty-two weeks of the year, and the figures on the margin the percentages above or below the average. With this general average the mean death-rate of each week is compared and the difference above or below calculated in percentages, which, when *plus, are placed* above the mean line of the figure, and when *minus*, below it. Thus as regards scarlatina (Fig. 1), the mean mortality of

FIG. 1.



Scarlet Fever.

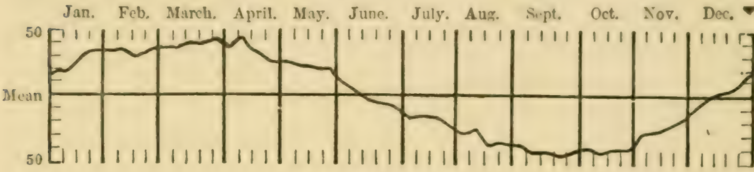
the fifty-two weeks is 49·6; on the first week of January it is 7 per cent. above the mean, from which time it continues to fall to the annual minimum, 35 per cent. below the mean in the middle of March; thence rises to the mean in the end of August; to the annual maximum, 60 per cent. above the mean, in the end of October, and thereafter steadily falls. The portion of the curve above the mean line thus shows the time of the year when, and the degree to which, the mortality from scarlatina is above its average, and the portion below the line when it is under it.

Fig. 2 shows similarly the distribution of the mortality from whooping-cough through the weeks of the year, and Fig. 3 the distribution of the mortality from small-pox. It is seen at once that the mortality curve from scarlatina is precisely the reverse of the curve of whooping-cough, the maximum death-rate period of the one

corresponding to the minimum period of the other, and *vice versâ.* It is also seen that the mortality curve for small-pox (Fig. 3) is quite distinct from the other two curves.

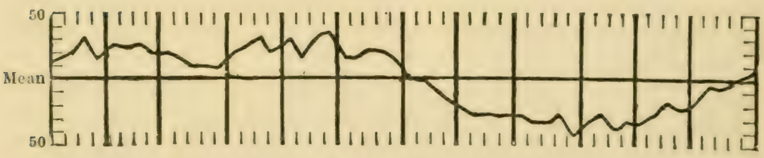
In order to ascertain the degree of steadiness of these curves, a curve was calculated and drawn for each of the seven epidemics of scarlatina and for each of the eight epidemics of whooping-cough during the thirty years, with the instructive result that the curve for

FIG. 2.



Whooping Cough.

FIG. 3.



Small-pox.

each of the separate epidemics was substantially identical with the general curve for the whole thirty years' period, each of the four prominent phases of each curve occurring all within a week of each other. As regards the small-pox curve, if the deaths during the epidemic of 1870–72, by far the most fatal of all the epidemics during the thirty years, be deducted from the general result, we obtain a curve which is substantially the same curve as that for the whole thirty years, but only less pronounced. From these results it follows, and the remark is of general application to all the curves, that the mortality curves for the different diseases arrived at in this inquiry may be regarded as true constants of these diseases for London.

The climate of London, looked at as influencing the health of the people, may be divided into six types of weather according to the season of the year. These are respectively—

Period 1.—Damp and cold, fourth week of October to third week of December.

Period 2.—Cold, fourth week of December to third week of February.

Period 3.—Dry and cold, fourth week of February to second week of April.

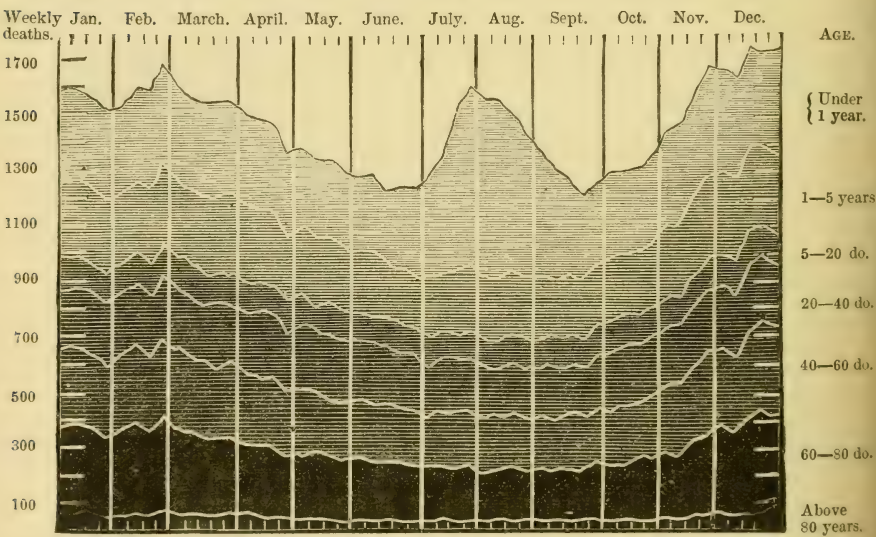
Period 4.—Dry and warm, third week of April to third week of June.

Period 5.—Heat, fourth week of June to first week of September.

Period 6.—Damp and warm, second week of September to third week of October.

The outstanding features of the death-rate in its relation to the varying types of weather through the year are shown by the top curve of Fig. 4, which represents the total mortality for all ages. This curve shows two maxima in the course of the year: the one, by far the larger of the two, extending over six months from November to April, and the other embracing the period from about the beginning of July to the autumnal equinox. It will be also observed that the comparatively short-continued but strongly-pronounced summer maximum is practically restricted to mere infants, whereas the larger winter maximum

Fig. 4.

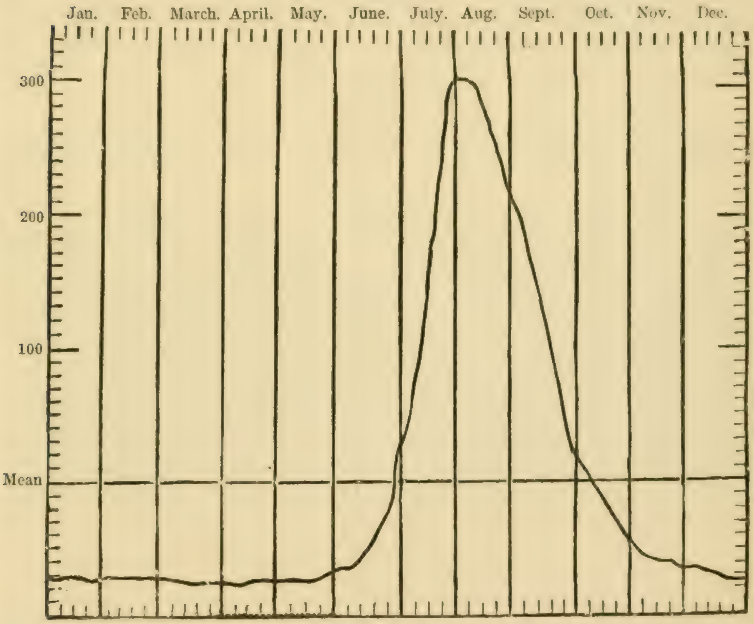


is a feature of the curves for all ages; and hence of all weather influences the cold element in the climate of London is that which is most destructive to life.

Figs. 5 to 10 are representative curves of those diseases which go to form the summer maximum when "*heat*" is the chief characteristic of the weather. The direct relation of the progress of mortality from diarrhœa to temperature is strikingly seen in the startling suddenness with which the curve shoots up during the hottest months of the year, and the suddenness, equally startling, with which it falls on the approach

of colder weather. The curves for dysentery, British cholera, and cholera are substantially the same as the curve for diarrhœa, and all show the same close obedience to temperature. It is a noteworthy circumstance that these four curves group themselves into pairs—diarrhœa and British cholera on the one side, and dysentery and Asiatic cholera on the other. The chief points of difference are that

FIG. 5.

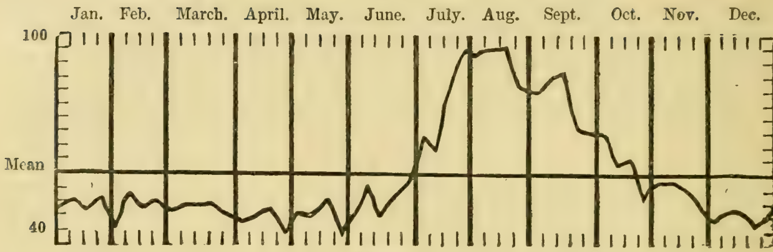


Diarrhœa.

dysentery and Asiatic cholera begin markedly to rise considerably later than the other two allied diseases, attain their maximum a month later, and fall more rapidly than they rose, the annual phases being nearly a month later than those of diarrhœa and British cholera, which diseases are less deeply seated in the system.

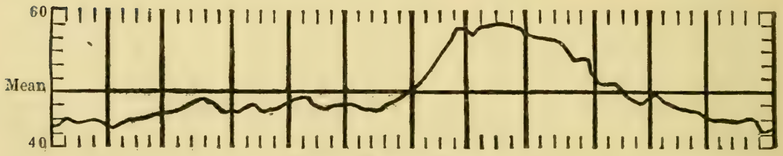
The peculiarly malignant character of summer diarrhœa among young children under five years of age may be shown by the weekly mortality from diarrhœa, rising from 20 in the middle of June, to 342 in the first week of August, 1880, when the mean temperature of July and August was about the average. In July 1876, when the temperature was 3°·6 above the average, the weekly mortality from diarrhœa among children rose to 502 on the last week of that month. On the

Fig. 6.



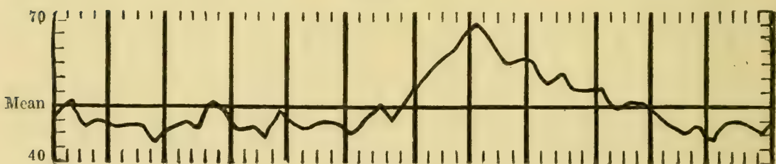
Thrush.

Fig. 7.



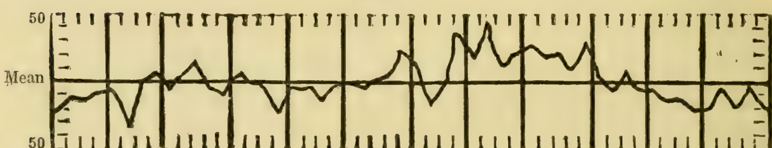
Tabes Mesenterica.

Fig. 8.



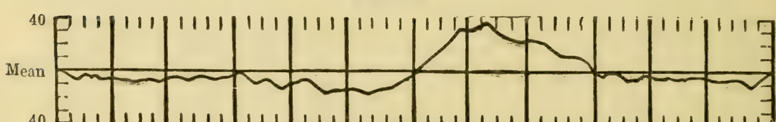
Enteritis.

Fig. 9.



Jaundice.

Fig. 10.

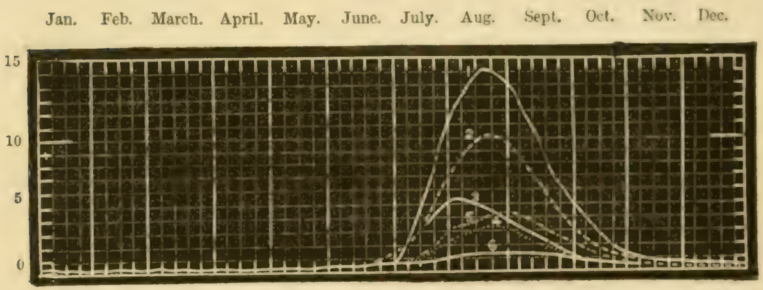


Atrophy and Debility.

other hand, during the cold summer of 1860, the diarrhœa mortality for all ages did not in any week exceed 90.

Of the British large towns the lowest mortality from summer diarrhœa is that of Aberdeen, which has the lowest summer temperature. The diarrhœa mortality of each town is found from year to year to rise proportionally with the increase of temperature, but the rate of increase differs widely in different towns, thus pointing to other causes than mere weather, or the relative temperatures and humidities of these towns, as determining the mortality. Fig. 11 shows the weekly death-rate from diarrhœa for six of the largest British towns, viz. Leicester, curve 1; Liverpool, 2; London, 3; Bristol, 4; Portsmouth, 5; and Edinburgh, 6; from which it is seen that though the summer temperature of London is higher than that of Liverpool and Leicester, its diarrhœa mortality is very much less. In this respect London contrasts very favourably with the great majority of British large

FIG. 11.



Weekly Deaths from Diarrhœa calculated on the Annual Mortality per 1000 of the population.

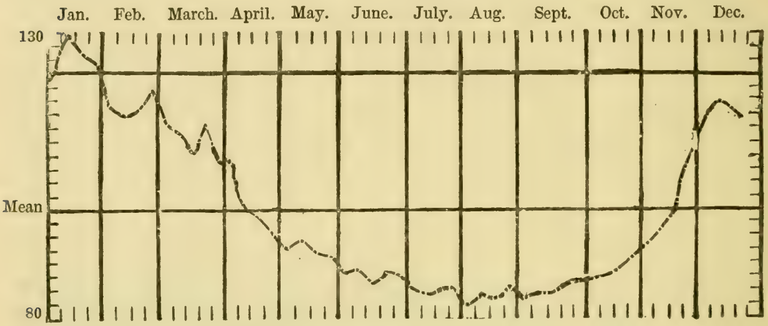
towns, showing its sanitary conditions generally are at least fairly satisfactory; but inasmuch as it is somewhat in excess of a few of the towns whose summer temperature is scarcely lower, London offers problems in this field to the sanitary reformer for his solution.

Figs. 6 to 10 give the curves for thrush, tabes mesenterica, enteritis, jaundice, and atrophy and debility, all of which have their maximum fatality during the hottest period of the year, and all of these, it will be noted, are bowel complaints. Indeed with the apparent exception of one or two nervous diseases, all those diseases which indicate an increase in their death-rate during the summer months are bowel complaints.

An examination of the curve for the whole mortality (Fig. 4) shows that the great preponderance of deaths in London takes place during the coldest months of the year. Of the diseases to which this excessive mortality is due the first place must be assigned to

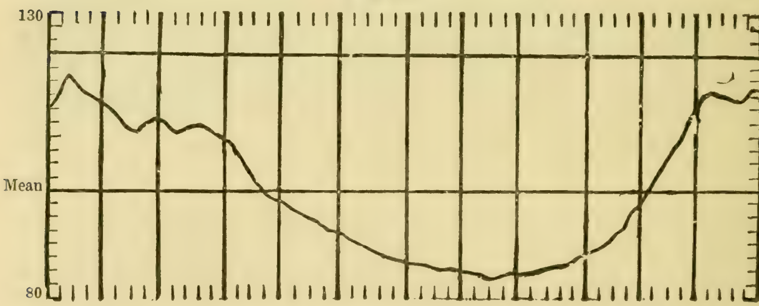
diseases of the respiratory organs, the more marked of which are given in Figs. 12 to 15. About one in eight of all deaths that occur is caused by bronchitis, and one in sixteen by pneumonia; so that nearly one-fifth of the deaths is occasioned by these two diseases of the respiratory organs. Our researches appear to warrant the conclusion that the greatest fatality from these diseases occurs when the temperature is

Fig. 12.



Asthma.

Fig. 13.



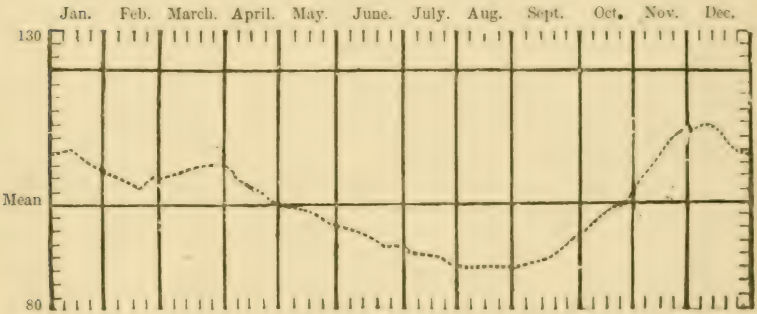
Bronchitis.

between 32° and 40°. In New York, where the mean winter temperature is 10°·0 lower than in London, the mortality from bronchitis and pneumonia is greatly less; and on the other hand, in Melbourne, where the winter temperature is about 10°·0 higher than that of London, the mortality from diseases of the respiratory organs forms but a small fraction of the whole deaths.

These four curves of the mortality from diseases of the respiratory organs are substantially the same, each having its maximum in the cold months, and its minimum in the warm months. Asthma shows,

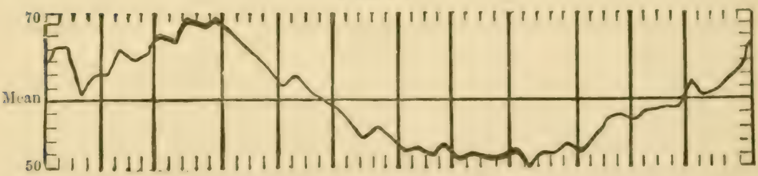
in the amplitude of its annual rage, the greatest sensitiveness to weather, and pneumonia the least. They all show, though in different degrees, a double-ridged maximum: the one ridge being in the middle of January, when the temperature falls to the annual minimum, and the other in March, when the combined qualities of cold and dryness are at the annual maximum. Asthma and bronchitis are decidedly at the maximum when the weather is coldest, whereas laryngitis has its maximum in March, when the weather is coldest and driest, the last disease thus forming the link connecting the more strictly throat diseases with diseases of the nervous system.

FIG. 14.



Pneumonia.

FIG. 15.



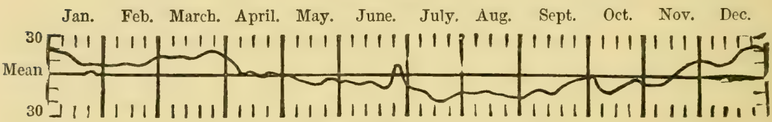
Laryngitis.

But an element of weather other than mere temperature plays an important part in bringing about the high death-rate from these diseases. That deleterious atmospheric influence is fog; and in cases where the fog is dense and persistent the mortality from diseases of the respiratory organs becomes truly appalling, as happened in London in 1880, when the mortality was nearly doubled. An examination of the fogs of London shows that they do not commence till the autumnal equinox; and it is at this epoch that asthma (Fig. 12), which is by far the most sensitive of all diseases to fog, starts from its annual minimum; and in the end of November and beginning

of December, when fogs become most frequent, the curves for asthma and bronchitis shoot up with startling suddenness.

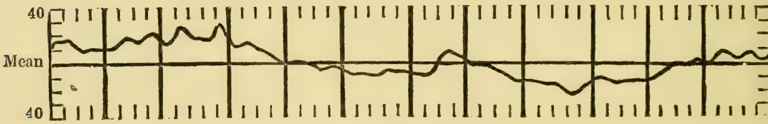
Figs. 16, 17, and 18 represent the curves for three of the nervous diseases, viz. apoplexy, convulsions, and cephalitis. Apoplexy will be observed to show a double-ridged maximum quite analogous to that of the diseases of the respiratory organs; whereas in the case of convulsions, the maximum may be regarded as quite single, and

Fig. 16.



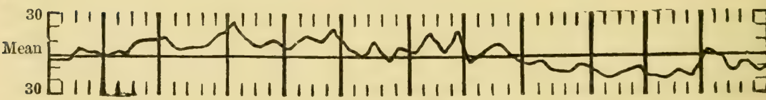
Apoplexy.

Fig. 17.



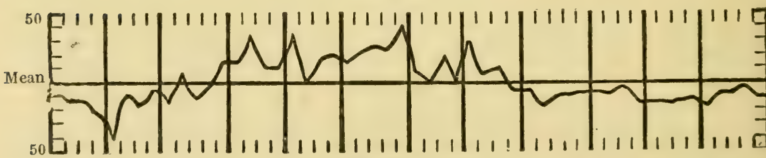
Convulsions.

Fig. 18.



Cephalitis.

Fig. 19.



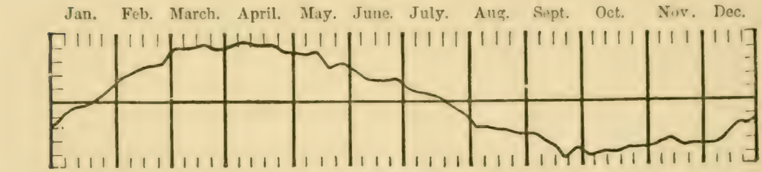
Suicides.

occurring in spring, this being the season when nervous diseases generally are most fatal. On the other hand, the curve for cephalitis stands alone among nervous diseases as having its annual maximum considerably later and as keeping above the mean till at least the end of July, thus covering that portion of the year when the climate is driest and hottest, as well as driest and coldest. The intimate

relations observed between the curve for suicides (Fig. 19) and that for cephalitis is very striking.

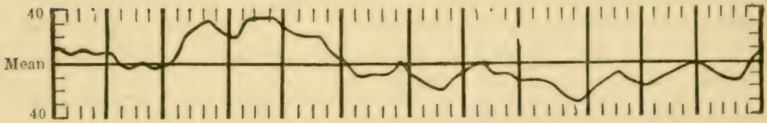
The maximum mortality for whooping-cough (Fig. 20), gout (Fig. 21), and phthisis (Fig. 22), occur in the same season as that for the nervous diseases. The maximum mortality from whooping-cough occurs in the spring months, and the curve suggests that this is more a disease of the nervous system than of the respiratory organs, a view which, singularly enough, was maintained by the elder Dr. Begbie, one of the most distinguished of our Edinburgh physicians, upwards of thirty years ago. The relations of gout to diseases of the nervous

Fig. 20.



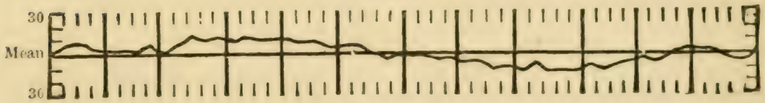
Whooping Cough.

Fig. 21.



Gout.

Fig. 22.



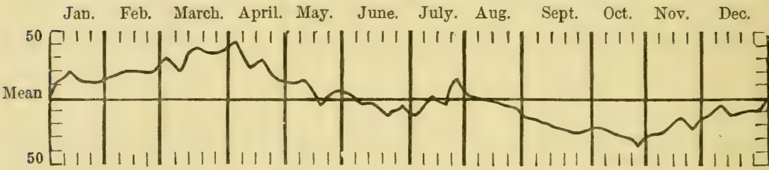
Phthisis.

system are too obvious to call for remark. Phthisis is one of the two most fatal scourges of our British climate, one out of every eight deaths which occur being caused by consumption. Its mortality-curve (Fig. 22) shows unmistakably its intimate relations to nervous diseases, thus affixing greater significance to its known complications with hereditary insanity, scrofula, and some other mental diseases.

Reference has been made to the influence of the heat of summer on certain nervous diseases. That influence acts fatally, both indirectly through the bowels in the case of the young, and directly on the nervous centres. The curve for convulsions (Fig. 17) is

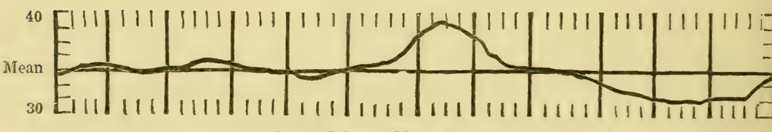
identical with that for teething (Fig. 23), and it may be added that the curve for hydrocephalus is simply a reproduction of the same curves. Now these curves show a small, but distinct, and, as revealed by each year's figures, a constantly recurring secondary maximum in summer, which in the case of London is almost wholly due to the bowel complications of these diseases. The curve (Fig. 24) for convulsions for New York, where the summer temperature is 10°·0 hotter than in London, shows this feature of the curve enormously magnified, so much so, indeed, that instead of being, as in London, an insignificant secondary maximum, it stands out as the prominent feature of the

Fig. 23.



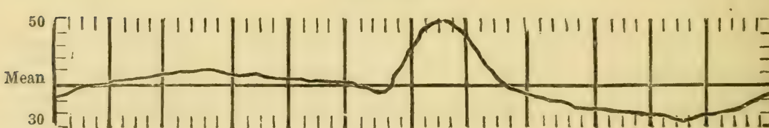
Teething.

Fig. 24.



Convulsions (New York).

Fig. 25.

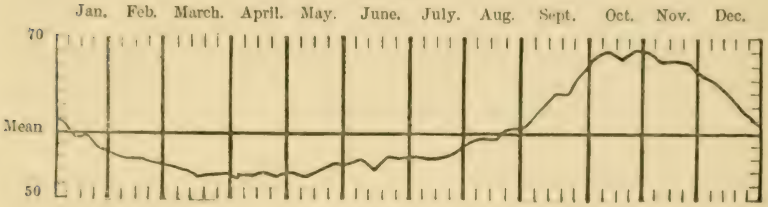


All Nervous Diseases (New York).

curve. Whilst this result is doubtless largely due to complications with bowel complaints, it is, as an examination of the statistics shows, in no small degree caused by the direct influence of the great summer heat of New York on the nervous centres. This is impressively shown by the mortality curve for the whole of the nervous diseases (Fig. 25), which is even more pronounced in this particular than the curve for convulsions alone (Fig. 24). Keeping this fact in view, the peaks showing an increased fatality in London from cephalitis (Fig. 18) and suicides (Fig. 19) during July and August acquire, in the eyes of the physician, a more impressive significance.

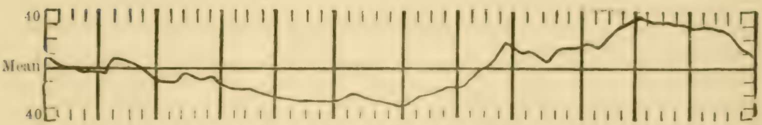
The curve for the whole mortality (Fig. 4) shows September and October to be two of the healthiest months of the year. The three curves, scarlet fever (Fig. 26), typhoid (Fig. 27), and diphtheria (Fig. 28), are the most striking exceptions to this, these curves all

Fig. 26.



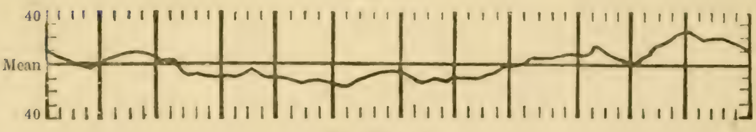
Scarlet Fever.

Fig. 27.



Typhoid Fever.

Fig. 28.



Diphtheria.

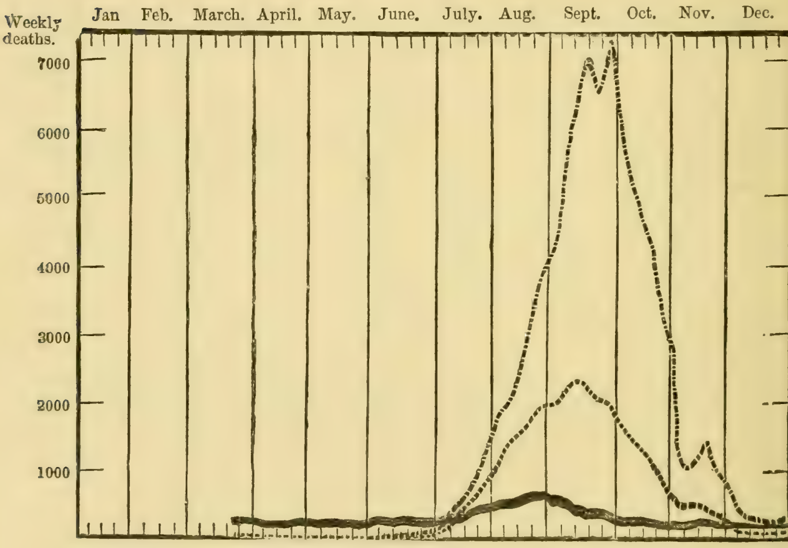
indicating either a large increase in the death-rate or a high mortality during these months. While closely related to each other, each of these three diseases has a distinct individuality of its own as regards the times of occurrence of the annual maxima and minima, and the varying amplitudes of their range from the mean line. It is a singular circumstance that diphtheria shows closer relations in its death-rate with typhoid than with scarlet fever.

Several other diseases suggest close alliances with each other through their seasonal death-rates. Thus the curve for mortification is substantially that of nervous diseases, and the curves for erysipelas and puerperal fever are in all essential respects the same, a fact of singular suggestiveness to the family practitioner. The curve for old age runs exactly parallel to that of paralysis, the old man's disease. The curves for skin diseases, rheumatism, dropsy, pericarditis, Bright's

disease, and kidney disease exhibit most striking, and in many cases the closest alliances with each other. Lastly, while bowel complaints attain their greatest mortality when temperature is highest, diseases of the respiratory organs when it is lowest, nervous diseases during the dry weather of spring and early summer, and skin diseases and certain fevers during the raw weather of autumn and early winter, such diseases as ileus, that are quite removed from weather influences, exhibit curves which show no obedience whatever to season, but only a succession of sharp, irregular serratures resembling the teeth of a saw.

Atrophy and debility are most fatal to the very young in summer,

Fig. 29.



The Great Plague of London.

but to the aged in winter; in the former case the complication being with bowel complaints, and in the latter with diseases of the respiratory organs. The annals of influenza show that a special character is given to this epidemic according to the season of the year in which it occurs. Thus when it occurs in spring the head and nervous system are most affected, but the bowels when the epidemic prevails in summer and autumn.

Fig. 29 shows by the doubly-dotted line, or highest curve, the weekly mortality of London during the Great Plague of 1665, the lower dotted curve the mean weekly mortality of the last six plagues, and the solid curve the mean weekly mortality from all other diseases

during the continuance of the last six plagues. The manner in which the plague, as a death-producer, obeyed the weather is striking, and full of interest. It did so exactly in the way in which we have seen bowel complaints to be influenced by weather. The curve of mortality for the plague bears no resemblance whatever to that for typhus, or indeed to any disease except bowel complaints. The fact that the progress of deaths from plague in relation to weather resembles so closely the corresponding progress of deaths from bowel complaints, raises the question whether there may not be a closer alliance between them than has been suspected. If we are correct in regarding such a question as a fair outcome of this investigation of the relations of weather and health, it is evident that such investigations may occasionally point to a seat of morbid processes which have been cloaked by prominent phenomena, apparently of a primary, but in reality of a secondary character.

[A. B.]

WEEKLY EVENING MEETING,

Friday, May 13, 1881.

Frederick J. Bramwell, Esq. F.R.S. in the Chair.

Francis Galton, Esq. F.R.S.

The Visions of Sane Persons.

[Reprinted, with slight revisions, from the 'Fortnightly Review,' June 1881.]

In the course of some recent inquiries into visual memory, I was greatly struck by the frequency of the replies in which my informants described themselves as subject to "visions." Those of whom I speak were sane and healthy, but were subject notwithstanding to visual presentations, for which they could not account, and which in a few cases reached the level of hallucinations. This unexpected prevalence of a visionary tendency among persons who form a part of ordinary society seems to me suggestive and worthy of being put on record.

Many of my facts are derived from personal friends, of whose accuracy I have no doubt. Another group comes from correspondents who have written at length with much painstaking, and whose letters appear to me to bear internal marks of scrupulous truthfulness. A third part has been collected for me by many kind friends in many countries, each of whom has made himself or herself an independent centre of inquiry; and the last, and much the most numerous portion consists of brief replies by strangers to a series of questions contained in a circular that I drew up. I have gone over all this matter with great care, and have cross-tested it in many ways whilst it was accumulating, just as any conscientious statistician would, before I began to form conclusions. I was soon convinced of its substantial trustworthiness, and that conviction has in no way been shaken by subsequent experience. In short, the evidence of the four groups I have just mentioned is quite as consistent as could have been reasonably desired.

In speaking of the tendency among sane and healthy persons to see images flash unaccountably into existence, it must be recollected that the images vary greatly in distinctness. Some are so faint and evanescent as to appear unworthy of serious notice; others leave a deep impression, and others again are so vivid as actually to deceive the judgment. All of these belong to the same category, and it is the assurance of their common origin that affords the justification I need for directing scientific attention to what many may be inclined to contemptuously disregard as the silly vagaries of vacant minds.

The lowest order of phenomena that admit of being classed as visions are the "Number forms" to which I have drawn attention on more than one occasion, but to which I must again very briefly allude. They are faint and fitful in many children, but are an abiding mental peculiarity in a certain proportion (say 5 per cent.) of adults, who are unable, and who have been ever unable as far back as they can recollect, to think of any number without referring it to its own particular habitat in their mental field of view. It there lies latent, but is instantly evoked by the thought or mention of it, or by any mental operation in which it is concerned. The thought of a series of consecutive numbers is therefore attended by a vision of them arranged in a perfectly defined and constant position, and this I have called a "Number form." Its origin can rarely be referred to any nursery diagram, to the clock-face, or to any incident of childhood. Nay, the form is frequently unlike anything the child could possibly have seen, reaching in long vistas and perspectives, and in curves of double curvature. I have even had to get wire models made by some of my informants in explanation of what they wished to convey. The only feature that all the forms have in common is their dependence in some way or other upon the method of verbal counting, as shown by their angles and other divisions occurring at such points as those where the 'teens begin, at the twenty's, thirty's, and so on. The forms are in each case absolutely unchangeable, except through a gradual development in complexity. Their diversity is endless, and the Number forms of different persons are mutually unintelligible.

These strange "visions," for such they must be called, are extremely vivid in some cases but are almost incredible to the vast majority of mankind, who would set them down as fantastic nonsense; nevertheless, they are familiar parts of the mental furniture of the rest, in whose imaginations they have been unconsciously formed and where they remain unmodified and unmodifiable by teaching. I have received many touching accounts of their childish experiences from persons who see the Number forms, and the other curious visions of which I shall speak. As is the case with the colour-blind, so with these seers. They imagined at first that everybody else had the same way of regarding things as themselves. Then they betrayed their peculiarities by some chance remark that called forth a stare of surprise, followed by ridicule and a sharp scolding for their silliness, so that the poor little things shrunk back into themselves, and never ventured again to allude to their inner world. I will quote just one of many similar letters as a sample. I received this, together with much interesting information, immediately after a lecture I gave last autumn to the British Association at Swansea* in which I had occasion to speak of the Number forms. The writer says:—

"I had no idea for many years, that every one did not imagine numbers in the same positions as those in which they appear to me. One unfortunate day I spoke of it, and was sharply rebuked for my

* See 'Fortnightly Review,' September 1880.

absurdity. Being a very sensitive child I felt this acutely, but nothing ever shook my belief that, absurd or not, I always saw numbers in this particular way. I began to be ashamed of what I considered a peculiarity, and to imagine myself, from this and various other mental beliefs and states, as somewhat isolated and peculiar. At your lecture the other night, though I am now over twenty-nine, the memory of my childish misery at the dread of being peculiar came over me so strongly, that I felt I must thank you for proving that, in this particular at any rate, my case is most common."

The next form of vision of which I will speak is the instant association of colour with sound, which characterises a small percentage of adults, but appears to be rather common, though in an ill-developed degree, among children. I can here appeal not only to my own collection of facts, but to those of others, for the subject has latterly excited some interest in Germany. The first widely known case was that of the brothers Nussbaumer, published in 1873 by Professor Bruhl, of Vienna, of which the English reader will find an account in the last volume of Lewis's 'Problems of Life and Mind' (p. 280). Since then many occasional notices of similar associations have appeared, but I was not aware that it had been inquired into on a large scale by any one but myself. However, I was gratified by meeting with a pamphlet a few weeks ago, just published in Leipsic by two Swiss investigators, Messrs. Bleuler and Lehmann. Their collection of cases is fully as large as my own, and their results in the more important matters are similar to mine. One of the two authors had the faculty very strongly, and the other had not; so they worked conjointly with advantage. As my present object is to subordinate details to the general impression that I wish to convey of the visionary tendency of certain minds, I will simply remark, first, that the persistence of the colour association with sounds is fully as remarkable as that of the Number form with numbers. Secondly, that the vowel sounds chiefly evoke them. Thirdly, that the seers are invariably most minute in their description of the precise tint and hue of the colour. They are never satisfied, for instance, with saying "blue," but will take a great deal of trouble to express or to match the particular blue they mean. Lastly, no two people agree, or hardly ever do so, as to the colour they associate with the same sound. I have hung upon the wall one of the most extraordinary diagrams of these colour associations that has, I suppose, ever been produced. It was drawn by Mr. J. Key, of Graham's Town, South Africa. He sent me in the first instance a communication on the subject, which led to further correspondence, and eventually to the production of this diagram of colours in connection with letters and words. I have no reason to doubt its trustworthiness, and am bound to say that, strange as it looks, and elaborate as it is, I have other written accounts that almost match it.

A third curious and abiding fantasy of certain persons is invariably to connect visualised pictures with words, the same picture

to the same word. These are perceived by many in a vague, fleeting and variable way, but to a few they appear strangely vivid and permanent. I have collected many cases of this peculiarity, and am much indebted to the authoress, Mrs. Haweis, who sees these pictures, for her kindness in sketching some of them for me, for permitting me to exhibit them on the screen, and to use her name in guarantee of their genuineness. She says:—

"Printed words have always had faces to me; they had definite expressions, and certain faces made me think of certain words. The words had *no* connection with these except sometimes by accident. The instances I give are few and ridiculous. When I think of the word Beast, it has a face something like a gurgoyle. The word Green has also a gurgoyle face, with the addition of big teeth. The word Blue blinks and looks silly, and turns to the right. The word Attention has the eyes greatly turned to the left. It is difficult to draw them properly because, like 'Alice's' 'Cheshire cat,' which at times became a grin without a cat, these faces have expression without features. The expression of course" [note the *naïve* phrase "of course."—F. G.] "depends greatly on those of the letters, which have likewise their faces and figures. All the little a's turn their eyes to the left, this determines the eyes of Attention. Ant, however, looks a little down. Of course these faces are endless as words are, and it makes my head ache to retain them long enough to draw."

Some of the figures are very quaint. Thus the interrogation "what?" always excites the idea of a fat man cracking a long whip. They are not the capricious creations of the fancy of the moment, but are the regular concomitants of the words, and have been so as far back as the memory is able to recall.

When in perfect darkness, if the field of view be carefully watched, many persons will find a perpetual series of changes to be going on automatically and wastefully in it. I have much evidence of this. I will give my own experience the first, which is striking to me, because I am very unimpressionable in these matters. I visualise with effort; I am peculiarly inapt to see "after-images," "phosphenes," "light-dust," and other phenomena due to weak sight or sensitiveness; and, again, before I thought of carefully trying, I should have emphatically declared that my field of view in the dark was essentially of a uniform black, subject to an occasional light-purple cloudiness and other small variations. Now, however, after habituating myself to examine it with the same sort of strain that one tries to decipher a sign-post in the dark, I have found out that this is by no means the case, but that a kaleidoscopic change of patterns and forms is continually going on, but they are too fugitive and elaborate for me to draw with any approach to truth. My deficiencies, however, are well supplied by other drawings in my possession. These are by the Rev. George Henslow, whose visions are far more vivid than mine. His experiences are not unlike those of Goethe, who said, in an often-quoted passage, that

whenever he bent his head and closed his eyes and thought of a rose, a sort of rosette made its appearance, which would not keep its shape steady for a moment, but unfolded from within, throwing out a succession of petals, mostly red but sometimes green, and that it continued to do so without change in brightness and without causing him any fatigue so long as he cared to watch it. Mr. Henslow, when he shuts his eyes and waits, is sure in a short time to see before him the clear image of some object or other, but usually not quite natural in its shape. It then begins to change from one object to another, in his case also for as long a time as he cares to watch it. Mr. Henslow has zealously made repeated experiments on himself, and has drawn what he sees. He has also tried how far he is able to mould the visions according to his will. In one case, after much effort, he contrived to bring the imagery back to its starting point, and thereby to form what he terms a "visual cycle." The following account is extracted and condensed from his very interesting letter, and will explain the photographs from his drawings that I am about to throw on the screen.

The first image that spontaneously presented itself was a cross-bow; this was immediately provided with an arrow, remarkable for its pronounced barb and superabundance of feathering. Some person, but too indistinct to recognise much more of him than the hands, appeared to shoot the arrow from the bow. The single arrow was then accompanied by a flight of arrows from right to left, which completely occupied the field of vision. These changed into falling stars, then into flakes of a heavy snow-storm; the ground gradually appeared as a sheet of snow where previously there had been vacant space. Then a well-known rectory, fish-ponds, walls, &c., all covered with snow, came into view most vividly and clearly defined. This somehow suggested another view, impressed on his mind in childhood, of a spring morning, brilliant sun, and a bed of red tulips: the tulips gradually vanished except one, which appeared now to be isolated and to stand in the usual point of sight. It was a single tulip, but became double. The petals then fell off rapidly in a continuous series until there was nothing left but the pistil, but (as is almost invariably the case with his objects) that part was greatly exaggerated. The stigmas then changed into three branching brown horns; then into a knob, while the stalk changed into a stick. A slight bend in it seems to have suggested a centre-bit; this passed into a sort of pin passing through a metal plate; this again into a lock, and afterwards into a nondescript shape, distantly suggestive of the original cross-bow. Here Mr. Henslow endeavoured to force his will upon the visions, and to reproduce the cross-bow, but the first attempt was an utter failure. The figure changed into a leather strap with loops, but while he still endeavoured to change it into a bow the strap broke, the two ends were separated, but it happened that an imaginary string connected them. This was the first concession of his automatic chain of thoughts to his will. By a continued effort the bow came, and then no difficulty

was felt in converting it into the cross-bow and thus returning to the starting-point.

I have a sufficient variety of cases to prove the continuity between all the forms of visualisation, beginning with an almost total absence of it, and ending with a complete hallucination. The continuity is, however, not simply that of varying degrees of intensity, but of variations in the character of the process itself, so that it is by no means uncommon to find two very different forms of it concurrent in the same person. There are some who visualise well and who also are seers of visions, who declare that the vision is not a vivid visualisation, but altogether a different phenomenon. In short, if we please to call all sensations due to external impressions "*direct*," and all others "*induced*," then there are many channels through which the "*induction*" may take place, and the channel of ordinary visualisation in the persons just mentioned is different from that through which their visions arise.

The following is a good instance of this condition. A friend writes:—

"These visions often appear with startling vividness, and so far from depending on any voluntary effort of the mind, they remain when I often wish them very much to depart, and no effort of the imagination can call them up. I lately saw a framed portrait of a face which seemed more lovely than any painting I have ever seen, and again I often see fine landscapes which bear no resemblance to any scenery I have ever looked upon. I find it difficult to define the difference between a waking vision and a mental image, although the difference is very apparent to myself. I think I can do it best in this way. If you go into a theatre and look at a scene, say of a forest by moonlight, at the back part of the stage, you see every object distinctly and sufficiently illuminated (being thus unlike a mere act of memory), but it is nevertheless vague and shadowy, and you might have difficulty in telling afterwards all the objects you have seen. This resembles a mental image in point of clearness. The waking vision is like what one sees in the open street in broad daylight, when every object is distinctly impressed on the memory. The two kinds of imagery differ also as regards voluntariness, the image being entirely subservient to the will, the visions entirely independent of it. They differ also in point of suddenness, the images being formed comparatively slowly as memory recalls each detail, and fading slowly as the mental effort to retain them is relaxed; the visions appearing and vanishing in an instant. The waking visions seem quite close, filling as it were the whole head, while the mental image seems further away in some far-off recess of the mind."

The number of persons who see visions no less distinctly than this correspondent is much greater than I had any idea of when I began this inquiry. I am permitted to exhibit the sketch of one, prefaced by a description of it by Mrs. Haweis. She says:—

"All my life long I have had one very constantly recurring vision,

a sight which came whenever it was dark or darkish, in bed or otherwise. It is a flight of pink roses floating in a mass from left to right, and this cloud or mass of roses is presently effaced by a flight of 'sparks' or gold speckles across them. The sparks totter or vibrate from left to right, but they fly distinctly upwards: they are like tiny blocks, half gold, half black, rather symmetrically placed behind each other, and they are always in a hurry to efface the roses: sometimes they have come at my call, sometimes by surprise, but they are always equally pleasing. What interests me most is that, when a child under nine, the flight of roses was light, slow, soft, close to my eyes, roses so large and brilliant and palpable that I tried to touch them: the *scent* was overpowering, the petals perfect, with leaves peeping here and there, texture and motion all natural. They would stay a long time before the sparks came, and they occupied a large area in black space. Then the sparks came slowly flying, and generally, not always, effaced the roses at once, and every effort to retain the roses failed. Since an early age the flight of roses has annually grown smaller, swifter, and farther off, till by the time I was grown up my vision had become a speck, so instantaneous that I had hardly time to realise that it was there before the fading sparks showed that it was past. This is how they still come. The pleasure of them is past, and it always depresses me to speak of them, though I do not now, as I did when a child, connect the vision with any elevated spiritual state. But when I read Tennyson's 'Holy Grail,' I wondered whether anybody else had had my vision,—'Rose-red, with beatings in it.' I may add, I was a London child who never was in the country but once, and I connect no particular flowers with that visit. I may almost say that I had never seen a rose, certainly not a quantity of them together."

A common form of vision is a phantasmagoria, o the appearance of a crowd of phantoms, sometimes hurrying past like men in a street. It is occasionally seen in broad daylight, much more often in the dark; it may be at the instant of putting out the candle, but it generally comes on when the person is in bed, preparing to sleep, but by no means yet asleep. I know no less than three men, eminent in the scientific world, who have these phantasmagoria in one form or another. A near relative of my own had them in a marked degree. She was eminently sane, and of such good constitution that her faculties were hardly impaired until near her death at ninety. She frequently described them to me. It gave her amusement during an idle hour to watch these faces, for their expression was always pleasing, though never strikingly beautiful. No two faces were ever alike, and no face ever resembled that of any acquaintance. When she was not well the faces usually came nearer to her, sometimes almost suffocatingly close. She never mistook them for reality, although they were very distinct. This is quite a typical case, similar in most respects to many others that I have.

A notable proportion of sane persons have had not only visions

but actual hallucinations of sight, sound, or other sense, at one or more periods of their lives. I have a considerable packet of instances contributed by my personal friends, besides a large number communicated to me by other correspondents. One lady, a distinguished authoress, who was at the time a little fidgeted, but in no way overwrought or ill, assured me that she once saw the principal character of one of her novels glide through the door straight up to her. It was about the size of a large doll, and it disappeared as suddenly as it came. Another lady, the daughter of an eminent musician, often imagines she hears her father playing. The day she told me of it the incident had again occurred. She was sitting in a room with her maid, and she asked the maid to open the door that she might hear the music better. The moment the maid got up the hallucination disappeared. Again, another lady, apparently in vigorous health, and belonging to a vigorous family, told me that during some past months she had been plagued by voices. The words were at first simple nonsense; then the word "pray" was frequently repeated; this was followed by some more or less coherent sentences of little import, and finally the voices left her. In short, the familiar hallucinations of the insane are to be met with far more frequently than is commonly supposed, among people moving in society and in good working health.

I have now nearly done with my summary of facts; it remains to make a few comments on them.

The weirdness of visions lies in their sudden appearance, in their vividness while present, and in their sudden departure. An incident in the Zoological Gardens struck me as a helpful simile. I happened to walk to the seal-pond at a moment when a sheen rested on the unbroken surface of the water. After waiting a while I became suddenly aware of the head of a seal, black, conspicuous, and motionless, just as though it had always been there, at a spot on which my eye had rested a moment previously and seen nothing. Again, after a while my eye wandered, and on its returning to the spot, the seal was gone. The water had closed in silence over its head without leaving a ripple, and the sheen on the surface of the pond was as unbroken as when I first reached it. Where did the seal come from, and whither did it go? This could easily have been answered if the glare had not obstructed the view of the movements of the animal under water. As it was, a solitary link in a continuous chain of actions stood isolated from all the rest. So it is with the visions; a single stage in a series of mental processes emerges into the domain of consciousness. All that precedes and follows lies outside of it, and its character can only be inferred. We see in a general way, that a condition of the presentation of visions lies in the over-sensitiveness of certain tracks or domains of brain action, and the under-sensitiveness of others; certain stages in a mental process being vividly represented in consciousness while the other stages are unfelt. It is also well known that a condition of partial

hyperæsthesia and partial anæsthesia is a frequent functional disorder, markedly so among the hysterical and hypnotic, and an organic disorder among the insane. The abundant facts that I have collected seem to show that it may also coexist with all the appearances of good health and sober judgment.

A convenient distinction is made between hallucinations and illusions. Hallucinations are defined as appearances wholly due to fancy; illusions, as misrepresentations of objects actually seen. There is also a hybrid case which depends on fanciful visions fancifully observed. The problems we have to consider are, on the one hand, those connected with "*induced*" vision, and, on the other hand, those connected with the interpretation of vision, whether the vision be *direct* or *induced.*

It is probable that much of what passes for hallucination proper belongs in reality to the hybrid case, being an illusive interpretation of some induced visual cloud or blur. I spoke of the ever-varying patterns in the field of view; these, under some slight functional change, may become more consciously present, and be interpreted into fantasmal appearances. Many cases, if time allowed, could be adduced to support this view.

I will begin, then, with illusions. What is the process by which they are established? There is no simpler way of understanding it than by trying, as children often do, to see "faces in the fire," and to carefully watch the way in which they are first caught. Let us call to mind at the same time the experience of past illnesses, when the listless gaze wandered over the patterns on the wall-paper and the shadows of the bed-curtains, and slowly evoked faces and figures that were not easily laid again. The process of making the faces is so rapid in health that it is difficult to analyse it without the recollection of what took place more slowly when we were weakened by illness. The first essential element in their construction is, I believe, the smallness of the area covered by the glance at any instant, so that the eye has to travel over a long track before it has visited every part of the object towards which the attention is generally directed. It is as with a plough, that must travel many miles before the whole of a small field can be tilled, but with this important difference—the plough travels methodically up and down in parallel furrows, the eye wanders in devious curves, with abrupt bends, and the direction of its course at any instant depends on four causes: on the easiest sequence of muscular motion, speaking in a general sense, on idiosyncrasy, on the mood, and on the associations current at the moment. The effect of idiosyncrasy is excellently illustrated by the "Number forms," where we saw that a very special sharply defined track of mental vision was preferred by each individual who sees them. The influence of the mood of the moment is shown in the curves that characterise the various emotions, as the lank drooping lines of grief, which make the weeping willow so fit an emblem of it. In constructing fire-faces it seems to me that the eye in its wanderings tends to follow a

favourite course, and it especially dwells upon the marks that happen to coincide with that course. It feels its way, easily diverted by associations based on what has just been noticed, until at last, by the unconscious practice of a system of "trial and error," it hits upon a track that will suit—one that is easily run over and that strings together accidental marks in a way that happens to form a naturally connected picture. The fancy picture is then dwelt upon, all that is incongruous with it becomes disregarded, while all deficiencies in it are supplied by the fantasy. These latest stages might be represented by a diorama. Three lanterns would converge on the same screen. The first throws an image of what the imagination will discard, the second of that which it will retain, the third of that which it will supply. Turn on the first and second, and the picture on the screen will be identical with that which fell on the retina. Shut off the first and turn on the third, and the picture will be identical with the illusion.

Visions, like dreams, are often mere patchworks built up of bits of recollections. The following is one of these:—

"When passing a shop in Tottenham Court Road, I went in to order a Dutch cheese, and the proprietor (a bullet-headed man whom I had never seen before) rolled a cheese on the marble slab of his counter, asking me if that one would do. I answered 'Yes,' left the shop, and thought no more of the incident. The following evening, on closing my eyes, I saw a head detached from the body rolling about slightly on a white surface. I recognised the face, but could not remember where I had seen it, and it was only after thinking about it for some time that I identified it as that of the cheesemonger who had sold me the cheese on the previous day. I may mention that I have often seen the man since, and that I found the vision I saw was exactly like him, although if I had been asked to describe the man before I saw the vision I should have been unable to do so."

Recollections need not be combined like mosaic work; they may be blended, on the principle I described two years ago, of making composite portraits. I showed that if two lanterns were converged upon the same screen, and the portrait of one person was put into one, and that of another person into the other, the portraits being taken under similar aspects and similar lights, then on adjusting the two images eye to eye and mouth to mouth, and so superposing them as exactly as the conditions admit, a new face will spring into existence. It will have a striking appearance of individuality, and will bear a family likeness to each of its constituents. I also showed that these composite portraits admitted of being made photographically from a large number of components. I suspect that the phantasmagoria may be due to blended memories; the number of possible combinations would be practically endless, and each combination would give a new face. There would thus be no limit to the dies in the coinage of the brain.

I have found that the peculiarities of visualisation, such as the

tendency to see Number-forms, and the still rarer tendency to associate colour with sound, are strongly hereditary, and I should infer, what facts seem to confirm, that the tendency to be a seer of visions is equally so. Under these circumstances we should expect that it would be equally developed in different races, and that a large natural gift of the visionary faculty might become characteristic not only of certain families, as among the second-sight seers of Scotland, but of certain races, as that of the Gipsies.

It happens that the mere acts of fasting, of want of sleep, and of solitary musing, are severally conducive to visions. I have myself been told of cases in which persons accidentally long deprived of food became subject to them. One was of a pleasure-party driven out to sea, and not being able to reach the coast till nightfall, at a place where they got shelter but nothing to eat. They were mentally at ease and conscious of safety, but they were all troubled with visions, half dreams, and half hallucinations. The cases of visions following protracted wakefulness are well known, and I also have collected a few. As regards the maddening effect of solitariness, it may be sufficiently inferred from the recognised advantages of social amusements in the treatment of the insane. It follows that the spiritual discipline undergone for purposes of self-control and self-mortification has also the incidental effect of producing visions. It is to be expected that these should often bear a close relation to the prevalent subjects of thought, and although they may be really no more than the products of one portion of the brain, which another portion of the same brain is engaged in contemplating, they often, through error, receive a religious sanction. This is notably the case among half-civilised races.

The number of great men who have been once, twice, or more frequently subject to hallucinations is considerable. A list, to which it would be easy to make large additions, is given by Brierre de Boismont ('Hallucinations, &c.' 1862), from whom I translate the following account of the star of the first Napoleon, which he heard, second-hand, from General Rapp:—

"In 1806 General Rapp, on his return from the siege of Dantzic, having occasion to speak to the Emperor, entered his study without being announced. He found him so absorbed that his entry was unperceived. The General, seeing the Emperor continue motionless, thought he might be ill, and purposely made a noise. Napoleon immediately roused himself, and without any preamble, seizing Rapp by the arm, said to him, pointing to the sky, 'Look there, up there.' The General remained silent, but on being asked a second time, he answered that he perceived nothing. 'What!' replied the Emperor, 'you do not see it? It is my star, it is before you, brilliant;' then animating by degrees, he cried out, 'it has never abandoned me, I see it on all great occasions, it commands me to go forward, and it is a constant sign of good fortune to me.'"

It appears that stars of this kind, so frequently spoken of in

history, and so well known as a metaphor in language, are a common hallucination of the insane. Brierre de Boismont has a chapter on the stars of great men. I cannot doubt that fantasies of this description were in some cases the basis of that firm belief in astrology which not a few persons of eminence formerly entertained.

The hallucinations of great men may be accounted for in part by their sharing a tendency which we have seen to be not uncommon in the human race, and which, if it happens to be natural to them, is liable to be developed in their over-wrought brains by the isolation of their lives. A man in the position of the first Napoleon could have no intimate associates; a great philosopher who explores ways of thought far ahead of his contemporaries must have an inner world in which he passes long and solitary hours. Great men may be even indebted to touches of madness for their greatness; the ideas by which they are haunted, and to whose pursuit they devote themselves, and by which they rise to eminence, having much in common with the monomania of insanity. Striking instances of great visionaries may be mentioned, who had almost beyond doubt those very nervous seizures with which the tendency to hallucinations is intimately connected. To take a single instance, Socrates, whose *daimon* was an audible not a visual appearance, was, as has been often pointed out, subject to cataleptic seizure, standing all night through in a rigid attitude.

It is remarkable how largely the visionary temperament has manifested itself in certain periods of history and epochs of national life. My interpretation of the matter, to a certain extent, is this—That the visionary tendency is much more common among sane people than is generally suspected. In early life, it seems to be a hard lesson to an imaginative child to distinguish between the real and visionary world. If the fantasies are habitually laughed at and otherwise discouraged, the child soon acquires the power of distinguishing them; any incongruity or nonconformity is quickly noted, the vision is found out and discredited, and is no further attended to. In this way the natural tendency to see them is blunted by repression. Therefore, when popular opinion is of a matter-of-fact kind, the seers of visions keep quiet; they do not like to be thought fanciful or mad, and they hide their experiences, which only come to light through inquiries such as these that I have been making. But let the tide of opinion change and grow favourable to supernaturalism, then the seers of visions come to the front. It is not that a faculty previously non-existent has been suddenly evoked, but that a faculty long smothered in secret has been suddenly allowed freedom to express itself, and it may be to run into extravagance owing to the removal of reasonable safeguards.

[F. G.]

WEEKLY EVENING MEETING,

Friday, June 3, 1881.

THOMAS BOYCOTT, M.D. F.L.S. Vice-President, in the Chair.

PROFESSOR W. G. ADAMS, M.A. F.R S.

Magnetic Disturbance, Auroræ, and Earth Currents.

THE object of establishing a magnetic observatory is to determine at any instant the direction and magnitude of the earth's magnetic force.

The direction of the magnetic force of the earth is the direction in which a small magnetic needle would point when it is freely suspended, so as to turn about an axis passing through its centre of gravity. But it is not easy to suspend a magnetic needle so as to turn freely, and yet to be sure that the axis about which it turns passes accurately through the centre of gravity of the needle; and if it does not so pass, then on suspending the needle we have not only the magnetic force, but also the gravitating force of the earth acting upon it to turn it about its axis, and the position which it takes up shows us the direction of these combined forces upon the magnetic needle.

This direction depends upon the mass of the needle, for to that its weight is due; it depends upon the form of the needle, and the position of its centre of gravity with regard to the axis on which it is hung; it depends also on the magnetic properties of the substance, so that it is not easy to determine even the direction of the magnetic force by a plan which, theoretically, is so very simple.

Instead of attempting to make the required determinations by such a method, it is necessary that a steadier mode of suspension should be adopted, and that may be done as soon as it is discovered in what vertical plane the force of gravity, combined with the earth's magnetic force, will cause such a needle to rest.

This is usually done by loading a steel needle at one end, and then magnetising it with its poles so arranged that the extra weight of the heavier end shall balance the downward pull of the magnetic force on the other end. In this case the needle when magnetised will remain at rest in a horizontal direction when suspended on a point on which it can turn freely in a horizontal plane.

A magnetic needle suspended in this way has been called a declination needle. Such a needle is employed in the mariner's compass, in our galvanometers for measuring currents of electricity, and in magnetic observatories for determining the declination, or what is sometimes called the variation, of the magnetic needle. This

needle determines the position of the vertical plane in which lies the direction of the earth's magnetic force, and which is called the plane of the magnetic meridian. The plane of the *magnetic* meridian is usually different from the vertical plane through the north and south poles, which is called the geographical meridian, or *the* meridian, and the angle between these two planes is the declination or variation of the magnetic needle.

If such a magnetic needle as I have just described be supported on horizontal knife-edges instead of being supported on a point, the needle when magnetised may remain at rest balanced in a horizontal direction, one end being pulled downwards by the earth's vertical magnetic force, and the other by the force of gravity. Any change in the intensity of the vertical magnetic force of the earth will be shown by an up or down motion of the marked end of the needle. Such an instrument, called a balance magnetometer, is specially adapted for showing any changes in the vertical magnetic force of the earth, and any changes or disturbances of the earth's *vertical* magnetic force of which I may speak this evening have been determined by means of such a balance magnetometer. We have then our declination or variation needle to determine the vertical plane called the magnetic meridian, and we have our balance magnetometer to determine any changes which may take place in the vertical magnetic force of the earth.

By the declination needle we can not only determine the plane of the magnetic needle, but by making the needle oscillate to and fro horizontally and counting the number of oscillations in a given time, we can determine the horizontal pull upon the poles of the needle; i. e. the intensity of the earth's horizontal magnetic force upon the needle, just as by the swing of a simple pendulum in a vertical plane under the action of the force of gravity we can determine the pull of the force of gravity upon the bob of the pendulum. By a similar method and by a properly suspended needle, either the vertical force or the total magnetic force of the earth may be determined.

In order then to determine the direction of the earth's magnetic force, we may make use of a declination needle to give us the vertical plane, and place the dipping needle in such a position that it will oscillate in that plane. When it comes to rest it will point in the direction of the total magnetic force, i. e. in the direction through the room of Faraday's lines of magnetic force. In order to determine the magnitude of that force the horizontal force may be found by finding the number of oscillations of the declination needle in the way that I have already explained, and these three determinations will give us the direction and magnitude of the earth's total magnetic force.

Another method of making the required determinations is to take a coil of copper wire, which is wound on a circular frame in such a way as to be capable of spinning on a diameter of the circular frame.

Faraday showed that on turning such a coil in a magnetic field a current of electricity is induced in the coil, and the strength of this current is proportional to the number of lines of force cut by the coil.

We may describe such an arrangement as a magneto-electric machine, in which the magnet employed is the earth itself. By means of this instrument we may determine either the horizontal or the vertical magnetic force of the earth. By placing the axis vertical and spinning the coil at a given rate we may determine the horizontal force, and by placing the axis horizontal in the magnetic meridian and spinning the coil at the same rate we may determine the vertical force, the currents produced in the two cases being in the same ratio as the numbers of the lines of force cut in the two positions.

The greater the angle at which the axis of rotation is inclined to the direction of the lines of force, the greater will be the number of them included in the revolving circle and the greater the induced current produced in the coil.

Thus placing the axis in different positions we get currents of different strengths, and may readily see that we get the greatest current when the axis is at right angles to the direction of the lines of force, i. e. to the line of the dip.

The current produced in each half-turn of a coil of wire revolving on an axis is proportional to the number of lines of force cut by the coil during its rotation, so that the total current in the coil will be proportional to the number of lines of force cut by the coil multiplied by the number of turns of wire in the coil.

When the axis of rotation is in the magnetic meridian, but perpendicular to the lines of magnetic force of the earth, the current in that half of the coil which is moving from west to east will be from north to south, and the current in the other half of the coil which is moving from east to west will be from south to north, so that in the whole coil we get during every half-turn a current in one direction all round the coil.

During the next half-turn we get a current all round the coil in the same direction as looked at from without, i. e. in the opposite direction in the coil of wire. The direction of the current in the coil, as we look at it from the east, is the same as the direction of rotation of the coil as we look at it from the north.

A continuous current may be obtained from the coil by reversing the connections with the ends of the coil by means of a commutator at the same time as the currents are reversed in the coil.

We may further make use of such a coil to find the direction of the lines of force, for if we place the axis parallel to the lines of force, the currents in opposite halves of the coil will balance one another, because each line of force is cut twice by the coil, and so no current is produced in the external circuit through the galvanometer.

If, then, we place the coil so as to get no current when we rotate

it, then the direction of the axis of the coil is the direction of the dipping needle, i. e. of the magnetic lines of force.

We will suppose now that for some point of time, say June 1st at twelve o'clock mid-day, the three magnetic elements, i. e. the declination, the horizontal force, and the vertical force, have been determined, we have now to consider the changes or disturbances produced in these magnetic elements, and the connection of these changes with other phenomena, and especially the connection between auroras, earth currents, and the larger and more irregular magnetic disturbances.

I have already drawn attention to the declination needle and the balance magnetometer for measuring the changes of declination and of the vertical force.

For measurement of the changes in the horizontal force a special instrument is employed, called a bifilar magnetometer, in which a magnet is suspended by two threads which are so placed that by their torsion acting against the horizontal magnetic force of the earth, the magnet is kept at rest in a horizontal position in a direction at right angles to the magnetic meridian. This completes the list of instruments for our magnetic observatory. Any change or disturbance of the horizontal force pulls this magnet round more or less in the horizontal plane, and its change of position is observed as in the other instruments. The results I have to bring before you this evening have been derived from the photographic registrations of similar instruments in different parts of the world, so that the motion of the needle has recorded its own tale on the prepared paper which is wrapped on a cylinder driven by clockwork, and so placed as to receive the spot of light reflected by the moving magnetic needle.

First, there are regular daily and yearly changes, showing that the sun produces regular changes in the three magnetic elements, which depend on the time of the day and the season of the year, so that the change of position and apparent motion of the sun with respect to the place of observation produce regular magnetic changes. These regular daily changes are accompanied by and have very generally been supposed to be due to electric currents or electric waves traversing the earth's crust, and a discussion by Dr. Lloyd of the observations made by Mr. Barlow in 1847 of currents on telegraph wires showed a very close relationship between the two-hourly changes of the declination of the needle and the changes of intensity and direction of earth currents on telegraph lines.

Both Dr. Lamont and Dr. Lloyd conclude, from their comparisons of earth currents and magnetic changes, that the changes of the declination needle cannot be due to the direct action of the electric current traversing the earth's crust, but that these currents or waves, extending to a considerable depth, alter by induction the magnetism of the earth itself, and this change of magnetism causes the observed changes in the declination needle. Thus the magnetic changes are the indirect effects of (not the earth current in its immediate neighbourhood but of) a change in the magnetism of the earth itself,

which may be due to an electric wave extending over a considerable area of the earth's surface. The point towards which the total earth current is directed follows the sun and seems to lag two or three hours behind, but not the same distance behind at different places.

These earth currents have been ascribed to different causes: thus Dr. Lamont regards them as the results of electric force emanating from the sun; De Saussure regards them as developed by evaporation, the vapour being positively charged and the water being negative; Dr. Lloyd regards them as effects of solar heat; whilst M. de la Rive ascribes them to chemical actions going on in the interior of the solid crust of the earth, the electricity being transported into the atmosphere by evaporation. Mr. Ellis, of the Greenwich Observatory, has shown the intimate relation between solar action and the regular diurnal magnetic changes of declination and horizontal force at Greenwich Observatory during thirty-five years, from 1841 to 1876, by a comparison of the observations of those elements. The results of his observations are shown on a large diagram which has been enlarged from his curves (published in the Phil. Trans. part ii. 1880, p. 541), and they show what a close relationship exists between solar activity and terrestrial magnetic changes. There are not only daily and yearly periods of the variations of the different magnetic elements, but there also seems to be in the horizontal intensity a period of twenty-five or twenty-six days, which is the time of rotation of the sun on his axis.

Other recent investigations have shown that these regular magnetic changes depend not only on the sun, but that they are also in part due to the action of the moon, and these portions depend upon the length of the lunar day and on the position of the moon with regard to the earth. Just as there are regular earth currents whose direction depends upon the sun, which we may call the solar earth-currents, so there are lunar earth-currents, which go through their changes under the action of the moon, and it has been shown that the effects are produced not immediately under the moon, but there is a lagging behind in the case of the lunar earth-currents just as in the case of solar earth-currents. In the case of the lunar earth-currents we cannot attribute the production of the electricity either to heat or to thermo-electric currents from one part to another of the earth's crust, and we must therefore look for some other source. May we not find it in the fact that the moon causes tides in the solid crust of the earth just as she causes tides in the oceans? The earth's crust is made up of elastic materials and materials capable of yielding and altering their form to a considerable amount with the change in the direction of the pull of the moon upon them. This crust also contains magnetic substances in abundance, which alter their form under the moon's attraction, and so from the changes of position of masses of magnetic matter changes are produced in the magnetism of the earth, which must give rise to induced currents of electricity or earth currents. Let us imagine a conductor of electricity outside the earth

stretching from the North Pole to the equator and fixed in space, with the earth, a magnetic body, revolving beneath it from west to east; then it follows, from Faraday's laws of induced currents, that the revolution of the earth on its axis would cause a current in the fixed conductor in a direction from the pole to the equator.

If the conductor moved over the surface of the earth from west to east, and the earth did not revolve, or revolved at a slower rate, then the current in the conductor would be from the equator to the pole. The current depends upon the relative motion of the earth and the wire.

If, then, we have an insulated wire running north and south, the tides in the earth's crust of which I have spoken will be equivalent to a lagging behind of magnetic matter, and so we may expect in that wire a current of electricity whose general direction would be from the equator to the pole. The position of the wire with reference to the magnetic pole of the earth would modify the direction of these earth currents, and it is quite conceivable that the position of England with regard to the magnetic pole might cause these regular earth currents to be greatest in the south-west and north-east direction. The lagging of the lunar earth-currents behind the position of the moon would also be accounted for by the lagging of the tides behind the moon.

If this is a true cause for some portion at least of the lunar earth-currents, then the same reasoning applied to the sun may in a smaller degree apply to the case of the regular solar diurnal earth currents, and may help to account for the lagging behind of the effects due to the sun, so that the fact that the greatest solar effect happens about 2.30 P.M. may not be entirely due to the fact that that is the hottest part of the day, but may also in part depend upon the tides.

We have now to consider those more sudden changes of the suspended magnets which are distinguished by the name magnetic disturbances.

In 1874, Dr. Lloyd said of them:—"The duration and the magnitude of these oscillations are as yet outside the domain of law, and probably depend upon so many operating causes that, like the gusts and lulls of the wind in an atmospheric storm, they will long baffle all attempts to refer them to the actuating forces, or even to reduce them to order."

Certain facts relating to these disturbances have long been known. From the series of observations started by Gauss in 1834, and made every five minutes at the same times at a variety of places at first in Europe, and afterwards in various parts of the world, the disturbing power was found to increase in northern latitudes, also it was made out that the appearance of a disturbance in several places occurred at the same time, but there were great differences in the results at different places.

In Europe the agreement was very close, and also in America, but the agreement between Europe and America was not so satisfactory.

The force seemed to originate in a certain point in the interior of

the earth, and the direction of the disturbing force seemed to be constant, yet sometimes there were great differences in the deviations at places not far apart, and from the result of his observations Weber was led to believe that there was a centre of disturbances which was somewhere in the neighbourhood of St. Petersburg.

However sudden and unconnected single disturbances may seem to be, they still follow certain laws in their occurrence; Sabine found that they had daily and yearly variations from their mean values, and that they had an eleven-year period, which agreed with the eleven-year period of the appearance of spots upon the sun.

Disturbances are more frequent in summer than in winter, and this applies to each hemisphere; and it has been confirmed by various observers that they are also subject to the influence of the moon. Lamont says of these disturbances, their cause is a force which is subject to certain laws, but which does not act constantly; the mean direction and frequency have yet to be discovered.

Observations have shown that the magnetic disturbances and electric currents on the earth are so nearly related to one another, that people naturally look upon the electric currents either in the crust of the earth, or in the atmosphere outside it, as the cause of the magnetic disturbances. These currents in the earth have usually been attributed to changes of temperature, because they also are found to be in some way governed by the sun.

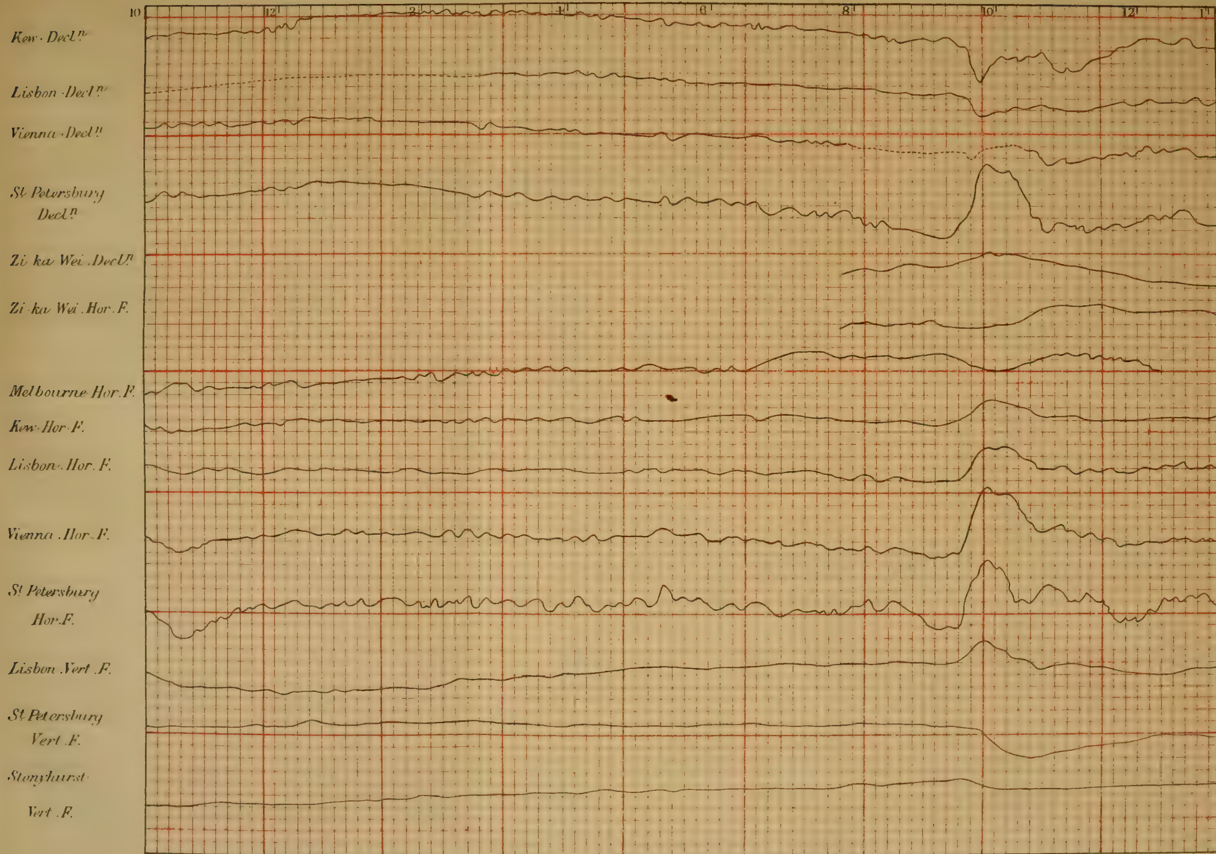
Now let us come to more recent observations of magnetic disturbances, with the improved methods of recording observations by photography which are now available. For some years past photographic records have been taken of the magnetic elements, but the curves have been laid aside, and very little use has been made of them; so much so, that some three or four years ago a circular letter from Mr. Ellery, Director of the Melbourne Observatory, was sent round to those scientific men who were supposed to be interested in the matter, to know whether it was advisable to continue the photographic records of magnetic changes at Melbourne, which is the most southern station, and the only station in the southern hemisphere except Mauritius, where such records are taken. Mr. Ellery did not for one moment suppose that they were of no value, but as no use was made of them he wished to know whether the money expended might not be better applied to another purpose. This matter has been taken up by the Kew Committee, of which Dr. De La Rue is the Chairman, and a recommendation was made that the directors of all observatories which possess instruments of the Kew pattern should be invited to send to Kew their photographic records, or careful tracings of them, for a given period, so that a comparison might be made of the results.

The period chosen was the month of March 1879, and records for the whole month have been sent from Lisbon, Coimbra, Stonyhurst, Vienna, St. Petersburg, and Bombay, in the northern hemisphere, and from Melbourne and the Mauritius in the southern hemisphere.

COMPARISON OF MAGNETIC ELEMENTS.

March 15th. 1879.

Plate I.



A preliminary account of a comparison of the declination curves from the European stations was brought before the British Association last year at Swansea, and this evening I have to bring before you some further points which come out of these comparisons.

It must be remembered that at northern stations the horizontal force is smaller in proportion to the whole force than it is at stations nearer to the equator, so that the same disturbance will produce less effect on the horizontal force or on the declination needle in latitudes near the equator.

Also the needles at different stations are by no means in the same state of sensibility, and even at the same station they change with time, so that they are not always equally sensitive, and when they lose their magnetism they have to be remagnetised.

Let us take the disturbances on March 15–16, 1879 (see Plate 1), which will illustrate some of the points which I wish to bring out prominently.

Not only do magnetic changes occur at the same time at different stations, but there is a great similarity between them.

We see that soon after 10 A.M., Greenwich time, on March 15, 1879, there is a disturbance wave showing first a diminution and then an increase in the horizontal force at St. Petersburg, Vienna, Kew, and Lisbon. At Melbourne, in Australia, there is a similar disturbance at the same time both in the declination and in the horizontal force.

Again between 2 and 3 P.M., and between 4 and 5, there are very small disturbances showing themselves at the same absolute time in the horizontal force and declination curves. About 5.20 P.M. there is a well-marked increase in the horizontal force and eastward deflection of the declination needles.

About 9.30 P.M., Greenwich time, a storm begins which lasts for about an hour. It is felt in the northern and in the southern hemispheres, near to and on both sides of the equator. At all European stations the horizontal force is increased during the first part of the storm and then diminished. At Lisbon the vertical force is first increased and then diminished, and at St. Petersburg and Stonyhurst there is a diminution in the vertical force at the same time as at Lisbon. If we regard the declination needles, we find that at St. Petersburg, Zi-ka-Wei and Melbourne, and at Bombay the declination westward is first increased and then diminished, whereas at Kew and Lisbon the motions are in the opposite direction. The declination at Vienna seems to be intermediate between Kew and St. Petersburg, but the curve is incomplete. At Bombay and the Mauritius, near to, but on opposite sides of the equator, the declination needles are deflected opposite ways. The local time at these places was from 1 to 2 o'clock at night. Now in what way can we account for such magnetic disturbances as these? If we assume that by magnetic induction from some cause or other the earth's magnetism is altered, then the position of the magnet which would produce the disturbance must be such that

its pole which attracts the marked end of our needle must lie at the beginning of the disturbance to the east of Kew and Lisbon, to the north of Vienna, and to the north-west of St. Petersburg; the Lisbon vertical force curve also shows it to be below the surface of the earth. Hence an inductive action equivalent to a change of position of the north magnetic pole towards the geographical pole would account for these changes. The strengthening and weakening of a magnet with its north pole to the north on the meridian of Vienna might possibly account for the magnetic changes observed between 9.30 and 10.30 at night, Greenwich time, on March 15, 1879. If we attempt to explain this disturbance by currents of electricity or discharges of statical electricity in the air above the needles, then we must imagine that at first there is a strong current from the south-west over St. Petersburg, from the west over Vienna, and from the north-west over Kew and Lisbon, the vertical force needle at Lisbon showing that the current from the north-west lies somewhat to the east of Lisbon; that at the Mauritius this current is from the north, and at Bombay from the south.

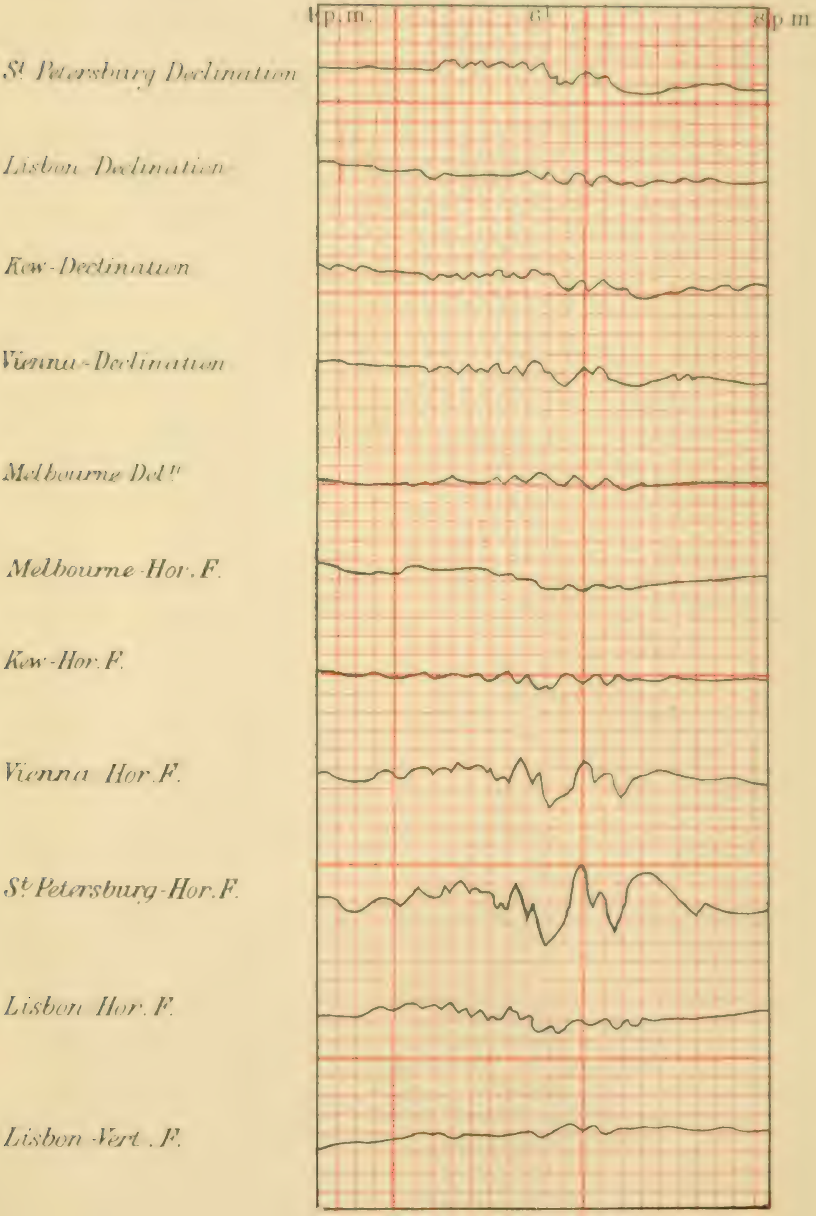
Hence we must imagine that a current of electricity passes down from the north-west to the south-east, going on towards the east over Vienna and towards the north-east over St. Petersburg. This must be kept up very much along the same line throughout the first part of the disturbance, and then the current or currents must be altered in strength in the same manner at all stations.

We will next consider what would hardly be called a magnetic storm, but a few very small deviations of the magnetic needle, lasting from about 5.30 to 7.30 P.M. on March 26, 1879 (see Plate 2). Only the comparison of the originals will give the closeness of the similarity of the curves, and the curves for Vienna and Kew are coincident.

When the declination needle is deflected to the west, the horizontal force needle is deflected with its marked end towards the south, so that in this disturbance the two needles are drawn towards the south-west at the same time with greater or less power, and twelve similar curves are clearly traced out in the Vienna and Kew curves during the two hours. From the remarkable similarity in these disturbances and their occurrence at the same time, we should expect that the cause of disturbance is so far removed from the places of observation that the difference of their distances from it need not be considered. This might not unreasonably be urged as an argument in support of a theory that such disturbances are due directly to the action of the sun regarded as a magnetic body. These disturbances are all so very small, that but for the comparison of photographs they would probably be lost sight of, yet we see that the same deflections occur at the same instant at Kew and at Vienna, at St. Petersburg and at Melbourne. The numerical comparisons of observations made every five minutes on certain days previously fixed upon would probably never have shown the way in which these minute changes of magnetic

COMPARISON OF MAGNETIC ELEMENTS.

March 26th. 1879. Plate 2.



power of the earth at widely distant places are related to one another. In one or two cases Señor Capello and Professor Balfour Stewart had compared the Lisbon and Kew curves for a particular disturbance, but the photographic magnetic records have never before been collected from other stations, and there has been no opportunity of comparing them. From the precise similarity of the forms of the curves in many cases we may say that the *rate of change* of magnetic disturbances at widely distant stations is the same. There is nothing fitful or flashing in such disturbances as these of March 26th. We might imagine a current in the crust of the earth, or a current or transfer of electricity in the air, near to, i.e. within 20 or 30 miles of each of these observatories, but to imagine the same current and the same variations of the current at so many different stations all changing in the same way at the same instant is difficult, unless it can be shown in what way all these changes are connected with the cause of such a regular electric discharge. It seems easier to imagine that such changes as these are due to a change produced by induction in the magnetism of the earth itself by some distant body. It is easy to show that the magnetism produced by a current in a magnetic substance round which it flows is greater in its action on a small magnetic needle than the direct action of the current itself. Hence a current flowing in the crust of the earth should produce its principal effect on a magnetic needle by the magnetic induction which the current induces in the earth itself.

Sometimes disturbances occur where at the same instant there are similar deflections of the declination needles at stations wide apart, and suddenly at one of the stations the needle no longer goes with the others, but is deflected for a considerable period in the opposite direction to the others, turning when they turn, and tracing out a similar curve, but turned always in the opposite direction. Such cases occurred frequently during March 1879, and especially on March 23rd, about 1.30 and about 7 P.M., Kew time, and on March 29th about 9 P.M.

An examination of the principal disturbances at Kew and at St. Petersburg seems to show that:—

(1) A diminution in the horizontal force is accompanied by greater easterly deflections of the declination needle at St. Petersburg than at Kew.

(2) Increase of the horizontal force is accompanied by greater westerly deflections at St. Petersburg than at Kew, or is sometimes accompanied by a westerly deflection at St. Petersburg and an easterly deflection at Kew.

These cases which I have taken will be sufficient to show how important it is that there should be additional magnetic observations, especially in the southern hemisphere, where photographic records should be taken, so that we may learn something about the magnetism of the earth. Practically we have to rely on one excellent observatory (Melbourne) for the whole of the southern hemisphere. Surely the

time has arrived when there should be photographic registration of the magnetic elements at such an important observatory as the Cape of Good Hope, especially when the French Government has decided, within the last few weeks, to establish a magnetic observatory at Cape Horn. With observatories at Melbourne, at Cape Horn, and at the Cape of Good Hope the southern hemisphere would be well supplied; and probably the Russian Government would then soon establish an observatory in the east of Siberia, where it is very desirable that a magnetic observatory should be established.

Now we can readily show by experiment the way in which the magnetic instruments may be disturbed in a magnetic observatory by the alteration of the strength of a magnet. Taking magnetic needles to represent the declination needle, the inclination needle, and the bifilar or horizontal force needle, we may place an electro-magnet in a given position with regard to them, and by altering the strength of that electro-magnet may cause these needles to trace out disturbances of a very decided character.

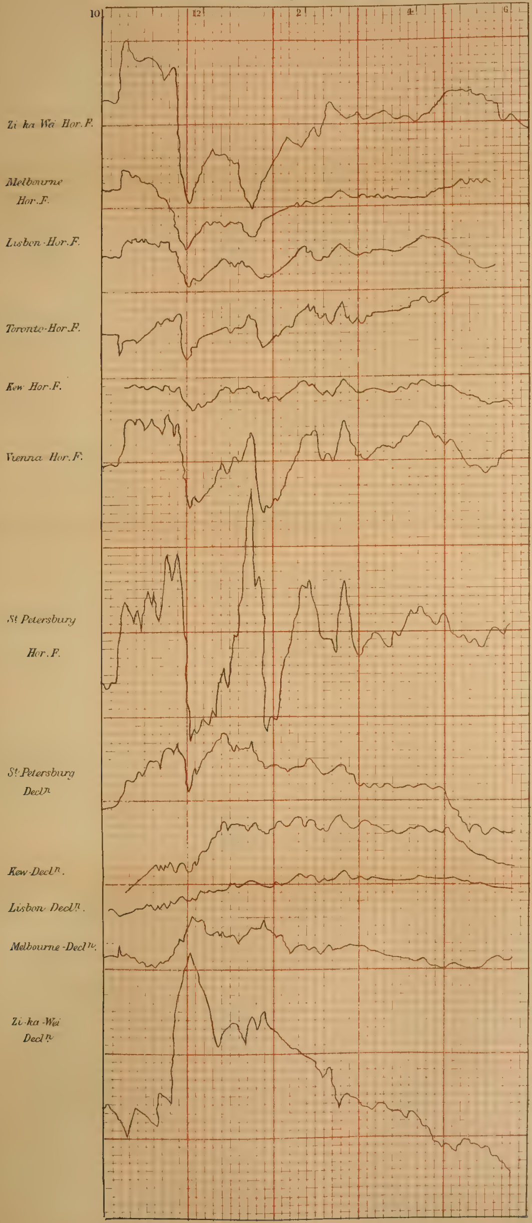
I have as yet been speaking of only moderate disturbances, but now let us come to some of the larger ones; and I have had the opportunity through the kindness of the Kew Committee and the observers at the various observatories mentioned, of studying the curves for the August magnetic storm, which began at 10.20 A.M., Greenwich time, on August 11th, and for convenience may be divided into three storms, one lasting from 10.20 on the 11th to 1 A.M. on the 12th; a second from 11.30 A.M. on the 12th to 7.20 on the 13th, and the third from 11.50 A.M. on the 13th to 7 to 8 A.M. on the 14th of August. I have prepared a large sheet on which these curves have been copied as accurately as possible for the first of these storms on the 11th (see Plate 3). For this storm I have also the curves from Toronto and from Zi-Ka-Wei. The first storm began on August 11th at the same instant at all the stations. There is a decided similarity, especially in the horizontal force curves, throughout the first part of this storm, and certain points in it stand out prominently. At Kew the beginning of the storm is not actually recorded, because the sheets of prepared paper on the time cylinders were changed precisely at 10.20 A.M., when the storm was beginning. The deflections are alike at Lisbon, Kew, Vienna, St. Petersburg, and, after the very first sudden deflection, at Toronto also. The greatest effect is produced at St. Petersburg; the similarity between the large disturbances at Vienna and at Toronto, in Canada, places differing about $6\frac{1}{2}$ hours in time, is remarkable. About 11.45, 1 P.M., and 2.40 P.M. there are very remarkable points of agreement.

From about 4.30 P.M. to 8 P.M., Greenwich time, i.e. from about 11 A.M. to 2.30 P.M., Toronto time, the deflections are opposed at Toronto and at Vienna or Kew. This would rather point to solar action as the cause of disturbance. In this case the Kew curve is not so much deflected as the Vienna curve, because the horizontal needle at Kew is not nearly so sensitive as at Vienna, and the relative

Plate 3.

COMPARISON OF MAGNETIC ELEMENTS.

10 a.m to 6 p.m Aug 11th 1880.



strengths of the actual disturbing forces at the two places can only be obtained by comparison of the scale values at the two places.

I will draw your attention to one other point on this day. At 9 P.M. the disturbances are all in the same direction, but about 11 P.M., whilst St. Petersburg agrees in direction with the others in a very violent phase of the storm, at Toronto the direction of the deflections is reversed, and this reversal of curves continues until about the end of this first of the three storms.

The second storm, beginning about 11.30 A.M. on the 12th and lasting until the next morning, was the most remarkable of the three. It not only baffles the telegraph clerks, who wish to keep out earth currents from their lines, but it even goes beyond the powers of the magnetic observatories which are specially designed to watch over them. Thus at Toronto the line goes off the edge of the paper on which the photographic record is taken. At Melbourne the motion is so rapid, and also at Vienna, that the plate is not sensitive enough to receive the impressions, the motion is too quick even for photography. At the time of greatest disturbance, about 12.20 mid-day, it is very remarkable that at Lisbon and at Zi-ka-Wei, near Shanghai, in China, two places nearly in the same latitude but nearly 9 hours apart in time, the vertical force is increased in precisely the same way and to the same amount at the same instant.

At Zi-ka-Wei, in China, the sudden change in the horizontal force on the needle amounted to about one hundredth part of the total horizontal force, and at St. Petersburg the change in the horizontal force amounted to one thirty-fifth part of the horizontal force, and the total force was changed by about one eightieth part of its full value.

These magnetic changes are so large as to be quite comparable, as we see, with the earth's total force, so that any cause which is shown to be incompetent from the nature of things to produce the one can hardly be held to account for the other.

Since, as I have shown, the large disturbances and the small disturbances do not follow totally different laws but agree equally well all over the earth, in so far as they agree we must attribute them to the same cause.

During this August storm, as also during the remarkable storm of January 31st last, great difficulties were experienced in working the telegraph lines, and Mr. Preece has been kind enough to send me particulars of these storms.

I am also greatly indebted to the Astronomer Royal for sending me tracings of the earth current photographic records taken at Greenwich Observatory during the August storm on two separate wires, one running from the north-east and the other from the south-east to Greenwich. The two tracings are bent opposite ways at the same time, so that when a current was running on one line towards Greenwich, on the other it was running away from it, and comparing these curves with the earth-current records from Derby and Haverfordwest and other places, it appears that the general direction

of currents during this storm was from south-west to north-east, or from south-south-west to north-north-east, with varying intensity, the agreement being very close between the disturbances of the declination needle and the Blackheath and Greenwich photographic record. From Mr. Preece's record also earth currents were violent from 10.30 A.M. on the 11th (i. e. they were noted within ten minutes of the beginning of the magnetic storm) to about 2.30 P.M., and again from 9 to midnight. They were very violent on August 12th, beginning at 11.30 A.M., the beginning of the second storm, and quieting down about 4.30 P.M., then beginning at 7.30 and lasting until 9.30 P.M.

Again on the 13th, they are strong for $1\frac{1}{2}$ hour from about 5 in the morning, i. e. just about the end of the second magnetic storm.

The general direction of the earth currents as observed at Derby or Haverfordwest, as well as at Greenwich, was from north-east to south-west.

Again on January 31st last another violent magnetic storm, in which, Mr. Preece tells me, the currents were even more violent than in the August storm.

Intimately connected with magnetic disturbances and earth currents is the phenomenon of the aurora or polar light, which is an electric discharge in the upper regions of the atmosphere. During the August and January storms the aurora was well seen in England, it was also seen at St. Petersburg and as far east as Siberia. It does not appear to have been seen, although it was looked for, at Zi-ka-wei, in China, by M. Dechevrens, the Director of the Observatory, although the magnetic storm was so violent there that the horizontal force was suddenly changed by one hundredth part of its total amount.

We may arrive at some idea of the character of the aurora by studying electric discharges in vacuum tubes, and Dr. De La Rue has already brought this subject before you in his Friday evening lecture.

We may gradually pass from electric discharges in air of ordinary density, in which we get the well-known electric spark between two surfaces, to air of less density but better conducting power, and then to air of less density still, but of such high resistance that no electricity will pass. Dr. De La Rue has shown that with 11,000 cells of his battery the striking distance between two points is about six-tenths of an inch in air of ordinary density of about 760 mm. pressure. When the pressure in a hydrogen tube is reduced to 21·7 mm., 8937 cells will cause a discharge to take place through 30 inches. When the pressure is reduced to ·642, about six-tenths of a mm., 430 cells will cause a discharge through the tube. When the pressure is still further reduced to ·0065, it requires 8937 cells to cause the discharge. So that the spark passes more readily at a pressure ·642 mm. than it does at a higher or a lower pressure.

This is also the case with air.

The lower regions of the earth's atmosphere offer great resistance to the passage of electricity, but as we ascend the pressure diminishes, and the electric resistance diminishes, until at last, at a height of between 30 and 40 miles, a level is reached where the air offers least resistance to the passage of electricity, where the pressure is about ·397 of a mm.; and above that level the electrical resistance again increases, so that at a height of about 80 miles the battery of 11,000 cells would not cause a spark to pass.

If we take a tube which has not been very highly exhausted, we see that the light from the positive pole extends nearly through the tube, and the dark space around the negative pole is small. As the exhaustion proceeds and the pressure of the air is diminished, the electric spark passes through greater and greater lengths and changes its character, until we get to the pressure corresponding to the least resistance. Beyond that the resistance increases, the dark space around the negative pole expands, and the molecules fly about more freely; those on the negative pole being charged with electricity, and being repelled from it, proceed for a long distance in straight lines, and possess the power of causing bodies on which they strike to glow. In Mr. Crookes's tubes we get very beautiful effects from this glowing of the glass tube itself, or from the glowing of substances in the path of the stream. We may regard this as a stream of molecules of gas charged with electricity, and we see the difference between this stream and the electric current in a vacuum tube at lower exhaustion by the action of the magnet upon it. In one case the current going through the molecules from pole to pole in the tube is bent out of its course by the magnet, and symmetrically by the two poles, and returns to its path, the line of least resistance through the molecules; whereas the stream of molecules at the higher exhaustion carrying their electricity with them, are carried away by the electric charge upon them, and get utterly lost and scattered on striking the side of the tube, yielding up a great deal of energy in the form of heat to the tube, or to the glowing platinum or other substance in the tube.

The aurora, as seen in the north-eastern parts of Siberia, where it is very often very brilliant, is described as consisting of single bright pillars rising in the north and in the north-east, gradually covering a large space of the heavens. These rush about from place to place, and, reaching up to the zenith, produce an appearance as if a vast tent was spread in the heavens, glittering with gold, rubies, and sapphires.

More exact attempts have been made to describe the aurora, and perhaps I may be allowed to quote Dalton's description of an aurora as seen by him.

A remarkable red appearance of clouds was noticed in the southern horizon, which afforded light enough to read by, and a remarkable effect was expected. He says:—"There was a large luminous horizontal arch to the southward, and one or more concentric arches

northward. All the arches seemed exactly bisected by the plane of the magnetic meridian. At 10.30 streamers appeared in the south-east, running to and fro from west to east. They increased in number and approached the zenith, when all of a sudden the whole hemisphere was covered with them, and exhibited such an appearance as baffles all description. The intensity of the light, the prodigious number and volatility of the beams, the grand intermixture of all the primitive colours in their utmost splendour, variegating the glowing canopy with the most luxuriant and enchanting scenery, afforded an awful, but at the same time a most pleasing and sublime, spectacle." But he adds:—"The uncommon grandeur of the scene only lasted one minute; the variety of colours disappeared, and the beams lost their lateral motion, and were converted, as usual, into the flashing radiations; but even then it surpassed all other appearances of the aurora, in that the whole hemisphere was covered with it."

In his address before the British Association in 1863, Sir William Armstrong speaks of the sympathy between forces operating in the sun and magnetic forces on the earth, and notices a remarkable phenomenon seen by independent observers on September 1, 1859:—"A sudden outburst of light, far exceeding the brightness of the sun's surface, was seen to take place, and sweep like a drifting cloud over a portion of the solar surface. This was attended with magnetic disturbances of unusual intensity and with exhibitions of aurora of extraordinary brilliancy. The identical instant at which the effusion of light was observed was recorded by an abrupt and strongly marked deflection in the self-registering instruments at Kew. The magnetic storm commenced before and continued after the event."

The daily and yearly periods of the magnetic changes, the change in the horizontal force depending on the sun's rotation on his axis, the agreement of the eleven-year period of magnetic disturbances, sun-spots, and auroras show that the sun plays a very important part in causing or regulating both the regular and irregular magnetic changes.

The sun may be a very powerful magnet, and when his magnetism is greatly altered we may see the effects of this disturbance in the bright faculæ and in the spots in his atmosphere. Such a change of magnetism would affect the magnetism of the earth, although the effect could not be very large, unless the sun is magnetised to an intensity much greater even compared to his mass than the earth is magnetised. Then, as there are tides in the seas around us, and probably in the earth's crust, so there are certainly very large tides in the ocean of air above us, and may not the sun and moon by dragging this air towards them as the earth revolves cause that friction between air and the earth, and also that evaporation, which together may account for the presence of and keep up the supply of positive electricity in the air and negative electricity in the earth? Again, these tides in the atmosphere will cause the mass of it to lag behind the revolving solid earth, and at a height of 30 or 40 miles we

have a layer of air which for air is a comparatively good conductor of electricity; here, then, we have not a lagging of the magnet behind the conductor (as described in the early part of my lecture), but a lagging of the conductor behind the magnet, and hence we may expect a current or a gradual heaping up of electricity in the air in the opposite direction to the current in the earth's crust. Thus whilst the tidal wave in the earth's crust would cause a current in a telegraph wire from the equator towards the poles, the regular tidal waves in the atmosphere would cause the gradual transfer of positive electricity from the poles towards the equator. This transfer may be of the nature of a current of electricity or of a mass of air carrying a static charge of electricity with it, for as Professor Rowland has shown that the motion of a static charge will produce magnetism, so we may expect from the principles of conservation of electricity that a change in the position of a magnet will, under such circumstances, produce motion of the static charge of electricity. When the air becomes charged up to discharging point, then we may get the sudden discharges such as the aurora in the air and the earth currents in the earth, and since the conducting layer of air approaches nearer to the earth in the colder polar regions, possibly within less than 20 miles of the earth's surface, it may be found that the discharge of the aurora may even take place from earth to air by gradual slow discharge, aided as it may be by the state of moisture of the air and by change of temperature and other causes.

[W. G. A.]

GENERAL MONTHLY MEETING,

Monday, June 6, 1881.

George Busk, Esq. F.R.S. Treasurer and Vice-President, in the Chair.

C. J. S. Spedding, Esq.
Mrs. Katharine Maria White,
Captain Henry Tryon Wing,

were elected Members of the Royal Institution.

The Special Thanks of the Members were voted to Mr. William Ladd for his valuable present of a Dynamo-Magneto-Electric Machine and Platinum Wire Stand.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

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WEEKLY EVENING MEETING,

Friday, June 10, 1881.

William Bowman, Esq. LL.D. F.R.S. Vice-President, in the Chair.

James Dewar, Esq. M.A. F.R.S.

FULLERIAN PROFESSOR OF CHEMISTRY AT THE ROYAL INSTITUTION, AND JACKSONIAN IN THE UNIVERSITY OF CAMBRIDGE.

Origin and Identity of Spectra.

On a former occasion I detailed the results of a joint research made in concert with my esteemed colleague Professor Liveing, on the "Reversibility of the Rays of Metallic Vapours."* The present lecture will be devoted to a record of the results of our work in relation to three disputed questions in spectroscopic investigation, viz. (1) the Carbon Spectrum, (2) the Magnesium Spectrum, and (3) the Identity of the Spectral Lines of different Elements.

Spectrum of Carbon Compounds.

The spectrum of the flame of hydrocarbons burning in air has been repeatedly described, first by Swan in 1856, and afterwards by Attfield, Watts, Morren, Plücker, Huggins, Boisbaudran, Piazzi Smyth, and others. The characteristic part of this spectrum consists of four groups of bands of fine lines in the orange, yellow, green, and blue respectively, which are hereafter referred to as the hydrocarbon bands. These four groups, according to Plücker and Hittorf, also constitute the spectrum of the discharge of an induction coil in an atmosphere of hydrogen between carbon electrodes. They are also conspicuous in the electric discharge in olefiant gas at the atmospheric and at reduced pressures.

Plücker and Hittorf notice the entire absence in the flame of olefiant gas of the two bright groups of lines (blue and violet as described below) characteristic of the flame of cyanogen.

Several observers have described the spectrum of the flame of burning cyanogen. Faraday, as long ago as 1829, called the attention of Herschel and Fox Talbot to it, and the latter, writing of his observations,† points out as a peculiarity that the violet end of the spectrum is divided into three portions with broad dark intervals, and that one of the bright portions is ultra-violet. More recently Dibbits, Morren, Plücker, and Hittorf have particularly described this spectrum. Dibbits ‡ mentions in the cyanogen flame fed with oxygen a series of

* 'Proceedings of the Royal Institution,' vol. ix. p. 204.

† 'Phil. Mag.' ser. iii. vol. iv. p. 114.

‡ 'Pogg. Ann.' 1864.

orange and red bands shaded on the less refrangible side (i. e. in the opposite way to the hydrocarbon bands), the four hydrocarbon bands more or less developed, a group of seven blue lines, a group of two or three faint blue (indigo) lines, then a group of six violet lines, and, lastly, a group of four ultra-violet lines. When the cyanogen is burnt in air, the hydrocarbon bands are less developed, and the three faint indigo lines are scarcely visible, but the rest of the spectrum is the same, only less brilliant.

Plücker and Hittorf* state that in the flame of cyanogen burning in air under favourable circumstances, the orange and yellow groups of lines characteristic of burning hydrocarbons are not seen, the brightest line of the green group appears faintly, the blue group is scarcely indicated; but a group of seven fluted bands in the blue, three in the indigo, and seven more in the violet, are well developed, especially the last. When the flame was fed with oxygen instead of air, they state that an ultra-violet group of three fluted bands appeared. They notice also certain red bands with shading in the reverse direction, which are better seen when the flame is fed with air than with oxygen. Other observers give similar accounts, noticing the brilliance of the two series of bands in the blue and violet above mentioned, and that they are seen equally well in the electric discharge through cyanogen.

Angström and Thalén, in a memoir "On the Spectra of Metalloids,"† contend that the channelled spectra of the hydrocarbon and cyanogen flames are the spectra respectively of acetylene and cyanogen, and not of carbon itself, and that in the flame of burning cyanogen we sometimes see the spectrum of the hydrocarbon superposed on that of the cyanogen, the latter being the brighter; and that in vacuum tubes containing hydrocarbons the cyanogen spectrum observed is due to traces of nitrogen.

No chemist who remembers the extreme sensibility of spectroscopic tests, and the difficulty, reaching almost to impossibility, of removing the last traces of air and moisture from gases, will feel any surprise at the presence of small quantities of either hydrogen or nitrogen in any of the gases experimented on.

Mr. Lockyer ‡ obtained a photograph of the spectrum of the electric arc in an atmosphere of chlorine, which shows the series of fluted bands in the ultra-violet, on the strength of which he throws over the conclusion of Angström and Thalén, and draws inferences regarding the existence of carbon vapour above the chromosphere in the coronal atmosphere of the sun, which, if true, would be contrary to all we know of the properties of carbon.

The conclusions of Angström and Thalén have been much strengthened by the results of a series of observations carried out by Professor Liveing and myself.

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* 'Phil. Trans.' 1865. † 'Nova Acta, Roy. Soc. Upsala,' vol. ix.
‡ 'Proc. Roy. Soc.' vol. xxvii. p. 308.

Electric Arc in different Gases.

The experiments were made with a De Meritens dynamo-electric machine, arranged for high tension, giving an alternating current capable of producing an arc between carbon poles in air of from 8 to 10 millims. in length. The carbon poles used were 3 millims. in diameter, and had been previously purified by prolonged heating in a current of chlorine. This treatment, though it removes a large part of the metallic impurities present in the commercial carbons, will not remove the whole, so that lines of calcium, iron, magnesium, and sodium may still be recognised in the arc. Besides the traces of metallic impurities, a notable quantity of hydrogen always remains unremovable by this treatment with chlorine.

The arc was taken in different gases inside a small glass globe (*aa* in Pl. I. Fig. 1) about 60 millims. in diameter, blown in the middle of a tube. The two ends of the tube (*bb*) were closed with dry corks, through which were passed (1) the carbons (*cc*), inserted through two pieces of narrow glass tubing; (2) two other glass tubes (*dd*) through which currents of the different gases experimented on were passed.

The arc in the globe filled with air gave a tolerably bright continuous spectrum, on which the green and blue hydrocarbon bands were seen, also the seven bands in the indigo (wave-lengths 4600 to 4502, Watts) as in the flame of cyanogen, and much more brightly the six bands in the violet (wave-lengths 4220 to 4158, Watts) and five ultra-violet. Besides these bands, lines of iron, calcium, and sodium were visible. The arc in this case was practically taken in a mixture of nitrogen and carbonic oxide, for in a short time the oxygen of the air is converted into carbonic oxide.

On passing through the globe a current of carbonic acid gas, the bands in the indigo, violet, and ultra-violet gradually died out until they ceased to be visible continuously, and when momentarily seen were only just discernible. On the other hand, the hydrocarbon bands, yellow, green, and blue, came out strong, and were even brilliant. Lines of iron and calcium were still visible. On stopping the current of carbonic acid gas and allowing air to diffuse into the globe, the violet and ultra-violet bands soon began to appear, and presently became permanent and bright, the hydrocarbon bands remaining bright.

When a continuous current of dry hydrogen was passed through the globe, the arc, contrary to what would be expected from the behaviour of the spark discharge in hydrogen, would not pass through more than a very short space, very much less than in air or carbonic acid gas. There was a tolerably bright continuous spectrum, with no trace of bands in the indigo, violet, or ultra-violet, and no metallic lines, with the exception of a fairly bright line in the red, which we identified, by comparison with the spark in a vacuum tube, with the C line of hydrogen. The F line, identified in like manner, was also seen as a faint diffuse band. This last line was in general

overpowered by the continuous spectrum, but was regularly seen when, from some variation in the discharge, the continuous spectrum became less brilliant. This was the first occasion on which we had seen the hydrogen lines in the arc, though Secchi * states that he had seen them by the use of moist carbon poles. The hydrocarbon bands in the green and blue were at intervals well seen. Those in the yellow and orange were, owing doubtless to the smaller dispersion of the light in that region, overpowered by the continuous spectrum. Whereas when air and carbonic acid gas were used, the inside of the globe was quickly covered with dust from the disintegrated poles, scarcely any dust was thrown off when the arc was passed in hydrogen.

In nitrogen a longer arc could be formed, and the indigo, violet, and ultra-violet bands of cyanogen all came out at intervals brilliantly. The green and blue hydrocarbon bands were also well developed.

On filling the globe with chlorine, keeping a current of that gas passing through it, the arc would not pass through a greater distance than about 2 millims. No metallic lines were visible. At first the violet bands, as well as the green and blue hydrocarbon bands, were visible; but gradually, when the current of chlorine had been passing for some minutes, there was nothing to be seen but a continuous spectrum with the green and blue hydrocarbon bands. Neither of these bands were strong, and at intervals the blue bands disappeared altogether.

The arc would not pass in a current of carbonic oxide through any greater space than in chlorine. There was much continuous spectrum; the yellow, green, and blue hydrocarbon bands were well seen, some of the indigo bands were just discernible, the violet had nearly, and the ultra-violet quite, gone from sight. No trace of the carbonic oxide bands, as seen in the spark discharge in that gas, was visible. This is the more remarkable since under similar circumstances two of the characteristic lines of hydrogen were seen.

In nitric oxide a very long arc could be obtained. The violet and ultra-violet cyanogen bands were well seen, the indigo bands were seen, but weaker. The blue and green hydrocarbon bands were also seen well when the arc was short, not so well when the arc was long. Many metallic lines of iron, calcium, and magnesium were seen.

In ammonia only a short arc could be obtained. All the bands were faint, but the indigo and violet and ultra-violet cyanogen bands were always visible.

These experiments with different gases eliminate to a large extent the influence of electric conductivity on the character of the spectrum.

Apart from the relative electric conductivity of gases, it is clear, from the experiments, that the length and character of alternating electric discharges between carbon poles in different gases do not follow the law which we should expect. It will require a pro-

* 'Compt. Rend.' 1873.

longed series of experiments to arrive at definite conclusions on this matter; but, in the meantime, it is highly probable that one of the main factors in producing these remarkable variations in the arc will be found to be the relative facility with which the carbon of the poles combines with the gaseous medium.

On a review of the above series of observations, certain points stand out plainly. In the first place, the indigo, violet, and ultra-violet bands, characteristic of the flame of cyanogen, are conspicuous in the arc taken in an atmosphere of nitrogen, air, nitric oxide, or ammonia, and they disappear almost, if not quite, when the arc is taken in a non-nitrogenous atmosphere of hydrogen, carbonic oxide, carbonic acid, or chlorine. These same bands are seen brightly in the flames of cyanogen and hydrocyanic acid, but are not seen in those of hydrocarbons, carbonic oxide, or carbon disulphide. The conclusion seems irresistible that they belong to cyanogen; and this conclusion does not seem to us at all invalidated by the fact that they are seen weakly, or by flashes, in the arc or spark taken in gases supposed free from nitrogen on account of the extreme difficulty of removing the last traces of air. They are never, in such a case, the principal or prominent part of the spectrum, and in a continuous experiment they are seen to fade out in proportion as the nitrogen is removed. This conclusion is strengthened by the recent discovery that cyanogen is always generated in the electric arc in atmospheric air.

The green and blue bands, characteristic of hydrocarbon flames, are well seen when the arc is taken in hydrogen; but, though less strong when the arc is taken in nitrogen or in chlorine, they seem to be always present in the arc, whatever the atmosphere. This is what we should expect, if they be due, as Angström and Thalén suppose, to acetylene; for we have found that the carbon electrodes always contain, even when they have been long treated with chlorine at a white heat, a notable quantity of hydrogen.

The hydrocarbon bands are well developed in the blowpipe flame, that is, under conditions which appear, at first sight, unfavourable to the existence of acetylene. We have, however, satisfied ourselves, by the use of Deville's aspirator, that acetylene may be withdrawn from the interior of such a flame, and from that part of it which shows the hydrocarbon bands brightly.

The question as to whether these bands are due to carbon itself or to a compound of carbon with hydrogen, has been somewhat simplified by the observations of Watts and others on the spectrum of carbonic oxide. There is, we suppose, no doubt now that that compound has its own spectrum quite distinct from the hydrocarbon flame spectrum. The mere presence of the latter spectrum feebly developed in the electric discharge in compounds of carbon supposed to contain no hydrogen, appears to us to weigh very little against the series of observations which connect this spectrum directly with hydrocarbons.

In the next place, it appears, from experiments, that the development of the violet bands of cyanogen, or the less refrangible

hydrocarbon bands, is not a matter of temperature only. For the appearance of the hydrogen lines C and F in the arc taken in hydrogen indicates a temperature far higher than that of any flame. Yet the violet bands disappear at that temperature, and the green bands are well developed. The violet bands are, nevertheless, seen equally well at the different temperatures of the flame, arc, and spark, provided cyanogen be the compound under observation in the flame, and nitrogen and carbon are present together at the higher temperatures of the arc and spark.

The question of the constitution of comets, since the discovery by Huggins* that the spectra of various comets are identical with the hydrocarbon spectrum, naturally leads to some speculation in connection with the conclusions to which our experiments point. Provided we admit that the materials of the comet contain ready-formed hydrocarbons and that chemical or electrical actions may take place, generating a high temperature, then the acetylene spectrum might be produced at temperatures no higher than that of ordinary flames without any trace of the cyanogen spectrum, or of metallic lines. Such actions might be brought about by the tidal disturbances involving collisions and projections of the constituents of the swarms of small masses circulating in orbits round the sun, which we have every reason to believe constitute the cometic structure. If, on the other hand, we assume only the presence of uncombined carbon and hydrogen, we know that the acetylene spectrum can only be produced at a very high temperature; and if nitrogen were also present, that we should at such a temperature have the cyanogen spectrum as well. Either then the first supposition is the true one, not disproving the presence of nitrogen; or else the atmosphere which the comet meets is hydrogen only and contains no nitrogen.

The Flame of Cyanogen.

The accompanying diagram (Pl. I. Fig. 2) shows the relative position of the bands in that part of the spectrum of the flame of cyanogen fed with a jet of oxygen which is more refrangible than the Fraunhöfer line F. Only those bands which are less refrangible than the solar line L have been before described, but photographs show another set of two shaded bands slightly less refrangible than the solar line N accompanied by a very broad diffuse band of less intensity on the more refrangible side of N; also a strong shaded band, which appears to be absolutely coincident with the remarkable shaded band in the solar spectrum, which has been designated by the letter P; and near this, on the less refrangible side, a much fainter diffuse band, which also seems to coincide with a part of the solar spectrum sensibly less luminous than the parts on either side of it. Watts found that the spectrum cyanogen of the flame did not disappear when the

* 'Proc. Roy. Soc.' vol. xvi. p. 386; vol. xxiii. p. 154; 'Phil. Trans.' 1868, p. 555.

flame was cooled by diluting the cyanogen with carbonic acid ; and we have found that it retains its characters when the cyanogen is burnt in nitric oxide. The flame in the last case must be one of the hottest known, from the large amount of heat evolved in the decomposition of cyanogen and nitric oxide, namely, 41,000 and 43,300 units respectively. There is in the case of cyanogen, as in the case of so many other substances, a difference in the relative intensities of the different parts of the spectrum at different temperatures, but no other change of character.

On the theory that these groups of lines are the product of an exceptional temperature in the case of the cyanogen flame, it is inconceivable that they could disappear by combustion in oxygen, instead of in ordinary air. Our observations accord with the statement of Morren, Plücker, Hittorf, and Thalén, that a cyanogen flame, fed with oxygen, when it is intensely luminous, still yields these peculiar groups. We have found these peculiar groupings in the flame when it had a current of oxygen in the middle, and was likewise surrounded outside with oxygen. There is nothing remarkable in the fact that only a continuous spectrum is seen to proceed from any hydrocarbon or nitrocarbon burning in excess of oxygen, as we know from Frankland's experiments that carbonic acid and water vapour at the high temperature of flame under compression give in the visible portion a continuous spectrum. In fact, this is what we should anticipate, provided intermediate, and not the final, compounds are the active sources of the banded spectrum.

Each of the five sets of bands shown in the diagram is attended on its more refrangible side by a series of rhythmical lines extending to a considerable distance, not shown in the diagram, but easily seen in the photographs.

Coal gas burning in oxygen gives no bands above that near G within the range of the diagram, Fig. 2 ; but beyond this our photographs show a spectrum of a character quite different from that at the less refrangible end. The most remarkable part of this spectrum is a long series of closely set strong lines, filling the region between the solar lines R and S, and ending abruptly with two strong lines a little beyond S. These are lines of various intensities, not regularly arranged so as to give shaded bands like those in the less refrangible part of the spectrum. Beyond these lines there is another large group of lines, not so strong or so closely set, but sharp and well defined. This peculiar part of the spectrum is really due to the vapour of water, and shall be discussed in the sequel.

Spark Discharge in various Gases.

Mr. Lockyer's experiments on the spectrum of carbon compounds are directly opposed to the results given above, as will be understood by the following extract from one of his papers on the subject : *—

* 'Proc. Roy. Soc.' vol. xxx. p. 336.

"I beg permission, therefore, in the meantime, to submit to the notice of the Society an experiment with a tube containing CCl_4,* which, I think, establishes the conclusions arrived at by prior investigators. And I may add that it is the more important to settle the question, as Messrs. Liveing and Dewar have already based upon their conclusions theoretical views of a kind which appear to me calculated to mislead, and which I consider to have long been shown to be erroneous." The following experiments have been made to test the accuracy of our previous work, and to confirm or disprove Mr. Lockyer's views.

The form of sparking tube employed was similar to that used by Salet. This was attached by thick rubber tubing to a straight glass tube of which one half, about 6 inches long, was filled with phosphoric anhydride, and the other half with small fragments of soda-lime to prevent any chlorine from the decomposition of the tetrachloride by the spark from reaching the Sprengel pump. The tetrachloride used had been prepared in our own laboratory, and fractionated until it had a constant boiling point of 77° C. Sufficient of it was introduced into the sparking tube to fill nearly one quarter of the bulb at the end, and the whole interior of the tube thoroughly wetted with it in order to facilitate the removal of the last traces of air.

When the tube containing the tetrachloride had been so far exhausted that little but condensible vapours were pumped out, the bulb was heated so as to fill the apparatus with vapour of tetrachloride, the pump still going, and this was repeated as long as any incondensible gas was extracted. Sparks were then passed through the tube for a short time, the pump still being kept going. After a short time it was unnecessary to keep the pump going, as all the chlorine produced by decomposition of the tetrachloride was absorbed by the soda-lime. On now examining the spectrum, no trace of any of the bands we ascribe to nitrocarbons could be detected, either by the eye or by photography, however the spark might be varied. The violet lines of chlorine described by Salet were more or less visible, coming out brightly when a condenser was used. Several tubes were treated in this way, and many photographs taken, but always with the same result; no trace appeared of either the seven blue, the six violet, the five ultra-violet, or of the still more refrangible bands of the cyanogen flame. All the photographs showed three lines in the ultra-violet, but these do not at all resemble the nitrocarbon bands, as they are not shaded. The least refrangible of the three is nearly coincident with the middle maximum in the ultra-violet set of five bands, but the other two do not coincide with any of these maxima. When a condenser is used, these three lines come out with much greater intensity, and two other triplets appear on the more refrangible side, as well as other lines. In order to compare the positions of these lines with the cyanogen bands, we have taken several

* Carbon tetrachloride.

photographs of the spark in tetrachloride simultaneously with a cyanogen flame, the latter being thrown in by reflection in the usual way.

Not one of many photographs so taken showed any traces of the cyanogen bands. The general character of the violet part of the spectrum of the spark in carbon tetrachloride taken without a condenser (but not the exact position to scale of wave-lengths of all the lines) is shown at B in Fig. 2. At C of the same diagram are shown the brightest of the additional lines which come out with the use of a condenser. Photographs of sparks taken in hydrochloric acid showed a precisely similar group of ultra-violet lines, so that the three lines which our photographs show amongst the five ultra-violet nitrocarbon bands are due to chlorine.

Having satisfied ourselves by repeated trials that pure carbon tetrachloride or trichloride, if free from nitrogen, does not give any of the bands we ascribe to nitrocarbon compounds, our next step was to determine whether the addition of nitrogen would bring them out, and if so, what quantity of nitrogen would make them visible.

For this purpose we introduced a minute fragment of bichromate of ammonia, carefully weighed, and wrapped in platinum foil, into the neck of one of the sparking tubes containing carbon tetrachloride, connected the tube to the Sprengel pump, and removed the air as before. The spark examined in the tube showed no trace of any nitrocarbon band. A pinch-cock was now put on the rubber tube, and the bichromate heated by a spirit-lamp to decomposition (whereby it is resolved into nitrogen, water, and oxide of chromium). On now passing the spark the six violet bands were well seen. There was no change in the condition of the coil or rheotome, so that the spark was of the same character as it had been before when no nitrocarbon bands were visible, and the change in the spectrum cannot be attributed to any change in the spark. The weight of the bichromate was between ·0005 and ·0006 grm.; and the nitrogen this would evolve would fill just about $\frac{1}{20}$ of a cubic centimetre at atmospheric pressure. The tube held 30 cub. centims., so that vapour of carbon tetrachloride when mixed with $\frac{1}{600}$ part of its volume of nitrogen, gives under the action of the electric spark the nitrocarbon bands distinctly. Other similar experiments confirmed this result. It is worthy of remark that the nitrocarbon bands were not seen instantaneously on the admission of nitrogen into the tube, but were gradually developed, as if it was necessary that a certain quantity of nitrocarbon compound should be formed under the influence of the electric discharge and accumulated before its spectrum became visible.

A tube, containing naphthaline, previously well washed with dilute sulphuric acid, dried and resublimed, was attached to the Sprengel pump, and treated as the tubes with tetrachloride had been. The spark in this tube likewise showed no nitrocarbon bands. After a time the tube cracked, and then the nitrocarbon bands made their appearance, and on setting the pump going a good deal of gas was

pumped out. When the air had again been pretty completely exhausted, the nitrocarbon bands were no longer visible, but gradually reappeared again as air leaked through the crack. Another tube, containing a mixture of naphthaline and benzol, showed no trace of the nitrocarbon bands.

The observation of the nitrocarbon bands in the spectrum of the spark in naphthaline was one of the reasons which led Watts at one time to ascribe these bands to free carbon.

In our first experiments with carbonic oxide the gas was made by the action of sulphuric acid on dried formiate of sodium.

At first the six violet cyanogen bands were well seen, and the seven blue bands faintly; but gradually, as the air became more completely expelled, the blue bands disappeared entirely, and then the violet bands so far died out that it was only by manipulating the coil that they could be made visible, and then only very faintly. A bubble of air about $\frac{1}{400}$ part of the volume of gas in the generating flask and tube, was now introduced, when almost immediately the bands reappeared brightly. As the stream of gas continued, they again gradually died away until they were represented only by a faint haze. It was subsequently found that each introduction of fresh acid into the flask was attended with a marked increase in the brightness of the nitrocarbon bands, which died away again when the current of gas was continued without fresh introduction of acid. On testing the acid it was found to contain, as is frequently the case with sulphuric acid, a very small quantity of the oxides of nitrogen. The difficulty of getting all the air expelled from the apparatus and reagents led us to adopt another method of making carbonic oxide. Carbonic oxide was generated by heating in a tube of hard glass in a combustion furnace a mixture of pure dry potassium oxalate with one quarter of its weight of quicklime, the mixture having been previously heated for some time to expel traces of ammonia. No trace whatever of the nitrocarbon bands could be detected in this carbonic oxide, however the spark might be varied. The pressure of the gas was reduced to 1 inch of mercury, while the spectrum was observed from time to time. Still no trace of the nitrocarbon bands could be detected. More of the oxalate was heated, and the observations repeated again and again, always with the same result. Carbonic oxide, therefore, if quite free from nitrogen, does not give, at the atmospheric or any less pressure, the nitrocarbon spectrum.

On passing the spark between carbon poles in nitrogen, the nitrocarbon bands are plainly seen; and remain visible through great variations in the character of the spark. Photographs taken, with and without the use of the condenser, showed the violet and ultraviolet nitrocarbon bands, including those near N and P. If the nitrogen was swept out by a current of carbonic acid gas, on passing the spark the nitrocarbon bands could no longer be detected, and photographs showed no trace of any of the ultra-violet bands.

In all the foregoing experiments the bands which Angström and Thalén ascribe to hydrocarbons were always more or less plainly seen. Much more care than has generally been thought necessary is needed if the last traces of hydrogen and its compounds are to be removed from spectral tubes. Indeed, water cannot be completely removed from apparatus and reagents which do not admit of being heated to redness.

Thus a mixture of carbonate of sodium and boric anhydride, previously to admixture heated red hot, was introduced into one end of a piece of combustion tube, near the other end of which wires had been sealed, and the open end drawn out; the mixture was then heated, and when it was judged that all the air was expelled, the tube was sealed off at atmospheric pressure. On passing sparks through it carbonic oxide bands and oxygen lines could be seen, but no hydrogen, hydrocarbon, or nitrocarbon bands could be detected. It appears, therefore, that the application of a red heat is likely to prove a more effectual means of getting rid of moisture than the use of any desiccating agent.

Are the groups of shaded bands seen in the more refrangible part of the spectrum of a cyanogen flame, of which the three which can be detected by the eye are defined by their wave-lengths (4600 to 4502, 4220 to 4158, and 3883 to 3850), due to the vapour of uncombined carbon, or, as we conclude, to a compound of carbon with nitrogen?

The evidence that carbon can take the state of vapour at the temperature of the electric arc is at present very imperfect. Carbon shows at such temperatures only incipient fusion, and that uncombined carbon should be vaporised at the far lower temperature of the flame of cyanogen is so incredible an hypothesis, that it ought not to be accepted if the phenomena admit of any other probable explanation. On the other hand, cyanogen or hydrocyanic acid is generated in large quantity in the electric arc taken in nitrogen, and Berthelot has shown that hydrocyanic acid is produced by the spark discharge in a mixture of acetylene and nitrogen, so that in the cases in which these bands shine out with the greatest brilliance, namely, the arc in nitrogen and the cyanogen flame, we know that nitrocarbon compounds are present. Further, we have shown that these bands fade and disappear in proportion as nitrogen is removed from the arc. Angström and Thalén had previously shown the same thing with regard to the discharge between carbon electrodes; and the conclusion to which they and we have come would probably have commanded universal assent if it had not been for the fact that these bands had been seen in circumstances where nitrogen was supposed to be absent, but where, in reality, the difficulty of completely eliminating nitrogen, and the extreme sensibility of the spectroscopic test, had been inadequately apprehended.

Our argument is an induction from a very long series of observations which lead to one conclusion, and hardly admit of any other

explanation. Mr. Lockyer, however, attempts to explain the disappearance of the bands when nitrogen is absent by the statement, "that the tension of the current used now brings one set of flutings into prominence, and now another." This is no new observation. It is well known that variations in the discharge produce variations in the relative intensities of different parts of a spectrum. Certain lines of magnesium, cadmium, zinc, and other metals, very brilliant in the spark, are not seen, or are barely seen, at all in the arc. His remark might be applied to the spectra of compounds as well as to those of elements. Variation in the discharge accounts very well for some of the variations of intensity in the bands if they be due to a nitrocarbon; it will not, however, account for the fact observed by us, that the bands, or those of them which have the greatest emissive power, and are best developed by the particular current used, come out on the addition of a minute quantity of nitrogen, when there is every reason to think that no variation of the current occurs.

Much the same may be said with regard to the changes of the spectrum produced by changes of temperature. We cannot infer from any of these changes that the spectrum is not due to a compound.

Again, Mr. Lockyer attempts to get over the difficulties of his case by the supposition that "the sets of carbon flutings represent different molecular groupings of carbon, in addition to that or those which give us the line spectrum."

Now, until independent evidence can be adduced that carbon can exist in the state of uncombined vapour at the temperature of a cyanogen flame, and that different groupings in such vapour exist, the hypothesis here enunciated is a gratuitous one, so long as the existence of nitrocarbon compounds in the flame, arc, and spark will sufficiently explain the facts.

The observation above recorded, that there is in the spectrum of cyanogen a strong shaded band coincident with the very characteristic dark shaded band P of the solar spectrum, strengthens materially the evidence in favour of the existence of these bands in the solar spectrum; the more so, as the series of lines at P has far more of the distinctive character of the cyanogen spectrum than any other series in the ultra-violet part of the solar spectrum.

The hypothesis that if present they are due to vapour of carbon uncombined in the upper cooler region of the chromosphere seems absurd. One object of our investigations has been to determine the permanence of compounds of the non-metallic elements, and the sensitiveness of the spectroscopic test in regard to them. It appeared probable that if such compounds exist in the solar atmosphere their presence would be most distinctly revealed in the more refrangible part of the spectrum. In the meantime it is sufficiently clear that the presence of nitrogen in the solar atmosphere may be recognised through cyanogen when free nitrogen might escape detection.

The series of experiments, unless proved to be wrong, are almost conclusive proof that Mr. Lockyer's views regarding the origin and variation of the carbon spectrum have no real experimental basis.

Spectrum of Magnesium.

The absorption spectrum of magnesium and of magnesium with potassium and sodium, as seen in iron tubes in a hydrogen atmosphere, described in the former lecture, correspond to no known emission lines of magnesium. We could only ascribe their origin to the mixtures employed as distinct from the separate elements, and therefore were led to investigate the conditions under which corresponding emission lines could be produced.

In 'Proc. Roy. Soc.' vol. xxvii. p. 494, the emission spectrum of sparks from an induction coil taken between magnesium points in an atmosphere of hydrogen is described as follows:—

"A bright line regularly appeared with a wave-length about 5210. . . . . This line does not usually extend across the whole interval between the electrodes, and is sometimes seen only at the negative electrode. Its presence seems to depend on the temperature, as it is not seen continuously when a large Leyden jar is employed, until the pressure of the hydrogen, and its resistance, is very much reduced. When well-dried nitrogen or carbonic oxide is substituted for hydrogen, this line disappears entirely; but if any hydrogen or traces of moisture be present, it comes out when the pressure is much reduced. In such cases the hydrogen lines C and F are always visible as well. Sometimes several fine lines appear on the more refrangible side of this line between it and the *b* group, which give it the appearance of being a narrow band shaded on that side." "In addition to the above-mentioned line, we observed that there is also produced a series of fine lines, commencing close to the most refrangible line of the *b* group, and extending, with gradually diminishing intensity, towards the blue . . . . from forty-five to fifty being visible, and placed at nearly equal distances from each other."

In a paper entitled "A New Method of Spectrum Observation,"* Mr. Lockyer regards this spectrum as illustrative and confirmatory of his views regarding the possibility of elemental dissociation at different heat-levels. The view taken by Mr. Lockyer may be expressed in his own words:—

"The flame spectrum of magnesium perhaps presents us best with the beautiful effects produced by the passage from the lower to the higher heat-level, and shows the important bearing upon solar physics of the results obtained by this new method of work. . . . In the flame the two least refrangible of the components of *b* are seen associated with a line less refrangible so as to form a triplet. A

* 'Proc. Roy. Soc.' vol. xxx. p. 22.

series of flutings and a line in the blue are also seen. . . . On passing the spark all these but the two components of *b* are abolished. We get the wide triplet replaced by a narrow one of the same form, the two lines of *b* being common to both.

"May we consider the existence of these molecular states as forming a true basis for Dalton's law of multiple proportions? If so, then the metals in different chemical combinations will exist in different molecular groupings, and we shall be able, by spectrum observations, to determine the particular heat-level to which the molecular complexity of the solid metal, induced by chemical affinity, corresponds. . . . Examples.—None of the lines of magnesium special to the flame spectrum are visible in the spectrum of the chloride either when a flame or a spark is employed."

In order to ascertain the true cause of the variations in the magnesium spectrum, the following experiments and observations were made, and they demonstrate that the views of Mr. Lockyer on this question must also be regarded as resting on faulty experimenting:—

1. *Observations on the Spark between Magnesium Points in Nitrogen and Carbonic Oxide at various Pressures.*

The points were pieces of magnesium wire. Round one end of each a platinum wire was tightly coiled and fused into the side of a glass tube. This tube was attached by fusion at one end to another tube filled with phosphoric anhydride, which in turn was connected with a Sprengel pump. The other end of the tube was connected by a thick rubber tube, capable of being closed by a pinchcock, with a gasholder containing nitrogen over strong sulphuric acid. The tube having been exhausted and filled with nitrogen two or three times, it was found that no line at 5210 was visible in the spark. The tube was now gradually exhausted, and the spark watched as the exhaustion proceeded. No line at 5210 was seen, although the exhaustion was carried nearly as far as the pump would carry it; nor was any hydrogen line (C or F) visible, either with or without the use of a jar. The communication with the gasholder was now opened, and the tube refilled with nitrogen at the atmospheric pressure; a communication was then made with another vessel containing hydrogen, which was allowed to diffuse into the tube for a very short time. On now passing the spark, the line at 5210 at once appeared, although the quantity of hydrogen diffused into the nitrogen must have been very small. The experiments with nitrogen at reduced pressure were repeated several times, with the same result. It was found necessary to have the phosphoric anhydride, as without it traces of moisture were left or found their way through the pump into the tube, and then, when the exhaustion was carried far enough, both the line at 5210 and the hydrogen lines, C and F, made their appearance. We have never, however, been able to detect the line at 5210, in nitrogen, without being able to detect C or F

either at the same time or by merely varying the discharge by means of a Leyden jar.

Experiments made in the same way with carbonic oxide instead of nitrogen led to precisely the same results.

2. *Observations on the Spark between Magnesium Points in Hydrogen at reduced Pressures.*

A tube, similar to those employed with nitrogen and carbonic oxide, was attached at one end to a Sprengel pump and mercury gauge, and at the other end to an apparatus for generating hydrogen. Dry hydrogen was passed through for some time, and the connection with the hydrogen apparatus closed. On sparking with the hydrogen at the atmospheric pressure, the line at 5210 and its attendant series were visible, and were still visible when a small Leyden jar was used with the induction coil, but disappeared almost entirely when a large Leyden jar was used. When the pressure of the hydrogen was reduced to half an atmosphere, the line at 5210 was seen faintly when a large Leyden jar was used, but not the series of fine lines. When the pressure was reduced to 180 millims., the series of fine lines began to show when the large jar was used. By still further reducing the pressure the whole series was permanently visible when the large jar was used; but when the exhaustion was carried still further they grew fainter, and almost disappeared. On gradually readmitting hydrogen, the same phenomena recurred in the reverse order.

3. *Observations on the Arc with Magnesium and Hydrogen.*

The line at 5210 is not seen in the arc in a lime or carbon crucible when magnesium is dropped in without the introduction of hydrogen. If, however, a gentle stream of hydrogen or of coal gas be led in through a perforation in one of the electrodes, the line at 5210 immediately makes its appearance, and, by varying the current of gas, it may be made to appear either bright or reversed. However small the current of hydrogen be made, the line can be detected as long as the current and the supply of magnesium continue, but disappears very quickly when the current of gas ceases.

4. *Observations on the Flame of Burning Magnesium.*

The line at 5210 may often be seen in the flame of magnesium burning in air, but both it and the series of fine lines which accompany it come out with greatly increased brilliance if the burning magnesium be held in a jet of hydrogen, of coal gas, or of steam.

The experiments above described, with nitrogen and carbonic oxide at reduced pressures, are almost if not quite conclusive against the supposition that the line at 5210 is due merely to the lower

temperature of the spark in hydrogen. From De La Rue and Müller's observations it would appear that nitrogen at a pressure of 400 millims. should produce much the same effect on the spark as hydrogen at 760 millims. Now the pressures of the nitrogen and carbonic oxide were reduced far below this without any trace of the line in question being visible. Moreover, the magnesium line at 4481, which is not seen in the arc, and may be reasonably ascribed to the higher temperature of the spark, may be seen in the spark at the same time as the line at 5210 when hydrogen is present. Nevertheless temperature does seem to affect the result in some degree, for when a large Leyden jar is used, and the gas is at the atmospheric pressure, the line almost disappears from the spark, to reappear when the pressure is reduced; but by no variation of temperature have we been able to see the line when hydrogen was carefully excluded.

A line of the same wave-length has been seen by Young in the chromosphere once. Its absence from the Fraunhöfer lines leads to the inference that the temperature of the sun is too high (unless at special times and places) for its production. If it be not due to a compound of magnesium with hydrogen, at any rate it occurs with special facility in the presence of hydrogen, and ought to occur in the sun if the temperature were not too high.

We have thus far been careful to ascribe this line and its attendant series to a mixture of magnesium and hydrogen rather than to a chemical compound, because this sufficiently expresses the facts, and we have not yet obtained any independent evidence of the existence of any chemical compound of those elements. We have independent evidence that mixtures which are not probably chemical compounds favour the production of certain vibrations which are not so strong or are not seen at all when the elements of those mixtures are taken separately. The remarkable absorptions produced by mixtures of magnesium with potassium and sodium above-mentioned belong to this class. We have not been able to obtain the emission spectra corresponding to these absorptions, but in the course of our observations on the arc we have frequently noticed that certain lines of metals present in the crucible are only seen, or come out with especial brilliance, when some other metal is introduced. This is the case with some groups of calcium lines which are not seen, or are barely visible, in the arc in a lime crucible, and come out with great brilliance on the introduction of a fragment of iron, but are not developed by other metals such as tin.

Spark Spectrum of Magnesium in Hydrogen under increased Pressures.

In order to ascertain if this peculiar spectrum could be produced at a high temperature in the presence of hydrogen, which we have already shown to be essential to its production at the atmospheric and at reduced pressures, experiments were made with hydrogen at pressures increasing up to twenty atmospheres.

On the supposition that this spectrum originates from the formation of some chemical compound, probably formed within certain limits of temperature when vapour of magnesium is in presence of hydrogen, the stability of the body ought to depend largely on the pressure of the gaseous medium. Like Graham's hydrogenium, this body might be formed in hydrogen of high pressure at a temperature at which it would under less pressure be decomposed. In fact, it has been shown by Troost that the hydrides of palladium, sodium, and potassium all follow strictly the laws of chemical dissociation enunciated by Deville; and increased pressure, by rendering the compound more stable, ought, if the secondary effect of such pressure in causing a higher temperature in the electric discharge were not overpowering, to conduce to a more continuous and brilliant spectrum of the compound. Conversely, if such a more continuous and brilliant spectrum be found to result, in spite of the higher temperature, from increased pressure, it can only be explained by the stability of the substance being increased with the pressure.

Now, what are the facts? When the spark of an induction coil, without a Leyden jar, is passed between magnesium electrodes in hydrogen at atmospheric pressure, the flutings in the green are, as before described, always seen, but they are much stronger at the poles and do not always extend quite across the field. As the pressure is increased, however, they increase in brilliance and soon extend persistently from pole to pole, and go on increasing in intensity, until, at fifteen and twenty atmospheres, they are fully equal in brilliance to the *b* group, notwithstanding the increased brightness these have acquired by the higher temperature, due to the increased pressure. The second set of flutings, those in the yellowish green, come out as the pressure is increased, and, in fact, at twenty atmospheres only the *b* group and the flutings are noticeable; if the yellow magnesium line be visible at all it is quite lost in the brilliance of the yellow flutings. The tail of fine lines of these flutings extend at the high pressure quite up to the green, and those of the green flutings quite up to the blue. On again letting down the pressure the like phenomena occur in the reverse order, but the brilliance of the flutings does not diminish so rapidly as it had increased. If, now, when the pressure has again reached that of the atmosphere, a large Leyden jar be interposed in the circuit, on passing the spark the flutings are still seen quite bright, and they continue to be seen with gradually diminishing intensity until the sparks have been continued for a considerable time. It appears that the compound, which had been formed in large quantity by the spark without jar at the higher pressures, is only gradually decomposed, and not re-formed, by the high temperature of the spark with jar. This experiment, which was several times repeated, is conclusive against the supposition that the flutings are merely due to a lower temperature. When the pressure was increased at the same time that the jar was employed, the flutings

did not immediately disappear, but the expansion of the magnesium lines and the increase of the continuous spectrum seemed to overpower them.

When nitrogen was substituted for hydrogen, the strongest lines of the green flutings were seen when the spark without jar was first passed at atmospheric pressure, probably from hydrogen occluded, as it usually is, in the magnesium electrodes. As the pressure was increased they speedily disappeared entirely, and were not again seen either at high or low pressures.

With carbonic oxide the same thing occurred as with nitrogen; but in this gas the flutings due to the oxide of magnesium (wave-length 4930 to 5060) were, for a time, very well seen.

Fig. 4, Plate III., shows more completely than we have given it before the general character of the magnesium-hydrogen spectrum, which consists of two sets of flutings closely resembling in character the hydrocarbon flutings, each fluting consisting of a multitude of fine lines closely set on the less refrangible side, and becoming wider apart and weaker towards the more refrangible side, but extending under favourable circumstances much farther than is shown in the figure. The set in the green is the stronger, and it was to this that our former observations were confined. It has two flutings, one beginning at about wave-length 5210 and the other close to b_1 on its more refrangible side. The other set consists of three principal flutings, of which the first begins at about wave-length 5618, the next at about wave-length 5566, and the third begins with three strong lines at about the wave-lengths 5513, 5512, 5511. Both sets are very well seen when a magnesium wire is burnt in the edge of a hydrogen flame, and in the arc in a crucible of magnesia when a gentle current of hydrogen is led into it. There is also a pair of bands in the blue beginning at about the wave-lengths 4850, 4802.

Mr. Lockyer states (*loc. cit.*) that none of the lines of magnesium, special to the flame spectrum, are visible in the spectrum of the chloride, either when flame or spark is employed. But we find that when the spark is taken between platinum points, from a solution of the chloride of magnesium, in a tube such as those used by Delachanal and Mermet, the line at wave-length 5210 can frequently be seen in it when the tube is filled with air, and that if the tube be filled with hydrogen the green flutings of magnesium-hydrogen are persistent and strong.

Repeated observations have confirmed our previous statements as to the facility with which the magnesium-hydrogen spectrum can be produced in the arc by the help of a current of the gas. In a magnesia crucible, by regulating the current of hydrogen, the flutings can be easily obtained either bright or reversed.

The variations in the spectrum of magnesium, and the conditions under which it is observed, throw additional light on the question of the emissive power for radiation of short wave-lengths of substances

at the temperature of flames to which we alluded in our paper on the spectrum of water.*

Ultra-Violet Spectrum of the Flame of Burning Magnesium.

When magnesium wire or ribbon is burnt in air, we see the three lines of the b group, the blue line about wave-length 4570, first noticed by us in the spark spectrum;† and photographs show, besides, the well-known triplet in the ultra-violet between the solar lines K and L sharply defined, and the line for which Cornu has found the wave-length 2850 very much expanded and strongly reversed. These lines are all common to the flame, arc, and spark spectra; and the last of them (2850) seems to be by far the strongest line both in the flame and arc, and is one of the strongest in the spark. But, in addition to these lines, the photographs of the flame show a very strong, somewhat diffuse, triplet, generally resembling the other magnesium triplets in the relative position of its components, close to the solar line M; and a group of bands below it extending beyond the triplet near L. These bands have, for the most part, each one sharply defined edge, but fade away on the other side; but the diffuse edges are not all turned towards the same side of the spectrum. The positions of the sharp edges of these bands, and of the strong triplet near M, are shown in Pl. III., Fig. 1. It is remarkable that the triplets near P and S are absent from the flame spectrum, and that the strong triplet near M is not represented at all either in the arc or spark. The hydrogen-magnesium series of lines, beginning at a wave-length about 5210, are also seen sometimes, as already described by us,‡ in the spectrum of the flame; but we have never observed that the appearance of these lines, or of the strong line with which they begin, is connected with the non-appearance of b_4. Indeed, we can almost always see all three lines of the b group in the flame, though as b_4 is the least strong of the three, it is likely to be most easily overpowered by the continuous spectrum of the flame.

Burning magnesium in oxygen instead of atmospheric air does not bring out any additional lines; on the contrary, the continuous spectrum from the magnesia overpowers the line spectrum, and makes it more difficult of observation.

Magnesia heated in the oxyhydrogen jet does not appear to give the lines seen in the flame, except that at 2850.

Spectrum of the Arc.

The spectrum of magnesium, as seen in the arc, contains several lines besides those heretofore described. These lines come out brightly, generally considerably expanded, when a fragment of magnesium is dropped into the crucible through which the arc is passing,

* 'Proc. Roy. Soc.' 1880, No. 201, p. 152.

† Ibid. vol. xxvii. p. 350.

‡ Ibid. vol. xxx. p. 96.

but rapidly contract and gradually become very faint or disappear entirely.

By examining the arc of a Siemens machine, taken in a crucible of dense magnesia under the dispersion of the spectrum of the fourth order, given by a Rutherford grating of 17,296 lines to the inch, we are able to separate the iron and magnesium lines which form the very close pair b_4 of the solar spectrum. Either of the two lines can be rendered the more prominent of the pair at will, by introducing iron or magnesium into the crucible. The less refrangible line of the pair is thus seen to be due to iron, the more refrangible to magnesium. Comparison of the solar line and the spark between magnesium points confirms this conclusion, that the magnesium line is the more refrangible of the two.

In the ultra-violet part of the spectrum photographs show several new lines. First, a triplet of lines above U at wave-lengths about 2942, 2938·5, 2937. These lines are a little below a pair of lines given by the spark for which Cornu has found the wave-lengths 2934·9, 2926·7. The latter pair are not seen at all in photographs of the arc, nor the former three in those of the spark. The strong line, wave-length about 2850, is always seen, very frequently reversed. Of the quadruple group in the spark to which Cornu has assigned the wave-lengths 2801·3, 2797·1, 2794·5, and 2789·9, the first and third are strongly developed in the arc, the other two hardly at all. Next follows a set of five nearly equidistant lines, well-defined and strong, but much less strong than the two previously mentioned, wave-lengths about 2782·2, 2780·7, 2779·5, 2778·2, 2776·9. The middle line is a little stronger than the others. The same lines come out in the spark.

Beyond these follow a series of pairs and triplets; probably they are triplets in every case; but the third, most refrangible, line of the triplets is the weakest, and has not in every case been noticed as yet. These succeed one another at decreasing intervals with diminishing strength, and are alternately sharp and diffuse, the diffuse triplets being the strongest. The positions are shown in Pl. III., Fig. 2. The series resembles in general character the sodium and the potassium series described by us in a former communication, and we cannot resist the inference that they must be harmonically related, though they do not follow a simple harmonic law. The most refrangible line in the figure at wave-length 2605 represents a faint diffuse band which is not resolvable into lines; it belongs, no doubt, to the diffuse members of the series, and, to complete the series, there should be another sharp group between it and the line at wave-length 2630. This belonging to the weaker members of the series is too weak to be seen.

It is worthy of remark that the line at wave-length 5710, described by us in a previous communication,* is very nearly the octave of the strong line at 2850. Moreover, the measures we have taken of the wave-length of this last line, with a Rutherford grating of 17,296

* 'Proc. Roy. Soc.' No. 200, p. 98.

lines to the inch, indicate a wave-length 2852 nearly, which is still closer to the half of 5710.

When metallic magnesium is dropped into a crucible of magnesia or lime through which the arc is passing, the electric current seems sometimes to be conducted chiefly or entirely by the vaporised metal, so that the lines of other metals almost or wholly disappear; but the line at wave-length 3278 does not in such cases appear, though the other magnesium lines are very strongly developed. The line at wave-length 2850 is often, under such circumstances, enormously expanded and reversed, those at wave-lengths 2801, 2794, and the alternate diffuse triplets, including those near L and near S, much expanded and reversed, and the group of five lines (2776–2782) sometimes reversed.

When the arc of a Siemens machine is taken in a magnesia crucible, the strong line of the flame spectrum, wave-length 4570, is well seen sharply defined; it comes out strongly and a little expanded on dropping in a fragment of magnesium. When a gentle stream of hydrogen is led in through a hollow pole, this line is frequently reversed as a sharp black line on a continuous background. From comparing the position of this line with those of the titanium lines in its neighbourhood, produced by putting some titanic oxide into the crucible, we have little doubt that it is identical with the solar line 4570·9 of Angström.

When the arc is taken in a crucible into which the air has access, it may be assumed that the atmosphere about the arc is a mixture of nitrogen and carbonic oxide. When a stream of hydrogen is passed, either through a perforated pole or by a separate opening, into the crucible, the general effect is to shorten the length to which the arc can be drawn out, increase the relative intensity of the continuous spectrum, and diminish the intensity of the metallic lines. Thus, with a very gentle stream of hydrogen in the magnesia crucible, most of the metallic lines, except the strongest and those of magnesium, disappear. Those lines which remain are sometimes reversed; those at wave-length 2850 and the triplet near L being always so. With a stronger stream the lines of magnesium also disappear, the b triplet being the last in that neighbourhood to go, and b_1 and b_2 remaining after b_4 had disappeared.

Chlorine seems to have an opposite effect to hydrogen, generally intensifying the metallic lines, at least those of the less volatile metals, but it does not sensibly affect the spectrum of magnesium. Nitrous oxide produces no marked effect; coal-gas acts much as hydrogen.

Spectrum of the Magnesium Spark, in Gases under High Pressures.

In the spark of an induction coil taken between magnesium points in air we get all the lines seen in the arc except two blue lines at wave-lengths 4350 and 4166, three lines above U, and the series of

triplets more refrangible than the quintuple group about wave-length 2780. The blue line wave-length about 4570 is best seen in the spark without a jar when the magnesium electrodes are close together, and the rheotome made to work slowly; and this and the other faint lines of the spark at about 4586 and 4808 require for their detection a spectroscope in which the loss of light is small.

On the other hand, some additional lines are seen. Of these, the strong line at wave-length 4481 and the weaker line at 4586 are well known. Another faint line in the blue at wave-length 4808 has been observed by us in the spark, and two diffuse pairs between H and the triplet near L. Two ultra-violet lines at wave-lengths 2934·9, 2926·7 (Cornu) are near, but not identical with, two lines of the arc above mentioned; and two more lines at wave-lengths 2797·1, 2789·9 (Cornu) make a quadruple group with the very strong pair conspicuous in the arc in this region. The spectrum of the spark ends, so far as we have observed, with the quintuple group (2782–2776) already described in the arc.

When a Leyden jar is used with the coil, some of the lines are reversed. This is notably the case with the triplet near L, the line at wave-length 2850, and those at 2801 and 2794. Cornu* noticed the reversal of the two less refrangible lines of the triplet near L under these circumstances. This effect is very much increased by increasing the pressure of the gas in which the spark is taken. The Cailletet pump is well suited for such experiments. The gases used were hydrogen, nitrogen, and carbonic oxide; and the image of the spark was thrown on to the slit of the spectroscope by a lens. In hydrogen, when no Leyden jar was used, the brightness of the yellow and of the blue lines of magnesium, except at first that at wave-length 4570, diminished as the pressure increased; while, on the other hand, the *b* group was decidedly stronger at the higher pressure. The pressure was carried up to 20 atmospheres, and then the magnesium lines in the blue and below almost or entirely disappear, leaving only the *b* group very bright, and the magnesium-hydrogen bands which are described below; even the hydrogen lines F and C were not visible. When a jar was used, the magnesium lines expanded as the pressure was increased; all three lines of the *b* group were expanded and reversed at a pressure of 5 atmospheres; the yellow line, wave-length 5528, was also expanded but not reversed; and the line at 4481 became a broad, very diffuse band, but the line at wave-length 4570 was but very little expanded. The expansion both of the *b* group and of the yellow line seemed to be greater on the less refrangible than on the more refrangible side of each line, so that the black line in those which were reversed was not in the middle. When the jar was used, the pressure could not be carried beyond 10 or $12\frac{1}{2}$ atmospheres, as the resistance became then so great that the spark would not pass across the small distance of about 1 millim. between the electrodes.

* 'Compt. Rend.' 1871.

At a pressure of $2\frac{1}{2}$ atmospheres, with a jar, the ultra-violet magnesium triplet near L was very well reversed, and the two pairs of lines on its less refrangible side (shown in Plate III., Fig. 3) were expanded into two diffuse bands.

In nitrogen and in carbonic oxide the general effects of increased pressure on the magnesium lines (not the magnesium-hydrogen bands) seemed to be much the same as in hydrogen. Without a jar the blue and yellow lines were enfeebled, and at the higher pressures disappeared, while the *b* group was very brilliant but not much expanded. With the jar all the lines were expanded, and all three lines of the *b* group strongly reversed. The bands of the oxide (wave-length 4930–5000) were not seen at all in hydrogen or nitrogen; they were seen at first in carbonic oxide, but not after the sparking had been continued for some time.

The disappearance of certain lines at increased pressure is in harmony with the observations of Cazin,* who noticed that the banded spectrum of nitrogen, and also the lines, grew fainter as the pressure was increased, and finally disappeared. When a Leyden jar is employed there is a very great increase in the amount of matter volatilised by the spark from the electrodes, as is shown by the very rapid blackening of the sides of the tube with the deposited metal, and this increase in the amount of metallic vapour may reasonably be supposed to affect the character of the discharge, and conduce to the widening of the lines and the reversal of some of them. Without a jar the amount of matter carried off the electrode also doubtless increases with the pressure and consequent resistance, and may be the cause of the weakening, as Cazin suggests, of the lines of the gas in which the discharge is passed. It is to be noted, moreover, that the disappearance of the hydrogen lines depends, in some degree, on the nearness of the electrodes. The lines C and F which were, as above stated, sometimes invisible in the spark when the electrodes were near, became visible, under circumstances otherwise similar, when the magnesium points had become worn away by the discharge.

Comparison of the Spectra.

When we compare the spectra of magnesium in the flame, arc, and spark, we observe that the most persistent line is that at wave-length 2850, which is also the strongest in the flame and arc, and one of the strongest in the spark. The intensity of the radiation of magnesium at this wave-length is witnessed by the fact that this line is always reversed in the flame as well as in the arc when metallic magnesium is introduced into it, and in the spark between magnesium electrodes when a Leyden jar is used. It is equally remarkable for its power of expansion. In the flame it is a broad band, and equally so in the arc when magnesium is freshly introduced, but fines down to a narrow line as the metal evaporates.

* 'Phil. Mag.' 1877, vol. iv. p. 154.

Almost equal in persistence are the series of triplets. Only the least refrangible pair of these triplets are seen in the flame, another pair are seen in the spark, but the complete series is only seen in the arc. We regard the triplets as a series of harmonics, and to account for the whole series being seen only in the arc we must look to some other cause than the temperature. This will probably be found in the greater mass of the incandescent matter contained in the crucible in which the arc was observed.

The blue line of the flame at wave-length 4570 is well seen in the arc, and is easily reversed, but is always a sharp line, increased in brightness but not sensibly expanded by putting magnesium into the crucible. In the spark, at atmospheric pressure, it is only seen close to the pole or crossing the field in occasional flashes; but seems to come out more decidedly at rather higher pressures, at least in hydrogen.

The series of bands near L, well developed in the flame, but not seen at all in the arc or spark, look very much like the spectrum of a compound, but we have not been able to trace them to any particular combination. Sparks in air, nitrogen, and hydrogen have alike failed to produce them. The very strong, rather diffuse triplet at M, with which they end, so closely resembles in general character the other magnesium triplets, that it may well be connected with that constitution of the magnesian particle which gives rise to the triple sets of vibrations in other cases, but, if so, its presence in the flame alone is not easily explained.

The occurrence of this triplet in the ultra-violet, and of the remarkable series of bands associated with it, as well as the extraordinary intensity of the still more refrangible line at wave-length 2850, which is strongly reversed in the spectrum of the flame, corroborates what the discovery of the ultra-violet spectrum of water had revealed, that at the temperature of flame substances while giving in the less refrangible part of the spectrum more or less continuous radiation, may still give, in the regions of shorter wave-length, highly discontinuous spectra, such as have formerly been deemed characteristic of the highest temperatures. This subject we will not discuss further at present, but simply remark that "it opens up questions as to the emissive power for radiation of short wave-lengths of gaseous bodies at the comparatively low temperature of flame with regard to which we are accumulating facts."

In the arc and spark, but not in the flame, we have next a very striking group of two very strong lines at wave-lengths about 2801 and 2794, and a quintuple group of strong but sharp lines above them. The former are usually reversed in the spark with jar, and all are reversed in the arc when much magnesium is present. There are also several single lines in the visible part of the spectrum common to the arc and spark. All of these may be lines developed by the high temperature of the arc and spark. Two blue lines in the arc have not been traced in the spark, but their non-appearance may

be due to the same cause as that above suggested for the non-appearance of the higher triplets, the smallness of the incandescent mass in the spark.

A triplet of lines in the arc near U appear to be represented in the spark by an equally strong, or stronger, pair near but not identical in position. The possibility of such a shift, affecting these two lines only in the whole spectrum and affecting them unequally, must in the present state of our knowledge be very much a matter of speculation. Perhaps sufficient attention has not hitherto been directed to the probability of vibrations being set up directly by the electric discharge independently of the secondary action of elevation of temperature. Some of the observations above described, and many others well known, indicate a selective action by which an electric discharge lights up certain kinds of matter in its path to the exclusion of others; and it is possible that in the case of vibrations which are not those most easily assumed by the particles of magnesium, the character of the impulse may slightly affect the period of vibration. The fact that, so far as observations go, the shift in the case of this pair of magnesium lines is definite and constant, militates against the supposition suggested. On the other hand, the ghost-like pairs of lines observed in the spark below the triplet near L, suggest the idea that some of the particles have their tones flattened by some such cause.

The strong pair at wave-lengths 2801, 2794, are accompanied in the spark, but not usually in the arc, by a much feebler, slightly more refrangible pair, but these have not the diffuse ghost-like character of those just alluded to.

These lines are phenomena of the high potential discharge in which particles are torn off the electrodes with great violence and may well be thrown into a state of vibration which they will not assume by mere elevation of temperature.

There are two lines in the spark besides the well-known line at wave-length 4481 which have not been observed in the arc, but they are feeble and would be insignificant if it were not the fact that they, as well as wave-length 4481, all short lines seen generally only about the poles, appear to be present in the solar spectrum. In the sun we seem to have all the lines common to the flame, arc, and spark, and possibly the strong triplet of the flame at M. We have noticed that when the spark is taken in hydrogen, the line at wave-length 4570 appears stronger than that at wave-length 4703, while the reverse is the case when the atmosphere is nitrogen. It is possible then that the atmosphere may, besides the resistance it offers to the discharge, in some degree affect the vibrations of the metallic particles.

The substantive result of the investigation is to prove that the chemical atoms of magnesium are capable of taking up a great variety of vibrations, and by mutual action on each other, or on particles of matter of other kinds, give rise to a great variety of vibrations of

the luminiferous ether; and to trace satisfactorily the precise connection between the occurrence of the various vibrations and the circumstances under which they occur, will require an extended series of observations.

On the Spectrum of Water.

In our observations "On the Spectrum of the Compounds of Carbon," we noticed that a remarkable series of lines, extending over the region between the lines S and R of the solar spectrum, were developed in the flame of coal-gas burning in oxygen.* The arrangement of lines and bands, of which this spectrum consists, is shown in the Pl. II., Fig. 3. It begins at the more refrangible end with two strong bands, with wave-lengths about 3062, 3068, and extends up to about the wave-length 3210. It is well developed in the flame of hydrogen as well as of hydrocarbons, burning in oxygen, and less strongly in the flames of non-hydrogenous gases, such as carbonic oxide and cyanogen, if burnt in moist oxygen. The same spectrum is given by the electric spark taken, without condenser, in moist hydrogen, oxygen, nitrogen, and carbonic acid gas, but it disappears if the gas and apparatus be thoroughly dried. We are led to the conclusion that the spectrum is that of water. The plate, Fig. 3, is a general view of this spectrum. It was necessary to pass a current of dry gas for fully an hour through the warmed sparking apparatus before the moisture was sufficiently absorbed by the dehydrating agents. When this was done, photographs of the spark showed either no trace, or only the faintest traces, of the spectrum above described. On introducing a drop of water, and letting it spread over a plug of asbestos placed in the current of gas, the spectrum above described at once imprinted itself on the photographic plate. The effect was the same, whether the gas used was hydrogen, oxygen, nitrogen, or carbonic acid. In the case of nitrogen, some of the channelled bands due to that gas overlap the water spectrum, and partly obscure it, but not so much but that it can be still very distinctly recognised. When a condenser is used, the water spectrum disappears. The same spectrum appears in the De Meritens arc, but is less fully developed. The spectrum we have figured does not by any means exhaust the ultra-violet spectra of the flames we have observed. In writing of this and other spectra which we have traced to compounds, we abstain from speculating upon the particular molecular condition or stage of combination of decomposition, which may give rise to such spectra. The fact of an ultra-violet spectrum of water occurring in spectra of flames opens up

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* This we recorded in a Note of date June 8, 1880, see 'Proc. Roy. Soc.' No. 205, p. 5. Dr. Huggins discovered the same spectrum independently, and communicated the same on June 16, 1880. Our paper on this special spectrum bears date 17th June. Both papers were read at the same meeting of the Society.

questions as to the emissive power for radiation of short wave-lengths of gaseous substances at comparatively low temperatures.

Such facts completely modify the inferences which have been drawn as to the continuity of flame spectra and the character of the specific absorption of the vapour of water.

Identity of Spectral Lines.

In Kirchhoff's 'Researches on the Spectra of the Chemical Elements,' p. 10, the following reference is made to the apparent identity of wave-length of some spectral lines.

"If we compare the spectra of the different metals with each other several of the bright lines appear to coincide. This is especially noticeable in the case of an iron and magnesium line at 1655·6 (b_4), and with an iron line and calcium line at 1522·7 (E). It seems to me to be a question of great interest to determine, whether these and other similar coincidences are real or only apparent; whether the lines in question actually fall one upon the other, or whether they lie very close together. I believe that my method of observation does not possess the requisite accuracy for the purpose of answering this question with any degree of probability, and I think that a large number of prisms and an increased intensity of light will prove necessary."*

The subsequent investigations of Angström and Thalén increased the number of apparent coincidences amongst the spectral lines of different elements.

The question of the identity of spectral lines exhibited by different elements is one of great interest, because it is very improbable that any single molecule should be capable of taking up all the immense variety of vibrations indicated by the complex spectrum of iron or that of titanium, and it might therefore be expected that such substances consist of heterogeneous molecules, and that some molecules of the same kind as occur in these metals should occur in more than one of the supposed elements. Further, the supposed identity of certain lines in the spectra of more than one element has been made by Mr. Lockyer the ground of an argument in support of a theory as to the dissociation of chemical elements into still simpler constituents, and in reference to this he wrote: † "The 'basic' lines recorded by Thalén will require special study, with a view to determine whether their existence in different spectra can be explained or not on the supposition that they represent the vibrations of forms, which, at an early stage of the planet's history, entered into combination with other forms, differing in proximate origin, to produce different 'elements.'"

Young, on examining with a spectroscope of high dispersion the

* 'Researches on the Spectra of the Chemical Elements,' by G. Kirchhoff, p. 10. 1862.

† 'Proc. Roy. Soc.' vol. xxx. p. 31.

70 lines given in Angström's map as common to two or more substances, has found that 56 are double or treble, 7 more doubtful, and only 7 appear definitely single, and he remarks: * "The complete investigation of the matter requires that the bright line spectra of the metals in question should be confronted with each other and with the solar spectrum under enormous dispersive power, in order that we may determine which of the components of each double line belongs to one and which to the other element." It is this confronting of the bright line spectra of some of the terrestrial elements which we have attempted, and of which we now give an account. For the dispersion we have used a reflecting grating similar to that used by Young, with 17,296 lines to the inch, and a ruled surface of about 3½ square inches; telescope and collimator, each with an aperture of 1½ inch and focal length 18 inches, the lenses being of quartz, cut perpendicularly to the axis and unachromatised, giving a very good definition with monochromatic light. The chromatic aberration is in this case an advantage, for when the telescope is in focus for lines in the spectrum of any given order, the overlapping parts of spectra of different orders are out of focus, and their brightness consequently more or less enfeebled. We have sometimes used green or blue glasses to enhance this result. The telescope and the collimator were generally fixed at about 45°, the collimator being more nearly normal to the grating than the telescope, and the grating moved to bring in successive parts of the spectra. For the parts of the spectra less refrangible than the Fraunhöfer line E the spectrum of the third order was employed, for the more refrangible rays that of the fourth order. The source of light was the electric arc taken in a crucible of magnesia or lime, the image of the arc being focussed on the slit; and, for the examination of any supposed coincidence, first one metal was introduced into the crucible, and the line to be observed placed on the pointer of the eye-piece; the second metal was then introduced, and then in most cases, as detailed below, *two* lines were seen where only one was visible before, and the pointer indicated which of the two belonged to the metal first introduced. In some cases where both metals were already in the crucible, we had to reinforce the spectrum of one of the metals by the introduction of more of that metal, which generally brought out the spectrum of that metal more markedly than the other, and enabled us to distinguish the lines with a high degree of probability. Thus the crucibles of magnesia, or the carbons, always contain sufficient lithium to show the orange line and the calcium line heretofore supposed coincident with it (wave-length 6101·9), but we observed these lines quite distinct and separated by a distance, estimated by the eye in comparison with the distance of neighbouring titanium lines, at about one division of Angström's scale. On dropping a minute piece of lithium carbonate into the crucible, the less refrangible line was seen to expand and

* 'American Journal of Science,' vol. xx. p. 353.

for a short time to be reversed, the other line remaining narrow and quite unaltered. When the lithium had evaporated, and both lines were again narrow, a small piece of Iceland spar was dropped into the crucible, which immediately caused the expansion, and on one occasion the reversal, of the more refrangible line, while now the less refrangible line was unaffected.

In this way we satisfied ourselves that the calcium line is the more refrangible of the two, and is probably represented by the line at wave-length 6101·9 in Angström's normal solar spectrum, while the lithium line appears to be unrepresented.

In the case of iron, which gives such a multitude of lines, it was *à priori* probable that some lines would be coincident, or nearly so, with lines of other elements; and in fact we find that in five-sixths of the supposed coincidences lines of iron are involved. We have, therefore, chiefly directed our attention to iron lines. A complete account of the separate resolutions will be found in the 'Proceedings of the Royal Society,' May, 1881.

Pl. II., Fig. 5, shows the appearance of the magnesium group of the solar spectrum as observed in spectroscopes used by different observers. The lines marked b^3 and b^4, which appear to be single lines in the maps of Angström and Kirchhoff, are resolved into double lines by the greater dispersion employed by Thollon. The following table shows the relative dispersion and number of lines seen by different observers when powerful instruments are directed to the same solar group:—

GROUP E OF SOLAR SYSTEM.

	Number of Lines.	Dispersion.
Angström	11 ..	800
Kirchhoff	12 ..	1400
Pickering	29 ..	2000
Young	36 ..	2720

The indium line 4101·2 we found very difficult to separate from the hydrogen line (h), as the latter had to be observed from a tube with a spark, and it is both faint and diffuse; but several observations all led to the conclusion that the indium line is very slightly less refrangible than that of hydrogen.

We have also directly compared the iron line at 5316·07 with the solar spectrum, and found that the iron line corresponds with the less refrangible of the two solar lines at this place, so that the chromospheric line is in all probability the other line of the pair.

There are still a few cases of supposed coincidences which we have not examined. The results which we have recorded strongly confirm Young's observations, and leave, we think, little doubt that the few as yet unresolved coincidences either will yield to a higher dispersion, or are merely accidental. It would indeed be strange if, amongst all the variety of chemical elements and the still greater variety of vibrations which some of them are capable of taking up,

there were no two which could take up vibrations of the same period. We certainly should have supposed that substances like iron and titanium, with such a large number of lines, must each consist of more than one kind of molecule, and that not single lines, but several lines of each, would be found repeated in the spectra of some other chemical elements. The fact that hardly single coincidences can be established is a strong argument that the materials of iron and titanium, even if they be not homogeneous, are still different from those of other chemical elements. The supposition that the different elements may be resolved into simple constituents, or into a single one, has long been a favourite speculation with chemists; but however probable this hypothesis may appear *à priori*, it must be acknowledged that the facts derived from the most powerful method of analytical investigation yet devised give it scant support.

[J. D.]

PLATE I.

Fig. 1.

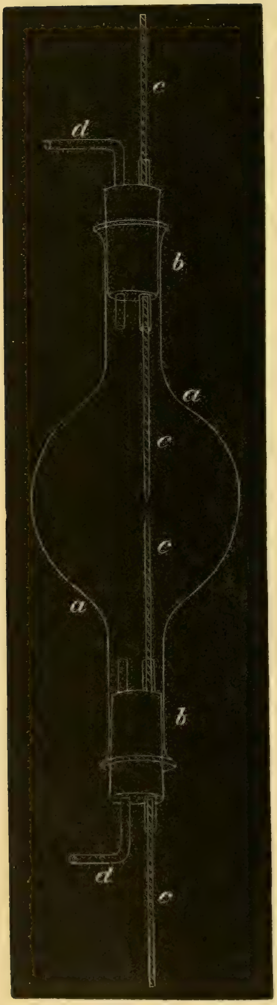
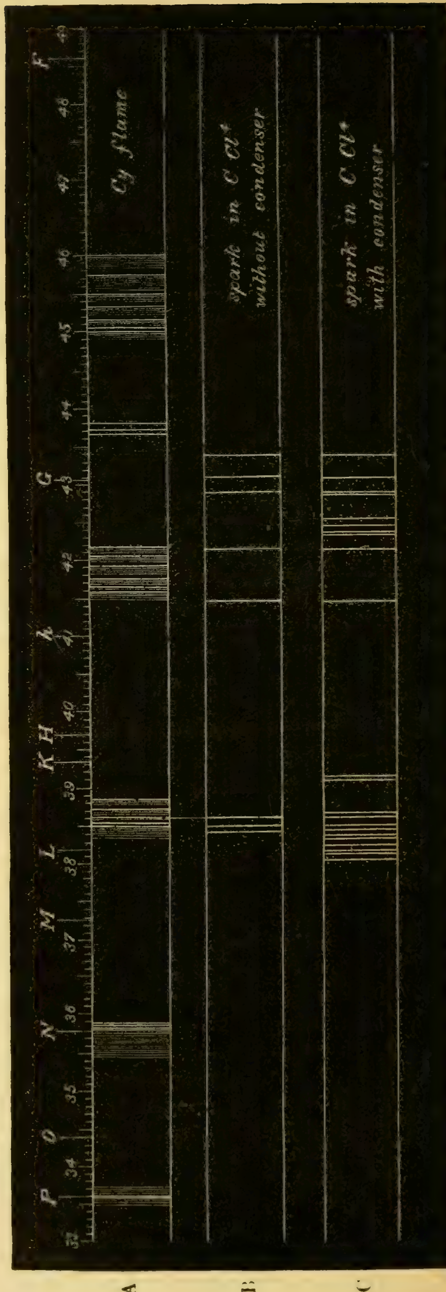


Fig. 2.

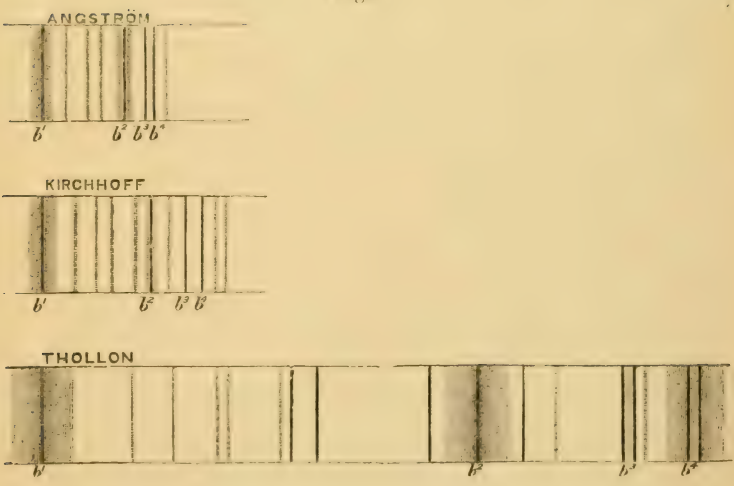



MAGNESIUM SPECTRUM
Fig. 1 Flame
Fig. 2 arc
Fig. 3 spark
Fig. 4 magnesium hydrogen
Fig. 1 Flame
Fig. 2 arc
Fig. 3 spark

PLATE II.

Fig. 3.

Fig. 5.



GENERAL MONTHLY MEETING,

Monday, July 4, 1881.

The Duke of Northumberland, D.C.L. LL.D. President, in the Chair.

S. P. Lucas Konarski, Esq.
Charles William Mitchell, Esq.

were elected Members of the Royal Institution.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

from

The Secretary of State for India—Account of the Great Trigonometrical Survey of India. Vol. VI. 4to. Dehra Dun, 1880.
The Governor General of India—Geological Survey of India: Records. Vol. XIV. Part 2. 8vo. 1881.

Accademia dei Lincei, Reale, Roma—Atti, Serie Terza: Transunti, Tome V. Fasc. 13. 4to. 1881.
Asiatic Society of Bengal—1881, Proceedings, No. 4. 8vo.
Journal, Vol. XLIX. Part I. Extra No. Vol. L. Part I. No. 1. Part II. No. 1. 8vo. 1881.
Astronomical Society, Royal—Monthly Notices, Vol. XLI. No. 7. 8vo. 1881.
Bankers' Institute—Journal, Vol. II. Part 6. 8vo. 1881.
Chemical Society—Journal for June, 1881. 8vo.
Crisp, Frank, Esq. LL.B. F.L.S. &c. M.R.I. (the Editor)—Journal of the Royal Microscopical Society, Series II. Vol. I. Part 3. 8vo. 1881.
Editors—American Journal of Science for June, 1881. 8vo.
Analyst for June, 1881. 8vo.
Athenæum for June, 1881. 4to.
Chemical News for June, 1881. 4to.
Engineer for June, 1881. fol.
Horological Journal for June, 1881. 8vo.
Iron for June, 1881. 4to.
Nature for June, 1881. 4to.
Revue Scientifique and Revue Politique et Littéraire, June, 1881. 4to.
Sanitary Engineering, No. 1. 4to. 1881.
Telegraphic Journal for June, 1881. 8vo.
Franklin Institute—Journal, No. 666. 8vo. 1881.
Geographical Society, Royal—Proceedings, New Series. Vol. III. No. 6. 8vo. 1881.
Geological Society—Quarterly Journal, No. 146. 8vo. 1881.
Houzeau, J. C. et Lancaster, A. (the Authors)—Bibliographie Générale de l'Astronomie. Tome II. Fasc. 3. 8vo. Bruxelles, 1881.
Linnean Society—Journal, Nos. 109–112. 8vo. 1879–81.
Lisbon Royal Academy of Sciences:—
Historia e Memorias:
Classe de Sciencias Moraes, Politicas e Bellas-Lettras: Nova Serie, Tomo V. Parte I. 4to. 1879.
Classe de Sciencias Mathematicas, Physicas e Naturaes: Nova Serie, Tomo V. Parte 2. 4to. 1878.
Jornal, Num. 24–29. 8vo. 1878–80.

Sessão Publica, em 9 Junio, 1880. 8vo.
E. A. Motta: Histologia Geral. 8vo. Lisboa, 1880.
Demosthenes: Oração da Coroa: Versão por J. M. L. Coelho 8vo. Lisboa, 1880.
W. Shakespeare: Hamlet: Traducção de Bulhão Pato. 8vo. Lisboa, 1879.
Fernão de Magalheãs: por Diego de Barròs Arana: Traducção de Fernando de Magalhães de Villas-Boas. 8vo. Lisboa, 1881.
Ovidio: os Fastos: Traducção por A. F. de Castilho. 3 vols. 8vo. Lisboa, 1862.
P. Calderon de Barca: Vida e Escriptos: por J. S. Ribeiro. 8vo. Lisboa, 1881.

Lisbon, Sociedade de Geografia—Boletim: 2e Serie, No. 4. 8vo. 1881.
Liverpool Literary and Philosophical Society—Proceedings, Vols. XXXIII. and XXXIV. 8vo. 1878–80.
Longmans, Messrs. & Co. (the Publishers)—H. Watts: Dictionary of Chemistry, Vol. VIII. Third Supplement, Part 2. 8vo. 1881.
Meteorological Office—Report of International Meteorological Committee, Berne, 1880. 8vo. 1881.
Pharmaceutical Society of Great Britain—Journal, June, 1881. 8vo.
Photographic Society—Journal, New Series, Vol. V. Nos. 8, 9. 8vo. 1881.
Scottish Society of Arts, Royal—Transactions, Vol. X. Part 3. 8vo. 1881.
Symons, G. J.—Monthly Meteorological Magazine, June, 1881. 8vo.
United Service Institution, Royal—Journal No. 110. 8vo. 1881.
Upsal University—Bulletin Mensuel de l'Observatoire Météorologique, 1880. Vol. XII. 4to. 1880–1.
Verein zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, 1881. Nos. 5, 6. 4to.
Zoological Society of London—Transactions, Vol. XI. Part 5. 4to. 1881.
Proceedings in 1881. Part 1. 8vo. 1881.

GENERAL MONTHLY MEETING,

Monday, November 7th, 1881.

George Busk, Esq. F.R.S. Treasurer and Vice-President, in the Chair.

Edward Easton, Esq. M.I.C.E.

was elected a Member of the Royal Institution.

The Special Thanks of the Members were given to the Secretary of State for India for the present of 'The People of India, by J. F. Watson and J. Kaye,' Volumes III.–VIII.

The Chairman announced that the Fullerian Professorship of Physiology became vacant on the 4th of November; and that the Managers would proceed to the election of a Professor on the 5th of December next.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Governor General of India—Geological Survey of India:
  Records. Vol. XIV. Part 3. 8vo. 1881.
  Memoirs: Vol XVI. Parts 2, 3. 4to. 1880.
  Palæontologia Indica: Series II. Vol. I. Parts 1–4. Series XI. Vol. II. Parts 1, 2. Series II. XI. XII. Vol. III. fol. 1880-1.
The Secretary of State for India—J. Forbes Watson and John William Kaye. The People of India (with Photographs). Vols. III.–VIII. 4to. 1868–75.
The Lords Commissioners of the Admiralty—Greenwich Observations for 1879. 4to. 1881.
  Cape Catalogue of Stars: 1834–40. 8vo. 1878.
  Catalogue of 12,441 Stars for 1880, from observations made at the Cape of Good Hope during the years 1871–9. 4to. 1881.
Académie des Sciences de l'Institut de France—Mémoires. Tome XLI. 2e Série. 4to. 1879.
  Présentés par Divers Savants. 2e Série. Tome XXVI. 4to. 1878.
Accademia dei Lincei, Reale, Roma—Atti, Serie Terza: Transunti, Tome V. Fasc. 14. 4to. 1881.
Actuaries, Institute of—Journal. No. 123. 8vo. 1881.
American Academy of Arts and Sciences—Proceedings. Vol. XVI. Parts 1 and 2. 8vo. 1881.
American Metrological Society—Proceedings. Vols. I. and II. 1873–79. 8vo. New York, 1880.
American Philosophical Society—Transactions, Vol. XV. Part 3. 4to. 1881.
  Proceedings, Nos. 107, 108. 8vo. 1880–1.
Antiquaries, Society of—Archæologia. Vol. XLVI. Part 2. 4to. 1881.

Archæological Survey of Western India—J. Burgess and Bhagwan Lal Indraji Pandit: Inscriptions from the Cave Temples of Western India. No. 10. 4to. Bombay, 1881.
Asiatic Society, Royal—Journal, New Series, Vol. XIII. Parts 3, 4. 8vo. 1881.
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Meteorologische und Magnetische Beobachtungen bei München. 1880. 8vo. 1881.
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Braine, Woodhouse, Esq. F.R C.S. M.R.I. (the Author)—Index to the Laws of Whist. 16to. 1881.
British Architects, Royal Institute of—Proceedings, 1881–82, Nos. 1 and 2. 4to. 1881.
Browning, Oscar, Esq. M.A. (the Author)—An Introduction to the History of Educational Theories. 8vo. 1881.
Chemical Society—Journal for July-Oct. 1881. 8vo.
Civil Engineers' Institution—Minutes of Proceedings, Vols. LXIV. LXV. and LXVI. 8vo. 1880.
Subject Index, Vols. I.–LVIII. 8vo. 1881.
Cole, John, Esq.—H. W. Cole, Q.C. Saint Augustine: a Poem. 8vo. 1877.
Commissioners in Lunacy—Thirty-fifth Report, 1880. 8vo. 1881.
Crisp, Frank, Esq. LL.B. F.L.S. &c. M.R.I. (the Editor)—Journal of the Royal Microscopical Society, Series II. Vol. I. Parts 4, 5. 8vo. 1881.
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Dax: Société de Borda—Bulletins, 2e Série, sixième Année: Trimestre 1, 2, 3. 8vo. Dax, 1879.
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Engineer for July–Oct. 1881. fol.
Horological Journal for July–Oct. 1881. 8vo.
Iron for July–Oct. 1881. 4to.
Nature for July–Oct. 1881. 4to.
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General Index of the fourth ten volumes of the Journal. 8vo. 1881.
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Glasgow Philosophical Society—Proceedings, Vol. XIII. No. 1. 8vo. 1881.
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Judge, Mark H. Esq. (the Secretary)—Parkes Museum of Hygiene: Official Catalogue of International Sanitary Exhibition, 1881. 8vo. 1881.
Kerslake, Thomas, Esq. (the Author)—Caer Pensauelcoit: A Long Lost Unromanised British Metropolis—A Re-assertion. (K 104) 8vo. 1881.
Linnean Society—Journal, Nos. 86, 87, 113, 114. 8vo. 1881.
Lisbon, Sociedade de Geografia—Boletim: 2e Serie, Nos. 5, 6. 8vo. 1881.
Lloyd, Wm. Watkiss, Esq. M.R.I. (the Author)—The History of Sicily to the Athenian War, with Elucidations of the Sicilian Odes of Pindar. 8vo. 1872.
The Age of Pericles: A History of the Politics and Arts of Greece from the Persian to the Peloponnesian War. 2 vols. 8vo. 1875.
Madras Literary Society—Madras Journal of Literature and Science for 1880. 8vo. 1881.
Manchester Geological Society—Transactions, Vol. XVI. Parts 6, 7, 8. 8vo. 1881.
Manchester Steam Users' Association—Reports, 1879. 8vo. 1879.
Mechanical Engineers, Institution—Proceedings, 1881. No 2. 8vo. 1881.
Medical and Chirurgical Society, Royal—Proceedings, No. 5. 8vo. 1881.
Meteorological Society—Quarterly Journal, Nos. 37, 38, 39. 8vo. 1880.
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Pasolini, Pietro Desiderio (the Author)—Documenti riguardanti Antiche Relazioni fra Venezia e Ravenna. 8vo. Imola, 1881.
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Rigg, Edward, Esq. M.A. (the Author)—Watchmaking. (Cantor Lectures). 8vo. 1881.
Royal College of Surgeons—Calendar, July 1881. 8vo.
Royal Society of London—Proceedings, Nos. 213, 214, 215. 8vo. 1881.
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Mémoires, 7e Série, Tome XXVIII. Nos. 3, 4, 5. 4to. 1880–1.
Saxon Society of Sciences, Royal—Philologisch-historische Classe: Abhandlungen: Band VIII. Nos. 2, 3. 8vo. 1880–1.
Verhandlungen, 1880, Nos. 1, 2. 8vo.

Mathematisch-physische Classe: Abhandlungen: Band XII. Nos. 2A, 5, 6. 8vo. 1880.

Verhandlungen, 1880, Nos. 1, 2. 8vo.

Siemens, C. Wm. Esq. D.C.L. F.R.S. M.R.I. (the Author)—On some Applications of Electric Energy to Horticulture and Agriculture, and a Contribution to the History of Secondary Batteries. (K 104) 8vo. 1881.

Smithsonian Institution, Washington—Smithsonian Contribution to Knowledge, Vol. XXIII. 4to. 1881.

Smithsonian Miscellaneous Collections, Vols. XVIII. XIX. XX. XXI. 8vo. 1880–1.

Joseph Henry: Memorial. 8vo. 1880.

Société Hollandaise des Sciences—Archives Néerlandaises, Tome XVI. Liv. 1, 2. 8vo. 1881.

Society of Arts—Journal, July–Oct. 1881. 8vo.

Statistical Society—Journal, Vol. XLIV. Parts 2, 3. 8vo. 1881.

Stone, W. H. Esq. M.B. (the Author)—Scientific Teaching (from Popular Science Review, July 1881).

Swedish Academy of Sciences, Royal—Handlingar (Memoirs), Vol. XIV. Part 2; XV. XVI. and XVII. 4to. 1876–9.

Ofversigt: af Förhandlingar-Argangen 34, 35, 36, 37. 8vo. Stockholm, 1877–80.

Bihang: Bandet IV. Häfte 1, 2. Bandet V. Häfte 1, 2. 8vo. 1877–80.

Lefnadsteckningar ofver: Band II. Häfte 1. 1878.

Symons, G. J.—Monthly Meteorological Magazine, July–Oct. 1881. 8vo.

Tasmania Royal Society—Monthly Notices for 1878. 8vo. 1880.

Telegraph Engineers, Society of—Journal, Part 37. 8vo. 1881.

Teyler Musée—Archives: Série II. Partie 1. 8vo. 1881.

United Service Institution, Royal—Journal, Nos. 111, 112. 8vo. 1881.

Verein zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, 1881, Nos. 7, 8. 4to.

Victoria Institute—Journal of Transactions, Nos. 58, 59. 8vo. 1881.

Vincent, B. Esq. Lib. R.I. (the Editor)—Haydn's Dictionary of Dates. 17th Edition. 8vo. 1881.

Yorkshire Archæological and Topographical Association—Journal, Part 25. 8vo. 1881.

Zoological Society of London—Proceedings in 1881. Parts 2, 3. 8vo. 1881.

GENERAL MONTHLY MEETING,

Monday, December 5, 1881.

George Busk, Esq. F.R.S. Treasurer and Vice-President, in the Chair.

Henry Chester, Esq.
John Grey, Esq.
Mrs. Thomas De Horne.
Vyvyan Charles Miles, Esq. B.A.
George Edward Nash, Esq.
William Russ Pugh, M.D.
John Barclay Scriven, Esq.
Francis Whitaker, Esq.
Robert Porter Wilson, Esq.

were elected Members of the Royal Institution.

The Managers reported, that at their Meeting this day, they appointed Professor John G. McKendrick, M.D. F.R.S.E. Fullerian Professor of Physiology for three years.

The Resignation of Mr. Warren De La Rue, as Secretary, on account of ill-health, was announced, to the great regret of the Members.

The following Lecture Arrangements were announced:—

Robert Stawell Ball, Esq. LL.D. F.R.S. Andrews Professor of Astronomy in the University of Dublin, and Royal Astronomer of Ireland.—Six Lectures (adapted to a Juvenile Auditory) on "The Sun, the Moon, and the Planets" (with illustrations by the electric light, &c.); on Dec. 27 (Tuesday), Dec. 29, 31, 1881; Jan. 3, 5, 7, 1882.

Professor John G. McKendrick, M.D. F.R.S.E. Fullerian Professor of Physiology.—Eleven Lectures on The Mechanism of the Senses; on Tuesdays, Jan. 17 to March 28.

Henry N. Moseley, Esq. M.A. F.R.S.—Four Lectures on Corals; on Thursdays, Jan. 19, 26, and Feb. 2, 9.

Philip Lutley Sclater, Esq. M.A. F.R.S. F.L.S. Ph.D. Secretary of the Zoological Society.—Four Lectures on The Geographical Distribution of Animals; on Thursdays, Feb. 16, 23, and March 2, 9.

Professor Tyndall, D.C.L. F.R.S. *M.R.I.*—Three Lectures; on Thursdays, March 16, 23, and 30.

Ernst Pauer, Esq. Principal Professor of the Pianoforte at the National Training School for Music.—Four Lectures on Ludwig von Beethoven (with illustrations on the Pianoforte); on Saturdays, Jan. 21, 28, and Feb. 4, 11.

William Watkiss Lloyd, Esq. *M.R.I.*—Four Lectures on The Language, Mythology, Construction, and Characteristics of the Iliad and Odyssey; on Saturdays, Feb. 18, 25, and March 4, 11.

Professor Harry Govier Seeley, F.R.S. F.G.S. &c.—Three Lectures on Volcanoes; on Saturdays, March 18, 25, and April 1.

The Presents received since the last Meeting were laid on the table, and the thanks of the Members returned for the same, viz.:—

FROM

The Lords Commissioners of the Admiralty—Nautical Almanac for 1885. 8vo. 1881.
Accademia dei Lincei, Reale, Roma—Atti, Serie Seconda: Vol. V. VI. VII. 4to. 1880.
Atti, Serie Terza: Vol. VI. Fasc. 1. 4to. 1881.
Memorie della Classe di Scienze Morale, Storiche e Filologiche, Vol. VI. 4to. 1881.
Agricultural Society of England, Royal—Journal. Second Series, Vol. XVII. Part 2. 8vo. 1881.
Asiatic Society of Bengal—Journal. Part II. No. 3. 8vo. 1881.
Astronomical Society, Royal—Monthly Notices, Vol. XLI. No. 9. 8vo. 1881.
Ateneo Veneto—Rivista Mensuale di Scienze, Lettere ed Arti. Nos. 2, 3, 4. Serie IV. 8vo. 1881.
Ball, Robert S. Esq. LL.D. F.R.S. (the Author)—Extension of the Theory of Screws to the Dynamics of any Material System. (Trans. Royal Irish Academy, Vol. XXVIII.) 4to. 1881.
Bankers' Institute—Journal, Vol. II. Parts 8, 9, 10. 8vo. 1881.
Bavarian Academy of Sciences, Royal—Sitzungsberichte: 1881, Heft 4. 8vo.
Birmingham Philosophical Society—Proceedings, Vols. I. and II. Parts 1 and 2. 8vo. 1877–81.
British Architects, Royal Institute of—Proceedings, 1881–2, Nos. 3, 4. 4to. 1881–2.
Transactions, Session 1880–1. 4to. 1881.
Chemical Society—Journal for Nov. 1881. 8vo.
Civil Engineers' Institution—Proceedings, 1881–2. 8vo. No. 1.
Clinical Society—Transactions, Vol. XIV. 8vo. 1881.
Editors—American Journal of Science for Nov. 1881. 8vo.
Analyst for Nov. 1881. 8vo.
Athenæum for Nov. 1881. 4to.
Chemical News for Nov. 1881. 4to.
Engineer for Nov. 1881. fol.
Horological Journal for Nov. 1881. 8vo.
Iron for Nov. 1881. 4to.
Nature for Nov. 1881. 4to.
Revue Scientifique and Revue Politique et Littéraire, Nov. 1881. 4to.
Telegraphic Journal for Nov. 1881. 8vo.
Franklin Institute—Journal, No. 671. 8vo. 1881.
Geographical Society, Royal—Proceedings, New Series. Vol. III. No. 12. 8vo. 1881.
Geological Society—Abstracts of Proceedings, 1881–2, No. 1. 8vo.
Irish Academy, Royal—Transactions, Vol. XXVIII. Part 7. 4to. 1881.
Linnean Society—Journal, No. 88. 8vo. 1881.
Manchester Geological Society—Transactions, Vol. XVI. Parts 9, 10. 8vo. 1881.
Mechanical Engineers, Institution—Proceedings, 1881. No. 3. 8vo. 1881.
Medical and Chirurgical Faculty, Maryland—Transactions, 83rd Session. 8vo. 1881.
Meteorological Society—Quarterly Journal, No. 40. 8vo. 1881.
Meteorological Record, No. 2. 8vo. 1881.
Middlesex Hospital—Reports, 1879. 8vo. 1881.

Mines Commission, Accidents in—Preliminary Report, with Evidence and an Index. 4to. 1881.
National Association for Social Science—Sessional Proceedings. Vol. XV. No. 1. 8vo. 1881.
Pharmaceutical Society of Great Britain—Journal, Nov. 1881. 8vo.
Photographic Society—Journal, New Series, Vol. VI. No. 2. 8vo. 1881.
Physical Society of London—Proceedings, Vol. IV. Part 5. 8vo. 1881.
Preussische Akademie der Wissenschaften—Monatsberichte: Juli–Oct. 1881. 8vo.
Quaritch, B. Esq.—A General Catalogue of Books. 8vo. 1880.
Siemens, C. Wm. Esq. D.C.L. LL.D. F.R.S. M.R.I. (the Author)—Science and Industry: An Address to the Birmingham and Midland Institute. (K 104). 8vo. 1881.
Society of Arts—Journal, Nov. 1881. 8vo.
Symons, G. J.—Monthly Meteorological Magazine, Nov. 1881. 8vo.
Telegraph Engineers, Society of—Journal, Part 38. 8vo. 1881.
Tyndall, John, Esq. D.C.L. F.R.S. M.R.I. (the Author)—Floating Matter of the Air. 12mo. 1881.
Verein zur Beförderung des Gewerbfleisses in Preussen—Verhandlungen, 1881, No. 9. 4to.
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